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Management's Incentives to Avoid Negative Earnings Surprises

Dawn A. Matsumoto
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ABSTRACT: Recent reports in the business press allege that managers take actions to avoid negative earnings surprises. I hypothesize that certain firm characteristics are associated with greater incentives to avoid negative surprises. I find that firms with higher transient institutional ownership, greater reliance on implicit claims with their stakeholders, and higher value-relevance of earnings are more likely to meet or exceed expectations at the earnings announcement.

I also examine whether firms manage earnings upward or guide analysts' forecasts downward to avoid missing expectations at the earnings announcement. I examine the relation between firm characteristics and the probability (conditional on meeting analysts' expectations) of having (1) positive abnormal accruals, and (2) forecasts that are lower than expected (using a model of prior earnings changes). Overall, the results suggest that both mechanisms play a role in avoiding negative earnings surprises.

Keywords: *financial analysts; forecast guidance; earnings management; forecast bias; financial reporting incentives.*

Data Availability: *Data are commercially available from the sources identified in the text.*

> After a particularly grim presentation by CEO Bill Gates and sales chief Steve Ballmer at an analysts' meeting two years ago, Goldman Sachs analyst Rick Sherlund ran into the pair outside and said, "Congratulations. You guys scared the hell out of people." Their response? "They gave each other a high five," Sherlund recalls. (Fox 1997)

I thank my dissertation chairman, Robert Bowen, for his continuous guidance in the development of this paper. I also thank the other members of my dissertation committee—Dave Burgstahler, James Jiambalvo, and John Miyamoto—as well as Brian Bushee, Paul Healy, Amy Hutton, Jane Kennedy, Chris Noe, and Greg Miller for their comments and suggestions. An earlier version of this paper benefited from the comments of workshop participants at the University of California, Berkeley; University of California, Los Angeles; University of Colorado at Boulder; Columbia University; Cornell University; Duke University; Harvard University; University of Michigan; New York University; The University of North Carolina at Chapel Hill; Northwestern University; University of Pennsylvania; University of Pittsburgh; Santa Clara University; Stanford University; University of Washington; and Washington University.

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I. INTRODUCTION

Recent articles in the business press suggest that managers of some firms place great importance on meeting or exceeding analysts' expectations, which they achieve either by using their discretion over reported earnings to meet expectations (earnings management) or by guiding analysts' earnings forecasts downward to improve their firms' chances of meeting or beating the forecast when earnings are announced (forecast guidance). Although the academic literature recognizes earnings management more commonly than forecast guidance as a mechanism for achieving specific reporting goals, the business press often refers to forecast guidance:

- A growth stock strategist states, "[m]anagement tries to be conservative in its earnings guidance because of how severely stocks are punished when earnings disappoint" (Ip 1997b).
- The head of quantitative research at Merrill Lynch observes, "Investor-relations people have been making sure the hurdle of expectations remains low so their companies can clear it easily" (McGee 1997).
- "As is the custom late in a quarter, companies have been jawboning analysts' estimates down to be sure the companies at least meet or exceed the consensus figure" (Bleakley 1997).
- "Companies increasingly are talking down their profit prospects to Wall Street analysts, thereby lowering expectations" (Vickers 1999).

Recent academic studies find evidence consistent with managers taking actions to avoid negative "bad news" earnings surprises (Payne and Robb 2000; Brown 2001; Burgstahler and Eames 2001). Moreover, Skinner and Sloan (2001) find that the stock market reaction to negative earnings surprises tends to be large and asymmetric, particularly for growth stocks, suggesting a high cost to missing analysts' expectations. These findings, as well as the anecdotes from the business press, suggest that managers perceive benefits from managing earnings or guiding forecasts to avoid negative earning surprises. In the case of earnings management, managers must implicitly believe that users either are unable to detect earnings manipulations or do not find it cost effective to do so. In the case of guiding forecasts, managers must believe that negative surprises at the earnings announcement are more costly than an initially lower forecast. Whether managers' perceptions are, in fact, true, the prior findings and anecdotal reports raise the following question: What factors influence managers to take actions to avoid negative earnings surprises? This study explores these incentives and the mechanisms, earnings management, and forecast guidance, through which managers achieve this goal.

I test a number of hypotheses about managers' incentives to avoid negative earnings surprises by examining the association between proxies for these incentives and the probability that the firm meets or beats analysts' forecasts at the earnings announcement. As predicted, the results suggest that firms with the following characteristics are more likely to meet or exceed analysts' expectations: (1) higher transient institutional ownership; (2) greater reliance on implicit claims with their stakeholders; and (3) greater value-relevance of earnings. These associations exist even after controlling for other factors associated with the probability of meeting or exceeding analysts' expectations, including the seasonal change in earnings, the growth in industrial production, firm size, and the magnitude of the initial forecast error.

I also examine the mechanisms managers use to avoid negative surprises. I measure abnormal accruals using the modified Jones model (Dechow et al. 1995). I also develop a measure of forecast guidance by modeling an expected forecast based on prior earnings changes and cumulative returns during the year, and then comparing this expected forecast to the consensus analyst forecast. If firms use their influence to keep expectations low, then I expect the published consensus forecast to be lower than the forecast produced by the model. I then examine the relation between firm characteristics and the probability (conditional on meeting analysts' expectations) of (1) managing earnings upward and (2) guiding forecasts downward. The results suggest that firms with higher transient institutional ownership are more likely both to manage earnings upward and to guide forecasts downward, and that firms with consistent patterns of prior losses appear less likely to engage in either behavior. Firms with a greater reliance on implicit claims with their stakeholders, and those in industries in which earnings are highly value-relevant, exhibit evidence of guiding forecasts downward but not of managing earnings upward, whereas high-growth firms appear to manage earnings upward but not guide forecasts downward. Overall, the results suggest that both mechanisms play a role in avoiding negative earnings surprises.

This paper contributes to the literature in a number of ways. First, this study is among the first to recognize that managers may guide forecasts downward to avoid reporting negative earnings surprises. Prior studies have examined the effect of management's *public* forecasts on analysts' forecast revisions (Baginski and Hassell 1990; Williams 1996; Hansen and Noe 1999). However, using only management's public disclosures to investigate downward guidance does not capture the effects on analysts' forecasts of any guidance provided in informal, private conversations.[1] This paper examines forecast guidance by comparing actual forecasts to a measure of expected forecasts in an attempt to better capture any informal guidance managers provide to analysts. The perception that managers often provide informal guidance was at least partially responsible for the SEC's recent passage of Regulation Fair Disclosure (Reg FD). If firms rely on guidance to avoid negative earnings surprises, then the new disclosure rule may diminish a firm's ability to avoid such surprises. The results of this study (conducted prior to the passage of Reg FD) provide insights into the types of firms most likely to be hurt by any inability to provide guidance in this new regulatory environment.

Second, this paper provides further evidence on cross-sectional differences in incentives to avoid negative earnings surprises. Evidence on the incentives for managers to bias either reported earnings or analysts' forecasts (or both) could have implications for studies that test market reactions to earnings announcements. For example, if the market adjusts for perceived downward guidance in analysts' forecasts when forming its expectations, then associations between market reactions and forecast errors based on *published* forecasts may be diminished.[2]

Finally, this study contributes to the literature that investigates bias in analysts' forecasts. Earlier studies, which generally found an optimistic bias in analysts' forecasts, focused on incentives for analysts to bias their forecasts (Francis and Philbrick 1993; Lin and

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[1] Ajinkya and Gift (1984), King et al. (1990), and Bamber and Cheon (1998) all recognize "private disclosure through analysts" as an alternative venue for forecast disclosure, but do not directly investigate this venue. Bamber and Cheon (1998) investigate analyst meetings as a disclosure venue; such meetings, although still a public disclosure mechanism, come closer to the analyst guidance investigated in this paper. The authors' purpose, however, was not to investigate the effect of these disclosures on analysts' forecast errors.

[2] The recent advent of "whisper forecasts" (unofficial forecasts passed among traders or on the Internet before earnings announcements) appears to be at least partially attributable to downward guidance in analysts' published forecasts (Bagnoli et al. 1999).

McNichols 1998).[3] In contrast to these studies, which implicitly assumed that managers prefer optimistic forecasts, this study directly explores the role managers' incentives play in guiding analysts' forecasts, and provides evidence consistent with anecdotal reports suggesting that some managers prefer lower forecasts to avoid negative surprises when the firm announces earnings.

In the next section, I discuss the related literature and provide descriptive evidence on the incentive to avoid negative earnings surprises. Section III explores possible incentives for managers to take actions to avoid negative surprises. Section IV describes the research design and provides descriptive statistics. Evidence of an association between the hypothesized incentives and avoiding negative surprises appears in Section V. In Section VI, I describe the approach employed to distinguish earnings management from forecast guidance and present evidence on the extent to which each mechanism is associated with the hypothesized incentives. Concluding remarks and suggestions for future research appear in Section VII.

II. BACKGROUND

Prior and Concurrent Research

Managers have strong incentives to avoid negative earnings surprises because such surprises generally lead to negative price revisions (Brown et al. 1987) and overall negative publicity for the firm. In addition, a recent study by Skinner and Sloan (2001) suggests that the market response to earnings surprises is asymmetric—the absolute magnitude of the price response to negative surprises significantly exceeds the price response to positive surprises, particularly for high-growth firms.

Evidence from recent empirical work is consistent with managers taking actions to avoid these negative earnings surprises. Burgstahler and Eames (2001) find in the distribution of analysts' forecast errors a larger-than-expected proportion (assuming a smooth distribution) of zero and small positive forecast errors. Brown's (2001) evidence of an overall increase in the percent of zero and positive forecast errors over time is consistent with managers taking actions to avoid negative earnings surprises (assuming the incentive to avoid negative earning surprises has increased over time). Richardson et al. (1999) also provide evidence of a temporal decline in the extent to which actual earnings fall short of analysts' expectations (i.e., analyst optimism).

Managers have two mechanisms for avoiding negative earnings surprises—they can manage earnings upward if unmanaged earnings fall short of expectations or they can guide analysts' expectations downward to avoid overly optimistic forecasts. Both mechanisms entail costs. Managing earnings is difficult because auditors and boards of directors scrutinize questionable accounting practices. Moreover, because accruals reverse in subsequent periods, managers are unlikely to be able to use abnormal accruals to continually increase earnings above expectations every period. Guiding analysts' forecasts downward requires revising current expectations downward if initial forecasts are too high, which could cause a negative stock price reaction at the forecast revision date. Guiding expectations early on to keep them at a "beatable" level is also costly to the extent that it leads to lower stock prices for an extended period of time. For either mechanism to be beneficial, the cost of a negative earnings surprise (e.g., negative stock price reactions or negative publicity for the firm at the earnings announcement date) must exceed the cost of managing earnings (e.g., reputation costs) or the cost of an initially lower forecast (e.g., lower initial stock prices).[4]

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[3] Fried and Givoly (1982), O'Brien (1988), Klein (1990), and Abarbanell (1991) also find evidence of an optimistic bias in analysts' forecasts.

Evidence from prior and concurrent studies is consistent with managers managing earnings to avoid missing analysts' expectations. Payne and Robb (2000) find that firms with premanaged earnings below analysts' expectations have greater positive abnormal accruals. Burgstahler and Eames (2001) find that firm-years in the zero-forecast-error category exhibit more positive earnings management than firm-years in the adjacent categories. Both studies conclude that managers manipulate accruals to avoid negative earnings surprises, but neither explores cross-sectional differences in managers' incentives to avoid negative surprises.

Although a number of studies explore managers' incentives to manage reported earnings to meet various reporting goals, few studies have considered the incentives for managers to *lower expectations* to avoid negative earnings surprises. In their sample of firms that preannounce earnings, Soffer et al. (2000) find that the market considers the majority of preannouncements bad news, but the majority of earnings announcements positive or neutral—suggesting that some firms preannounce bad news, but then reveal positive surprises at the earnings announcement. However, using only the firm's public disclosures to investigate managers' guidance of analysts does not capture the effects of any guidance managers provide privately to analysts. Burgstahler and Eames (2001) find the revision of forecasts from the beginning to the end of the year is more negative for firm-years in the zero-forecast-error category than in adjacent categories, suggesting that managers guide forecasts downward when doing so will allow them to meet analysts' exact expectations. While the evidence from these studies is consistent with downward forecast guidance, neither study explores cross-sectional differences in the propensity to engage in guidance.

This paper contributes to this emerging literature by exploring cross-sectional differences in the propensity to avoid negative earnings surprises. In addition, I investigate the cross-sectional differences in two mechanisms managers likely use to avoid earnings disappointments: earnings management and forecast guidance. Finally, I employ a new technique to investigate forecast guidance.

Descriptive Evidence of the Incentive to Avoid Negative Surprises

The majority of financial press accounts of managers' downward guidance have appeared in the last few years; therefore, the proportion of quarters with positive earnings surprises is likely to have increased in recent years. Using Zacks earnings surprise file, I compute the percentage of firm-quarters where actual reported earnings (E^{REP}) meets or exceeds the consensus forecast outstanding at the earnings announcement (F^{EA}) for each year from 1985 to 1997.[5] Table 1 reports the results of this analysis. Similar to the findings in Brown (2001) and Richardson et al. (1999), both of which are based on data from

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[4] Prior research is consistent with negative earnings surprises being more costly than negative forecast revisions. Skinner and Sloan (2001) show that the magnitude of the market response to earnings surprises is greater for negative surprises than for positive ones. On the contrary, no such evidence of an asymmetry exists for forecast revisions: Stickel (1991) finds no difference in the absolute magnitude of the returns following forecast revisions in the top 5 percent vs. the bottom 5 percent of revisions. However, given the different sample periods these studies used, further work is necessary before any conclusions are possible.

[5] I use the Zacks-provided consensus because the forecast and actual earnings are provided to three decimal places (as opposed to the two decimal places used for the individual analyst estimates), thereby reducing potential measurement error for firms with multiple stock splits. I also eliminate forecast errors if both the forecast and actual earnings equals zero in the database (less than 1 percent of the sample). I assess the extent to which stale forecasts affect the consensus by comparing the absolute forecast error derived from an independently constructed consensus (based on forecasts made within the last 90 days) to the Zacks-provided consensus. The Zacks-provided consensus has a smaller mean absolute forecast error, suggesting staleness is not a significant problem.

TABLE 1
Trend over Time in the Probability of Meeting or Exceeding Analysts' Expectations vs. the Seasonal Change in Earnings

Year	*% Meeting Expectations*[a]	*% Positive Changes in Earnings*[b]
1985	41.0	53.8
1986	44.2	59.9
1987	47.5	69.5
1988	50.9	71.9
1989	44.5	60.9
1990	42.6	56.7
1991	49.0	52.4
1992	54.8	64.1
1993	55.5	63.9
1994	62.0	68.5
1995	61.8	65.0
1996	66.7	62.7
1997	70.1	63.7
Spearman Rank correlation[c]	0.907	0.197
p-value	0.001	0.668

[a] Percent of firm-quarters in each year in which actual earnings met or exceeded the analyst consensus forecast outstanding at the earnings announcement (as reported in Zacks surprise file).

[b] Percent of firm-quarters in each year in which earnings (quarterly Compustat item no. 8) exceeded the earnings in the same quarter one year prior.

[c] Rank correlations are between the year and (1) percent of firm-quarters meeting or exceeding expectations and (2) the percent of firm-quarters with a positive seasonal change in earnings.

I/B/E/S, the percentage of quarters in which earnings met or exceeded analysts' expectations has increased over time (Spearman rank correlation = 0.90, $p < 0.001$), consistent with growing emphasis on avoiding negative earnings surprises.[6]

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[6] A recent working paper by Abarbanell and Lehavy (2000) suggests that the trend in the tendency to meet or beat analysts' forecasts may be database-driven and not a true change in behavior. However, the results reported here are similar to the results reported in Brown (2001), who uses I/B/E/S data. In addition, the results are inferentially similar ($r = 0.90$, $p < 0.001$), if I compute forecast errors based on Compustat-reported earnings per share before extraordinary items (quarterly data item no. 19). I use the Zacks-reported earnings per share (rather than the Compustat earnings per share) in computing forecast errors because analysts often forecast a number different from bottom-line earnings (generally earnings before nonrecurring/special items). If the benchmark managers are concerned about analysts' forecasts, then it seems appropriate to judge the success of their actions using the same definition of earnings that the analysts are forecasting.

The increase is unlikely to be due to analysts underestimating the effect of positive macroeconomic events on firm profits. Table 1 shows that there is no significant trend over time in the percent of quarters with increases in earnings per share before extraordinary items (quarterly Compustat data item no. 8) from the same quarter in the previous year (i.e., the seasonal change in earnings). Thus, the tendency to meet or exceed analysts' consensus forecasts does not appear to result from a general increase in earnings performance that analysts failed to anticipate. Figure 1 plots the trend in these two percentages, which appear to move together in the first half of the sample period. From approximately 1991 onward, however, the percent of quarters that meet or exceed analysts' expectations has increased steadily, whereas the percent of quarters with increases in earnings has not.

The foregoing evidence is consistent with anecdotal reports suggesting managers take actions to avoid negative earnings surprises, particularly in recent years. In the next section I discuss firm characteristics that are likely associated with the incentive to meet or exceed analysts' forecasts.

III. HYPOTHESIS DEVELOPMENT

Institutional Ownership

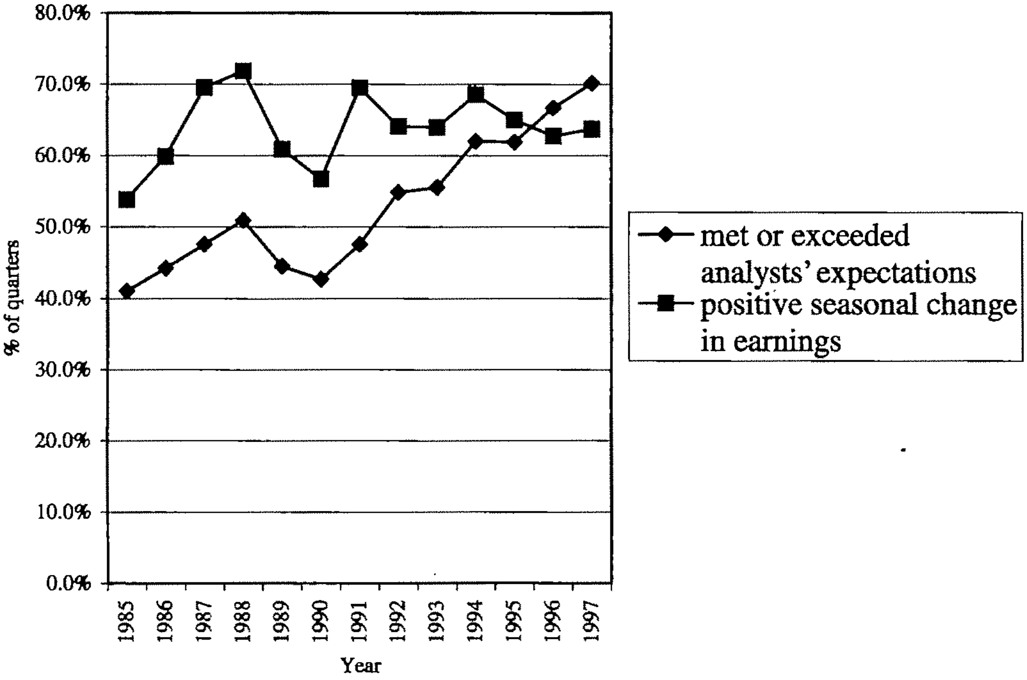
A manager is likely concerned that a negative earnings surprise will lead to significantly lower stock prices and adversely affect his or her performance evaluation. Puffer and Weintrop (1991), for example, find that the probability of CEO turnover increases with the shortfall of actual earnings from analysts' expectations. To the extent institutional investors overemphasize near-term profits (Porter 1992; *Business Week* 1987), managers of firms with higher institutional ownership likely perceive greater costs to missing analysts' forecasts. Frequently cited reasons for institutional investors' focus on current earnings include pressure for near-term portfolio performance; difficulty in analyzing all the stocks in a highly diversified portfolio; and the need for a defensible measure for trading due to their fiduciary responsibilities (Eames 1995). Because earnings surprises are readily available from a number of sources (e.g., First Call, Zacks, I/B/E/S), they are a simple, defensible heuristic on which to base trades, and institutional investors likely react strongly to negative earnings surprises. Lang and McNichols (1997) present evidence consistent with institutional investors trading based on earnings surprises. Moreover, because the business press focuses more heavily on earnings surprises than on intervening forecast revisions, the former is more likely than the latter to improve the appearance of portfolios at quarter-end, giving managers of firms with greater institutional ownership an incentive to guide forecasts downward to avoid negative earnings surprises. The empirical, survey, and anecdotal evidence suggests that managers of firms with higher institutional ownership have greater incentives both to manage earnings and to guide forecasts, leading to the following hypothesis:

> **H1:** Firms with higher institutional ownership are more likely to take actions to avoid negative earnings surprises.

To test this hypothesis, I use the percent of total shares owned by institutions (%INST) as reported in the Spectrum database, averaged over the four preceding quarters. Table 2 summarizes the study's hypotheses and proxy variables.

Alternatively, one might argue that institutional investors are more sophisticated, and are therefore more likely to see through accounting manipulations and managerial forecast guidance. However, Bushee (1998) finds that a high proportion of ownership by institutions exhibiting "transient" behavior (i.e., institutions with high portfolio turnover that engage in momentum-trading strategies) increases the probability that managers cut research and

FIGURE 1
Trend over Time in the Probability of Meeting or Exceeding Analysts' Expectations vs. the Seasonal Change in Earnings[a]



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[a] This figure plots the percent of firm-quarters in each year for which (1) actual earnings meet or exceed the analyst consensus forecast outstanding at the earnings announcement (as reported in Zacks surprise file), and (2) actual earnings exceed earnings in the same quarter one year prior (i.e., positive seasonal change in earnings).

development costs to boost earnings. Because incentives to avoid negative earnings surprises may differ depending on the *type* of institutional ownership, I also report results after partitioning the percent of institutional ownership into the percent held by "transient" (%TRAN) and "nontransient" institutions (%NONTRAN).[7]

Reliance on Implicit Claims with Stakeholders

A firm's other stakeholders—customers, employees, suppliers, and so forth—are also consumers of its financial information. Bowen et al. (1995) (hereafter BDS) argue that a firm's financial image influences stakeholders' assessments of its ability to fulfill its implied commitments, leading to more favorable terms of trade with these stakeholders. BDS conclude that managers' incentives to choose income-increasing accounting methods (to enhance the firm's financial image) is strongest for firms that rely heavily on implicit claims with their stakeholders.

In judging a firm's financial performance, these stakeholders are likely to react more strongly to earnings announcements than to analysts' initial forecasts because they either

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[7] I thank Brian Bushee for providing his classification scheme to conduct this analysis.

TABLE 2
Incentives for Avoiding Negative Earnings Surprises and Control Variables

	Construct	*Proxy*	*Variable Name*	*Expected Relation with MEET*[a]
	Incentive variables:			
H1	Institutional ownership	Percent of shares held by institutions (reported on Spectrum)	%INST	Positive
		Percent of shares held by transient institutions (as defined in Bushee [1998])	%TRAN	Positive
		Percent of shares held by nontransient (quasi-indexers and dedicated) institutions (as defined in Bushee [1998])	%NONTRAN	Positive
H2	Reliance on implicit claims	Dummy variable indicating membership in durable goods industry (SICs 150–179, 245, 250–259, 283, 301, 324–399)	DUR	Positive
		R&D ÷ Assets (Compustat item #4 ÷ item #44)	R&D	Positive
		Measure of labor intensity (1 − (PPE ÷ Gross Assets)): 1 − (Compustat item #118 ÷ (item #44 + item #41))	LABOR	Positive
		Factor scores from factor analysis	ICLAIM	Positive
H3	Value-relevance of earnings	Dummy variable indicating losses (Quarterly Compustat item #8 less than zero) in each of the four most recent quarters (i.e., quarters t–1 to t–5)	LOSS	Negative
		Decile rank of R^2 from industry-specific yearly regressions of cumulative excess returns on seasonal change in earnings	EARNRET	Positive
H4	Growth prospects	Consensus long-term growth forecast at the beginning of the year (reported in Zacks consensus estimate history)	LTG	Positive
H5	Litigation risk	Dummy variable indicating membership in high-risk industry (SICs 2833–2836, 3570–3577, 7370–7374, 3600–3674, 5200–5961)	LIT	Positive

(*Continued on next page*)

TABLE 2 (Continued)

Construct	*Proxy*	*Variable Name*	*Expected Relation with MEET*[a]
Control Variables:			
Change in earnings	Dummy variable indicating positive change in earnings from the same quarter in the prior year (Compustat item #8)	POSUE	Positive
Macroeconomic growth	Average annual growth in industrial production over prior 12 months	INDPROD	Positive
Firm size	Log of market value of equity (Compustat item #61 × item #14)	LOGMV	Positive
Uncertainty in forecasting environment	Absolute value of the difference between reported earnings and the initial consensus forecast (measured as the first forecast each analyst made after the prior year's earnings announcement), deflated by price at the end of the prior year (Quarterly Compustat item #14)	|FE|	Negative

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[a] MEET is the probability that actual earnings meet or exceed the analyst consensus forecast outstanding at the earnings announcement date (as reported in Zacks surprise file).

have limited ability or do not find it cost effective to fully process all information about the firm. Moreover, the financial press discusses earnings announcements more prominently than initial forecasts, making the comparison of actual earnings to analysts' forecasts the more salient information on which to judge firm performance. As a result, avoiding negative surprises at the earnings announcement by either managing earnings or guiding forecasts likely yields more optimal terms of trade with stakeholders. Thus, I expect that:

H2: Firms with greater reliance on implicit claims with stakeholders are more likely to take actions to avoid negative earnings surprises.

I test this hypothesis using the three proxy variables from BDS with the highest explanatory power in their 1993 sample period:[8]

- Membership in a durable goods industry (DUR), using the same SIC codes BDS used: 150–179, 245, 250–259, 283, 301, and 324–399,
- Research and Development expenditures (R&D, quarterly Compustat item no. 4) scaled by total assets (quarterly Compustat item no. 44),[9] and
- A measure of labor intensity (LABOR) equal to 1 minus the ratio of gross property, plant, and equipment (quarterly Compustat item no. 118) to firm size (measured as total gross assets, quarterly Compustat item no. 44 + item no. 41).

I presume firms with higher values for each of these variables rely more on implicit claims with their stakeholders; as a result, I expect all three variables to be positively related to the tendency to avoid negative earnings surprises. As with %INST, I average the value of these variables over the four preceding quarters.

Value-Relevance of Earnings

A firm's economic circumstances are also likely to influence its managers' perceptions of the benefits of avoiding negative surprises at the earnings announcement. For example, if the value-relevance of a firm's earnings is low (i.e., earnings are a poor indicator of future cash flows and firm value), then shareholders likely react less strongly to negative earnings surprises; hence, managers of such firms are likely to be less concerned about failing to meet analysts' expectations:

H3: Firms with low value-relevance of earnings are less likely to take actions to avoid negative earnings surprises.

To test this hypothesis I use two measures of value-relevance. The first is a dummy variable indicating firms that report losses before extraordinary items (quarterly Compustat item no. 8) in *each* of the last four quarters, $t - 1$ to $t - 5$ (LOSS). Prior studies demonstrate a lower association between returns and earnings (Hayn 1995) for loss firms. Prior research also suggests that meeting or beating analysts' expectations is less important for firms that incur losses (Dégeorge et al. 1999) and that analysts' forecasts are more optimistic for loss firms (Brown 2001). In contrast to these prior studies, I suggest that an underlying firm characteristic (low value-relevance of earnings) explains the association between losses and the tendency to avoid negative surprises. Consequently, I do not measure the existence of a loss *in the same quarter* as the forecast error, but rather use a lagged measure of *consistent* losses. I expect a negative relation between LOSS and the tendency to avoid negative earnings surprises.

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[8] The level of implicit claims with stakeholders is inherently unobservable and any proxies are subject to alternative interpretations. For example, because high-technology firms often invest heavily in R&D and also frequently use employee stock options to compensate employees, it is possible that R&D expenditures are related to the use of stock options. However, including a variable to capture the extent of stock option use (Compustat item no. 215: Common shares reserved for conversion – stock options) does not affect the results and the coefficient on this variable is not significant.

[9] Because firms do not always report R&D expenditures on a quarterly basis, I use the annual Compustat tapes to fill in quarters with missing observations. Specifically, if any quarters in a given fiscal year have missing values indicating that the firm reported the values on an annual basis only, then I include one-fourth of the annual value reported on the annual Compustat tapes as the quarterly amount of R&D. In addition, following BDS, I set R&D to zero if the variable is not reported.

The second measure of value-relevance of earnings, an industry-specific R^2 from a regression of 12-month returns on earnings, captures industry-specific differences in the value-relevance of earnings. Prior studies suggest the value-relevance of earnings may be lower in certain industries, particularly high-technology industries (Amir and Lev 1996; Franzen 2000). To compute this measure, I regress excess daily returns (cumulated from three days after the quarter $t - 4$ earnings announcement to three days after the quarter t earnings announcement) on the change in earnings per share from quarter $t - 4$ to quarter t, scaled by price per share at the end of quarter $t - 4$. I run regressions by year for each four-digit SIC code. Given the potential for measurement error with this variable, I use the yearly decile rank of the industry's R^2 measure as the proxy for value relevance (EARNRET). I expect a positive relation between EARNRET and the tendency to avoid negative earnings surprises.

Long-Term Growth Prospects

Managers of high-growth firms likely have greater incentives to avoid missing earnings expectations. Collins and Kothari (1989) show the market reaction to earnings announcements is greater for firms with high-growth opportunities. In addition, Skinner and Sloan (2001) find that the asymmetry in the market response to positive vs. negative earnings surprises is stronger for high-growth than for low-growth firms. Thus, managers of high-growth firms are likely to have strong incentives to avoid negative surprises. Moreover, a firm with high long-term growth forecasts may be able to maintain a "buy" recommendation from analysts despite a lower initial short-horizon forecast because analysts' stock recommendations are more closely related to the firm's long-term growth, rather than to its short-horizon forecasts (Bandyopadhyay et al. 1995). Therefore, keeping expectations low to avoid negative earnings surprises is likely to be less costly for high-growth firms:

> **H4:** Firms with high-growth prospects are more likely to take actions to avoid negative earnings surprises.

To test this hypothesis, I use the consensus long-term growth forecast outstanding at the end of quarter $t - 1$ as reported in Zacks consensus estimate history (LTG).

Litigation Risk

A sudden price drop can also precipitate shareholder litigation (Tucker 1991; Alexander 1991); therefore, managers of firms with higher risk of shareholder lawsuits likely have greater incentive to avoid negative earnings surprises. In addition, because liability hinges on managers misleading the market (e.g., through unduly optimistic disclosures or failure to disclose promptly material adverse information), keeping analysts' expectations low diminishes the merits of the plaintiffs' case should a suit be filed. Skinner (1994, 1997) argues similarly that the incentive to preempt via voluntary disclosure is greater for adverse than for favorable earnings news. This discussion suggests the following hypothesis:

H5: Firms with higher *ex ante* litigation risk are more likely to take actions to avoid negative earnings surprises.

Consistent with prior research (Francis et al. 1994; Soffer et al. 2000; Ali and Kallapur 2001), I use an industry dummy variable to identify firms in high-risk industries (LIT): SIC codes 2833–2836 (biotechnology), 3570–3577 and 7370–7374 (computers), 3600–3674 (electronics), and 5200–5961 (retailing). I use an industry indicator rather than measures such as price volatility to obtain a measure of *ex ante* litigation risk that is unaffected by managers' prior efforts to avoid negative surprises (price volatility is likely affected by these actions).

IV. RESEARCH DESIGN AND DESCRIPTIVE STATISTICS

I classify firm-quarter observations as either meeting or exceeding expectations (MEET = 1) if reported earnings meet or exceed the forecast outstanding at the earnings announcement ($E^{REP} \geq F^{EA}$) or as not meeting expectations (MEET = 0) if reported earnings fall short of expectations ($E^{REP} < F^{EA}$). Because the results in Table 1 suggest an increase in the tendency to avoid negative surprises in recent years, my tests concentrate on the later years in the sample, specifically, 1993 to 1997. In addition, firms in regulated industries likely have different incentives than those in nonregulated industries; therefore, I exclude financial institutions (SIC codes 6000–6999), utilities (SIC codes 4800–4999), and other quasi-regulated industries (SIC codes 4000–4499, and 8000 and higher).[10] The final sample consists of firm-quarters from nonregulated industries with the data required to measure the proxy and control variables. As outlined in Table 2, the variables require data from the Zacks, Compustat, Spectrum, and CRSP databases.

I measure most of the independent variables with a lag (i.e., prior to quarter t) to strengthen the argument over the direction of causality. Also, any contemporaneously measured proxy variable that includes a component of firm performance will likely be correlated with the probability of a positive earnings surprise if there is an unexpected shock to earnings (i.e., extreme good or bad performance is more likely to surprise analysts). This correlation would exist regardless of any management actions to avoid negative surprises and would therefore bias my results.

Control Variables

I also include four control variables, two to control for unexpected contemporaneous shocks to earnings, one to control for firm size, and one to control for uncertainty in the forecasting environment. Unexpected macroeconomic shocks affect earnings surprises (O'Brien 1988, 1994); I control for these shocks by including both a firm-specific variable and a macroeconomic variable. A dummy variable (POSUE) indicates firm-quarters with a positive seasonal change in earnings before extraordinary items (quarterly Compustat item no. 8). This variable controls for the relation between the change in earnings and the forecast error. Because O'Brien (1994) finds a positive relation between growth in industrial production and analysts' forecast errors, I also control for the average annual growth in industrial production calculated over the 12 months ending at quarter t (INDPROD).

Prior research also suggests that larger firms have less optimistic biases in analysts' forecasts (Brous and Kini 1993; Brown 1997; Das et al. 1998). Thus, I control for size using the log of the market value of equity (LOGMV, quarterly Compustat item no. 61 × item no. 14).

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[10] The time-trend analysis presented in Table 1 is inferentially similar using a sample that excludes these regulated industries.

Finally, I control for uncertainty in the forecasting environment because it is likely more difficult for managers to guide analysts' forecasts when uncertainty is high. I include the absolute value of the initial forecast error, deflated by price at the end of the prior year ($|FE| = abs(E^{REP} - F^{INI}) \div$ quarterly Compustat item no. 14) as a measure of forecasting uncertainty. I compute the initial forecast (F^{INI}) using the first forecast each analyst made within 90 days after the earnings announcement from the same quarter of the previous year (i.e., approximately nine months before the end of the quarter) using Zacks individual estimate history file.

Descriptive Statistics

Descriptive statistics on the dependent, incentive, and control variables appear in Table 3. The average for the dependent variable (MEET) indicates that firms met or exceeded analysts' expectations in approximately 65 percent of the firm-quarters in the sample. Approximately 45 percent of the firm-quarters in the sample are from firms in durable goods industries and 36 percent from firms in high-litigation-risk industries. Only a small portion of the firm-quarters in the sample (5 percent) is from firms with a consistent history of

TABLE 3

Descriptive Statistics for Dependent Variable, Proxies for Incentives to Avoid Negative Earnings Surprises, and Control Variables

Variable[a]	*n*	*Mean*	*Standard Deviation*	*1st Quartile*	*Median*	*3rd Quartile*
Dependent Variable:						
MEET	29,460	0.652	0.476	0.000	1.000	1.000
Proxies for Incentives to Avoid Negative Earnings Surprises:						
%INST	28,914	0.448	0.216	0.289	0.464	0.617
%TRAN	28,914	0.091	0.080	0.032	0.071	0.128
%NONTRAN	28,914	0.356	0.178	0.222	0.367	0.492
DUR	29,460	0.450	0.498	0.000	0.000	1.000
R&D	29,460	0.013	0.024	0.000	0.002	0.017
LABOR	29,460	0.663	0.245	0.491	0.708	0.877
ICLAIM	29,460	0.000	1.000	−0.711	−0.128	0.697
LOSS	29,460	0.053	0.225	0.000	0.000	0.000
EARNRET	27,703	4.185	2.577	2.000	4.000	6.000
LTG	24,970	18.305	8.463	12.333	16.116	22.429
LIT	29,460	0.365	0.481	0.000	0.000	1.000
Control Variables:						
POSUE	29,127	0.656	0.475	0.000	1.000	1.000
INDPROD	29,460	0.049	0.012	0.039	0.050	0.058
MV	29,403	2,280.833	7,483.006	150.445	428.805	1,457.422
|FE|	29,067	0.009	0.016	0.001	0.004	0.010

[a] MEET is a dichotomous variable equal to 1 if a firm's actual earnings meets or exceeds the analyst consensus forecast outstanding at the earnings announcement (as reported in Zacks surprise file), 0 otherwise. Table 2 defines the incentive and control variables. For descriptive purposes I present statistics on the actual, rather than the log, market value of equity (MV). For the remaining tables, I use the log of market value of equity (LOGMV).

prior losses. In addition, the sample firms are relatively large (mean market value of equity of \$2.28 billion) with a somewhat higher percent of institutional ownership (45 percent) than reported in other studies (e.g., approximately 39 percent in Jiambalvo et al. [2001] and 36 percent in Bushee [2001]). This difference is likely due to an increase in institutional ownership over time (Gompers and Metrick 2001) as well as data requirements that bias the sample toward relatively larger firms. Finally, 66 percent of firm-quarters in the sample have a positive change in earnings relative to the same quarter of the prior year (POSUE), similar to the percent meeting or exceeding analysts' expectations.

Table 4 presents a correlation matrix of all variables, with Pearson (Spearman) correlations below (above) the diagonal. The correlations between the dependent variable (MEET) and each of the incentive variables, presented in the first column/row, are all statistically significant in the predicted directions: %INST, DUR, R&D, LABOR, EARNRET, LTG, and LIT are all positively related to MEET, and LOSS is negatively related to MEET.[11] Partitioning %INST into the percent held by transient (%TRAN) and nontransient institutions (%NONTRAN), I find both types of institutions are associated with a higher probability of meeting analysts' expectations, although the correlation is stronger for transient institutions. While the correlations are all statistically significant, the magnitudes of many of the correlations are not particularly large. In addition, since all the control variables are significantly correlated with MEET (particularly POSUE and |FE|) it is important to control for these factors in tests of the incentive variables.

Among the incentive and control variables, many of the correlations are significant and several are above 0.3. Not surprisingly, the implicit claims variables DUR, RD, and LABOR are all positively correlated with one another. These variables are meant to proxy for essentially the same construct; therefore, I use factor analysis to reduce them to a single construct (ICLAIM) for the logit analysis conducted in the next subsection.[12,13]

Logit Regressions

I perform logit regressions modeling the probability that a firm meets or exceeds analysts' forecasts at the earnings announcement:

$$\begin{aligned}\text{Prob}(\text{MEET} = 1) = \text{F}(&\beta_0 + \beta_1\%\text{INST}_i + \beta_2\text{ICLAIM}_i + \beta_3\text{LOSS}_i \\ &+ \beta_4\text{EARNRET}_i + \beta_5\text{LTG}_i + \beta_6\text{LIT}_i + \beta_7\text{POSUE}_i \\ &+ \beta_8\text{INDPROD}_i + \beta_9\text{LOGMV}_i + \beta_{10}|\text{FE}_i| + \varepsilon_i) \qquad (1)\end{aligned}$$

where:

$$\text{F}(\beta'\text{X}) = \frac{e^{\beta'\text{X}}}{1 + e^{\beta'\text{X}}}$$

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[11] Results of t-tests of differences in the means between the two groups (MEET = 1 vs. MEET = 0) are inferentially similar. I present the correlation results for simplicity.

[12] I use the principal components method of factor analysis to reduce the three variables to a single construct (use of the common factor method produces essentially identical results). Retaining factors with eigenvalues greater than 1 (Hair et al. 1995) results in the retention of one factor, consistent with the assumption that the three factors proxy for a single construct. The final communality estimates (i.e., the correlation between the common factor and the individual variables) for DUR, RD, and LABOR are 0.560, 0.428, and 0.494, respectively, indicating that the common factor is basically a weighted average of the three variables (standardized).

[13] I conducted collinearity diagnostics on the final model (i.e., the model using ICLAIM in place of DUR, RD, and LABOR), including variance inflation factors and condition indices (see Belsley et al. 1980) using an ordinary least squares (OLS) model. All variance inflation factors are less than 10 and condition indices are less than 30, suggesting collinearity is not a serious problem (Kennedy 1992).

TABLE 4
Correlations among the Dependent Variable, the Proxies for Incentives to Avoid Negative Earnings Surprises, and Control Variables[a]

		INCENTIVE VARIABLES											*CONTROL VARIABLES*			
	MEET	*%INST*	*%TRAN*	*%NONTRAN*	*DUR*	*R&D*	*LABOR*	*ICLAIM*	*LOSS*	*EARNRET*	*LTG*	*LIT*	*POSUE*	*INDPROD*	*LOGMV*	*|FE|*
MEET		0.09**	0.11**	0.05**	0.02**	0.04**	0.02**	0.03**	−0.06**	0.02**	0.06**	0.02**	0.35**	0.09**	0.09**	−0.27**
%INST	0.09**		0.66**	0.93**	−0.02**	−0.02**	−0.12**	−0.09**	−0.17**	0.07**	−0.13**	−0.06**	0.09**	0.00	0.46**	−0.19**
%TRAN	0.12**	0.62**		0.40**	0.03**	0.10**	0.07**	0.09**	−0.08**	0.02**	0.25**	0.11**	0.07**	0.02**	0.33**	−0.15**
%NONTRAN	0.05**	0.93**	0.30**		−0.04**	−0.07**	−0.19**	−0.16**	−0.18**	0.08**	−0.30**	−0.13**	0.08**	−0.01*	0.43**	−0.17**
DUR	0.02**	−0.02**	0.03**	−0.03**		0.49**	0.16**	0.75**	0.10**	0.00	0.07**	0.03**	0.01	−0.00	−0.10**	0.07**
R&D	0.01*	−0.09**	0.09**	−0.15**	0.28**		0.22**	0.72**	0.24**	−0.14**	0.25**	0.30**	−0.04**	0.02**	−0.04**	0.04**
LABOR	0.02**	−0.10**	0.10**	−0.17**	0.19**	0.25**		0.65**	0.09**	−0.05**	0.38**	0.22**	−0.03**	−0.01	−0.21**	0.01
ICLAIM	0.02**	−0.10**	0.10**	−0.17**	0.70**	0.74**	0.66**		0.21**	−0.09**	0.36**	0.29**	−0.03**	0.01*	−0.20**	0.07**
LOSS	−0.06**	−0.17**	−0.07**	−0.18**	0.10**	0.42**	0.09**	0.30**		−0.15**	0.10**	0.12**	−0.10**	0.04**	−0.18**	0.19**
EARNRET	0.02**	0.07**	0.02**	0.08**	0.00	−0.18**	−0.03**	−0.10**	−0.15**		−0.08**	−0.14**	0.04**	−0.04**	−0.01	0.02**
LTG	0.05**	−0.14**	0.28**	−0.29**	0.06**	0.40**	0.34**	0.38**	0.17**	−0.09**		0.36**	0.03**	0.09**	−0.27**	−0.04**
LIT	0.02**	−0.05**	0.13**	−0.13**	0.03**	0.36**	0.22**	0.30**	0.12**	−0.15**	0.35**		−0.03**	0.03**	−0.07**	0.00
POSUE	0.35**	0.09**	0.08**	0.08**	0.01	−0.07**	−0.02**	−0.04**	−0.10**	0.04**	0.01	−0.03**		0.03**	0.10**	−0.39**
INDPROD	0.09**	0.00	0.02**	−0.01	0.00	0.04**	−0.01	0.02**	0.04**	−0.03**	0.09**	0.02**	0.03**		−0.01*	−0.03**
LOGMV	0.09**	0.41**	0.23**	0.39**	−0.09**	−0.14**	−0.19**	−0.20**	−0.17**	−0.02**	−0.25**	−0.06**	0.09**	−0.01		−0.33**
|FE|	−0.22**	−0.17**	−0.13**	−0.15**	0.04**	0.09**	0.01	0.07**	0.19**	0.01	−0.04**	0.03**	−0.26**	−0.03**	−0.26**	

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*, ** Significant at $p < 0.05$ and $p < 0.01$, respectively.

[a] Pearson correlations appear below the diagonal, Spearman correlations above it.

See Table 2 for variable definitions.

I also perform the logit regression replacing %INST with %TRAN and %NONTRAN to investigate whether the *type* of institutional ownership affects the incentive to avoid negative earnings surprises.

V. RESULTS: INCENTIVES FOR AVOIDING NEGATIVE SURPRISES

Results of the logit regressions appear in Table 5. Columns 3 through 5 present the results using %INST in the regression (Model 1), while columns 6 through 8 report results using %TRAN and %NONTRAN in place of %INST (Model 2). In the first model, the coefficient on %INST is positive and significant ($p < 0.001$), suggesting that firms with higher institutional ownership are more likely to meet or exceed expectations (consistent with H1). However, when I use %TRAN and %NONTRAN in place of %INST, only the coefficient on %TRAN is significantly positive, suggesting that transient institutional ownership motivates managers to avoid negative earnings surprises but that nontransient institutional ownership does not.

The coefficient on ICLAIM is significantly positive in both models ($p < 0.001$), consistent with H2's prediction that managers have greater incentives to avoid negative surprises when reliance on implicit claims with stakeholders is greater. Consistent with the conjecture that firms with low value-relevance of earnings have less incentive to avoid negative earnings surprises (H3), firms with consistent prior losses (LOSS) are *less* likely to meet or exceed expectations, and firms in industries in which earnings are more value-relevant (EARNRET) are *more* likely to do so. These results hold for both specifications of the model ($p < 0.005$).

The significantly positive coefficient on LTG in Model 1 supports H4—firms with relatively higher long-term growth prospects appear more likely to avoid negative earnings surprises. However, in the model using %TRAN and %NONTRAN in place of %INST, the coefficient on LTG is not significant.[14] The correlations in Table 4 reveal that LTG is significantly positively associated with %TRAN and significantly negatively associated with %NONTRAN. Thus, in the first model LTG may reflect transient institutional ownership; it may not be the presence of growth prospects that provides the incentive to avoid negative earnings surprises, but rather that firms with higher growth prospects have higher transient institutional ownership. Similar arguments may apply to litigation risk. Whereas Model 1 suggests that firms in highly litigious industries (LIT) are marginally more likely to meet or exceed analysts' expectations (supporting H5), the specification including %TRAN and %NONTRAN does not support this hypothesis.

Columns 5 and 8 report the marginal effects of each variable in the two models. The marginal effects are analogous to the slope coefficients in an OLS regression.[15] The marginal effect for %INST in model 1, 0.087, when multiplied by the interquartile range (0.317 for this sample), suggests that moving from the first to the third quartile of %INST increases the probability of meeting or exceeding analysts' expectations by approximately 3 percent. In the second model, a similar analysis suggests that moving from the first to the third quartile of %TRAN increases the probability of meeting or exceeding analysts' expectations by 5 percent. The change in probabilities for ICLAIM and EARNRET are 3 percent and

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[14] An alternative proxy for growth prospects is the market-to-book ratio. Using the market-to-book ratio (MB) averaged over the prior four quarters (winsorized at the 1st and 99th percentile to reduce the effect of outliers) in place of LTG produces inferentially similar results.

[15] I compute the marginal effects as $e^{\beta'X}/(1 + e^{\beta'X})^2$ where $\beta'X$ is computed at the mean values of the independent variables (Greene 1993). Technically, one should calculate the marginal effect for dichotomous variables as the difference in probability when the variable is equal to 1 vs. 0, evaluated at the mean of the other variables. This procedure produces nearly identical values for the marginal effects as those produced using the above formula.

TABLE 5

Logit Analysis of the Probability of Meeting or Exceeding Analysts' Expectations and the Incentives to Avoid Negative Earnings Surprises[a]

Model 1:

$$Prob(MEET = 1) = F(\beta_0 + \beta_1\%INST_i + \beta_2 ICLAIM_i + \beta_3 LOSS_i + \beta_4 EARNRET_i + \beta_5 LTG_i + \beta_6 LIT_i + \beta_7 POSUE_i + \beta_8 INDPROD_i + \beta_9 LOGMV_i + \beta_{10}|FE_i| + \varepsilon_i)$$

Model 2:

$$Prob(MEET = 1) = F(\gamma_0 + \gamma_1\%TRAN_i + \gamma_2\%NONTRAN_i + \gamma_3 ICLAIM_i + \gamma_4 LOSS_i + \gamma_5 EARNRET_i + \gamma_6 LTG_i + \gamma_7 LIT_i + \gamma_8 POSUE_i + \gamma_9 INDPROD_i + \gamma_{10} LOGMV_i + \gamma_{11}|FE_i| + \varepsilon_i)$$

		Model 1			*Model 2*		
Variable[b]	*Predicted Sign*	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]
Intercept		−1.197	0.001		−0.964	0.001	
Incentives for Avoiding Negative Surprises:							
%INST (H1)	+	0.405	0.001	0.087			
%TRAN	+				2.358	0.001	0.506
%NONTRAN	+				−0.182	0.964	−0.039
ICLAIM (H2)	+	0.010	0.001	0.021	0.091	0.001	0.020
LOSS (H3)	−	−0.302	0.001	−0.065	−0.254	0.003	−0.055
EARNRET (H3)	+	0.016	0.004	0.004	0.016	0.005	0.003
LTG (H4)	+	0.010	0.001	0.002	0.002	0.231	0.000
LIT (H5)	+	0.058	0.048	0.012	0.037	0.146	0.008
Control Variables:							
POSUE	+	1.369	0.001	0.294	1.363	0.001	0.293
INDPROD	+	13.260	0.001	2.848	13.451	0.001	2.888
LOGMV	+	0.030	0.003	0.007	0.023	0.019	0.005
|FE|	−	−27.962	0.001	−6.004	−28.258	0.001	−6.068
Log Likelihood			−12,665.79			−12,625.30	
Chi-square			3,545.24			3,626.21	
p-value			0.001			0.001	
No. of Observations:							
Meet/Exceed			15,235			15,235	
Did not meet			7,520			7,520	
Total			22,755			22,755	

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[a] The dependent variable equals 1 if a firm's actual earnings meets or exceeds the analyst consensus forecast outstanding at the earnings announcement (as reported in Zacks surprise file), 0 otherwise.

[b] See Table 2 for variable definitions.

[c] p-values are one-tailed.

[d] The marginal effects are computed as $e^{\beta'X}/(1 + e^{\beta'X})^2$, where $\beta'X$ is computed at the mean values of X.

1 percent, respectively, in both specifications of the model. Finally, the probability of meeting or exceeding analysts' expectations at the earnings announcement is approximately 6 percent (5 percent) lower in Model 1 (Model 2) for firms with four quarters of prior losses.

These changes in probability may seem small compared to the marginal effects for some of the control variables. For example, the marginal effect for POSUE equals 0.294, indicating that a positive seasonal change in earnings increases the probability of meeting or exceeding analysts' expectations by 29 percent. However, because they use *ex post* information (i.e., reported earnings in quarter t), neither POSUE nor |FE| *predict* positive forecast errors.

The above analysis treats each firm-quarter as a separate observation; however, many of the firm characteristics do not change across quarters. Consequently, I repeated the analysis after aggregating the variables across quarters at the firm level, resulting in one observation per firm. Aggregating the dependent variable, MEET, across quarters yields the *percent* of quarters in which a firm meets expectations. Regressing this variable on the average of the independent variables yields identical inferences. Thus, the results are not attributable to the inclusion of multiple, nonindependent observations.[16]

Finally, the previous analysis models the probability of meeting or exceeding analysts' expectations and therefore combines observations that *exactly meet* analysts' earnings expectations with those that *exceed* analysts' expectations. Some firm characteristics may provide incentives to meet analysts' expectations exactly rather than exceed those expectations. For example, exactly meeting expectations may be more effective at avoiding shareholder litigation than exceeding expectations.

Consequently, I separately model the probability of exactly meeting (vs. not meeting) expectations and the probability of exceeding (vs. not meeting) expectations. The untabulated results suggest that different firm characteristics are associated with exactly meeting (vs. exceeding) expectations. When I model the probability of exactly meeting expectations, LOSS, LTG, and LIT are highly significant, suggesting that firms with a history of prior losses have weaker incentives, and firms with higher growth prospects and higher litigation risk have greater incentives, to exactly meet expectations. These variables are generally not significant when modeling the probability of *exceeding* expectations (LOSS and LTG are significant in the specification that uses %INST but not in the specification that includes %TRAN and %NONTRAN). In contrast, ICLAIM and EARNRET are not significant when modeling the probability of exactly meeting expectations, but are highly significant when modeling the probability of exceeding expectations. These results suggest that firms that rely on implicit claims with their stakeholders, and those in industries with more value-relevant earnings have greater incentives to exceed expectations (as oppose to meet expectations exactly). Finally, %INST and %TRAN are significant in both regressions; however, the effect is stronger in the model of exceeding expectations.

It is possible the differences between exactly meeting vs. exceeding expectations are the result of the different mechanisms used (earnings management vs. forecast guidance) to avoid negative surprises. In the next section, I explore these mechanisms further.

VI. MANAGING EARNINGS OR MANAGING EXPECTATIONS?

Managers can use two mechanisms to avoid negative earnings surprises—managing earnings upward or guiding analysts' forecasts downward. Prior studies have developed proxies for earnings management via discretionary accruals (e.g., Dechow et al. 1995) (hereafter DSS), but the academic literature has not developed similar proxies for forecast guidance. I use previously developed methods to measure abnormal accruals (as a proxy for the unobservable discretionary accruals) and I develop a similar approach to measure forecast guidance. I then use these two measures to determine whether the firm characteristics are associated with the tendency to manage earnings and/or analysts' forecasts.

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[16] I also conducted the analyses with dummy variables to control for time-period-specific effects. Including fiscal-quarter dummy variables (i.e., indicating fiscal quarters 1–4) yields similar inferences. Including dummy variables that indicate each quarter in the sample period also yields results similar to those reported in the tables, except that the coefficient on LIT is significant at $p < 0.01$ in both Models 1 and 2.

Proxy for Discretionary Accruals

To develop a proxy for discretionary accruals, I use the modified Jones model (Jones 1991) described in DSS, adapted for quarterly data:

$$TA_{ijtq}/A_{ijtq-1} = \alpha_{jt}[1/A_{ijtq-1}] + \beta_{1jt}[\Delta REV_{ijtq}/A_{ijtq-1}] + \beta_{2jt}[PPE_{ijtq}/A_{ijtq-1}] + \beta_{3jt}QTR4_{ijtq} + \varepsilon_{ijtq} \qquad (2)$$

where:

TA_{ijtq} = total accruals, defined as Δcurrent assets − Δcurrent liabilities − Δcash + Δshort-term debt − depreciation for firm i in two-digit SIC code j in quarter q of year t;[17]
A_{ijtq} = total assets for firm i in two-digit SIC code j in quarter q of year t;
ΔREV_{ijtq} = change in revenues for firm i in two-digit SIC code j in quarter q of year t;
PPE_{ijtq} = gross property, plant, and equipment for firm i in two-digit SIC code j in quarter q of year t;[18] and
$QTR4_{ijtq}$ = 1 if quarter q is the fourth fiscal quarter, 0 otherwise.

I include an indicator variable for the fourth fiscal quarter because accruals in the fourth quarter may differ from accruals in the first three quarters due to increased auditor scrutiny and firms' tendency to report special items in the fourth quarter (Francis et al. 1996).

I estimate the model for each firm-year using all firm-quarters in that year from the same two-digit SIC code except those from the firm for which I am estimating the parameters (as in DeFond and Jiambalvo [1994]). The sample includes only firm-years with ten or more firm-quarters of data in the same industry (to ensure sufficient data for parameter estimation). Applying the parameter estimates to the actual values for each firm-quarter in that year (adjusting for the change in receivables as in DSS) provides an estimate of total accruals. The difference between actual total accruals and this estimate represents abnormal accruals, and I use this difference as a proxy for the discretionary portion of total accruals. I classify firm-quarters with positive abnormal accruals (POSAA = 1) as having upward earnings management, and those with negative abnormal accruals (POSAA = 0) as having downward earnings management.

Proxy for Forecast Guidance

I develop a measure of forecast guidance using a method similar to the abnormal accrual measure. I first estimate the expected portion of analysts' forecasts by modeling the seasonal change in earnings as a function of the prior quarter's seasonal change in earnings and returns cumulated over the current year:

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[17] I define all change variables as the difference between quarter t and quarter t − 1 (not quarter t − 4) unless otherwise noted.

[18] For firms that report a balance for property, plant, and equipment (PPE, quarterly Compustat item no. 118) in the fourth fiscal quarter but report missing data in quarters 1–3, I compute the year-to-year change in PPE and add to each of the interim quarters a proportional amount of this change based on the proportion of annual depreciation incurred in that quarter.

$$\Delta EPS_{ijtq}/P_{ijtq-4} = \alpha_{jt} + \beta_{1jt}(\Delta EPS_{ijtq-1}/P_{ijtq-5}) + \beta_{2jt}CRET_{ijtq} + \varepsilon_{ijtq} \tag{3}$$

where:

ΔEPS_{ijtq} = earnings per share for firm i in four-digit SIC code j in quarter q of year t, less earnings per share for the same firm four quarters prior (i.e., quarter t − 4), as reported in Zacks;

P_{ijtq} = price per share for firm i in four-digit SIC code j at the end of quarter q of year t, as reported by quarterly Compustat (adjusted for splits); and

$CRET_{ijtq}$ = cumulative daily excess returns for firm i in four-digit SIC code j in quarter q of year t obtained from CRSP. Returns are cumulated from three days after the quarter t − 4 earnings announcement to 20 days before the quarter t earnings announcement.

This model is consistent with prior research that finds a positive serial correlation in the seasonal change in earnings (Freeman and Tse 1989; Bernard and Thomas 1990). I include the cumulative returns over the year (prior to the earnings announcement) to capture additional value-relevant information that an analyst might use to estimate earnings.[19] Ending the accumulation period 20 days before the current quarter's earnings announcement ensures the returns represent information that an analyst could use in forecasting earnings for the current quarter.

I estimate the model for each firm-year using all firm-quarters in that year from the same four-digit SIC code, except those from the firm for which I am estimating the parameters. Similar to the abnormal accrual model, the sample includes only firm-years with ten or more firm-quarters of data in the same industry. To mitigate the effect of extreme values on the parameter estimates, I delete the top and bottom half-percent of each variable (i.e., $\Delta EPS_t/P_{t-4}$, $\Delta EPS_{t-1}/P_{t-5}$, and $CRET_t$).

Table 6, Panel A presents descriptive statistics on the parameter estimates for all firm-years with data available. Parameter estimates for β_1 and β_2 are positive on average and highly significant as expected. In addition, the average adjusted R^2 for the estimation regressions is 0.24, suggesting that the model has reasonable explanatory power.

The estimate of analysts' expected forecast should use only data that would be available to analysts in making their forecast. Thus, I use the parameter estimates from the prior firm-year to determine the expected change in EPS (E[ΔEPS]). I then add this value to the earnings from the same quarter in the prior year to obtain the expected forecast (E[F]) of the current quarter's earnings:

$$E[\Delta EPS_{ijtq}] = [\hat{\alpha}_{jt-1} + \hat{\beta}_{1jt-1}(\Delta EPS_{ijtq-1}/P_{ijtq-5}) + \hat{\beta}_{2jt-1}CRET_{ijtq}] \times P_{ijtq-4}; \tag{4}$$

$$E[F_{ijtq}] = EPS_{ijtq-4} + E[\Delta EPS_{ijtq}]. \tag{5}$$

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[19] I include returns in my model of expected forecasts under the assumption that stock prices incorporate additional information not reflected in analysts' forecasts. For example, if the stock market adjusts for any downward bias in analysts' forecasts, then these higher expectations will be captured in higher stock returns. If, however, the markets' expectation is based solely on analysts' forecasts, then including returns in my model of expected forecasts will not help capture the effect of this guidance and will weaken the power of my tests for forecast guidance.

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TABLE 6
Descriptive Statistics on Proxy for Managers' Guidance of Analysts' Forecasts

Panel A: Parameter Estimates from a Model of Expected Change in EPS (n = 16,476)[a]

$$\text{Model: } \Delta EPS_{ijtq}/P_{ijtq-4} = \alpha_{jt} + \beta_{1jt}(\Delta EPS_{ijtq-1}/P_{ijtq-5}) + \beta_{2jt}CRET_{ijtq} + \varepsilon_{ijtq}$$

	Mean	*Standard Deviation*	*t-statistic*	*1st Quartile*	*Median*	*3rd Quartile*
α	0.002	0.005	45.35	−0.000	0.002	0.004
β_1	0.313	0.382	105.30	0.116	0.297	0.492
β_2	0.013	0.019	84.45	0.004	0.010	0.019
Adjusted R^2	0.241	0.219	141.39	0.081	0.196	0.363

Panel B: Descriptive Statistics on Unexpected Forecast (UEF) Proxy (n = 15,848)[b]

$$UEF_{ijtq} = F_{ijtq} - \{EPS_{ijtq-4} + [\hat{\alpha}_{jt-1} + \hat{\beta}_{1jt-1}(\Delta EPS_{ijtq-1}/P_{ijtq-5}) + \hat{\beta}_{2jt-1}CRET] \times P_{ijtq-4}\}$$

	Mean	*Standard Deviation*	*t-statistic*	*1st Quartile*	*Median*	*3rd Quartile*
UEF	−0.010	0.189	−6.563	−0.077	−0.005	0.059

[a] The panel reports the average parameter estimates from a model of expected changes in earnings per share, estimated for each firm-year using data for all firms in the same four-digit SIC code in that same year, except the firm for which the parameters are being estimated. ΔEPS_{ijtq} is the seasonal change in earnings per share as reported in Zacks' surprise file. P_{ijtq} is price per share as reported by Quarterly Compustat (adjusted for splits). $CRET_{ijtq}$ is the cumulated daily excess returns from three days after the quarter t − 4 earnings announcement to 20 days before the quarter t earnings announcement. The subscripts i, j, t, and q denote the firm, four-digit SIC code, year, and quarter, respectively.

[b] The panel reports descriptive statistics on a proxy for forecast guidance (UEF), calculated as the difference between the last published consensus forecast reported on Zacks surprise file (F) and the expected forecast based on the model reported in Panel A.

Subtracting the expected forecast from the last published consensus forecast for the quarter provides the "unexpected" portion of the forecast (UEF). If managers attempt to keep expectations low to avoid negative earnings surprises, then the actual consensus forecast will be less than the estimated forecasts, and UEF will be less than zero. Panel B of Table 6 provides descriptive statistics on UEF.[20] On average, analysts' published forecasts are approximately a penny less than the model predicts. The following analysis classifies firm-quarters as having downward-guided forecasts (DOWN = 1) if UEF is negative and as not having downward-guided forecasts (DOWN = 0) if UEF is positive.

Empirical Analysis

To provide evidence that the two proxies capture mechanisms for avoiding negative earnings surprises, I examine the relation between: (1) the probability of meeting or exceeding analysts' forecasts (MEET), and (2) the signs of abnormal accruals (POSAA) and unexpected forecasts (DOWN). Table 7, Panel A presents two contingency tables; the first

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[20] For these descriptive statistics, I winsorize the data, setting the value of the bottom and top 1 percent of UEF to the 1st and 99th percentile values, respectively. The results of the following analysis are unaffected by this procedure because I use only the sign of the unexpected forecasts to classify firms as having managed forecasts downward to meet analysts' expectations.

TABLE 7
Association between: (1) Meeting or Exceeding Analysts' Expectations, and (2) Managing Earnings or Guiding Forecasts

Panel A: Contingency Tables Classifying Firm-Quarters Based on (1) Meeting or Exceeding Analysts' Expectations, and (2) the Sign of Abnormal Accruals and Unexpected Forecasts

Number of firm-quarters (% of firm-quarters conditional on meeting or not meeting expectations)	*Proxy for Managing Earnings: Abnormal Accruals*[a]		*Proxy for Guiding Forecasts: Unexpected Forecasts*[b]	
	Positive (POSAA = 1)	*Negative (POSAA = 0)*	*Positive (DOWN = 0)*	*Negative (DOWN = 1)*
Meeting or Exceeding Expectations (MEET = 1)	7,758 (53.32%)	6,793 (46.68%)	4,898 (45.88%)	5,778 (54.12%)
Not Meeting Expectations (MEET = 0)	3,561 (49.98%)	3,564 (50.02%)	2,626 (50.77%)	2,546 (49.23%)
	$\chi^2 = 21.35$ $p < 0.001$		$\chi^2 = 33.48$ $p < 0.001$	

Panel B: Logit Analysis of the Probability of Meeting or Exceeding Analysts' Expectations as a Function of Abnormal Accruals, Unexpected Forecasts, and Control Variables

Model:

$$Prob(MEET = 1) = F(\beta_0 + \beta_1 POSAA_i + \beta_2 DOWN_i + \beta_3 POSUE_i + \beta_4 INDPROD_i + \beta_5 LOGMV_i + \beta_6 |FE_i| + \varepsilon_i)$$

Variable[c]	*Predicted Sign*	*Coefficient*	*p-value*[d]	*Marginal Effect*[e]
Intercept		−0.727	0.001	
POSAA	+	−0.025	0.506	−0.005
DOWN	+	0.380	0.001	0.081
Control Variables:				
POSUE	+	1.399	0.001	0.297
INDPROD	+	10.774	0.001	2.283
LOGMV	+	0.022	0.080	0.005
|FE|	−	−36.090	0.001	−7.648
Log Likelihood		−8,331.36		
Chi-square		2,380.24		
p-value		0.001		
No. of Observations:				
Meet/Exceed		10,219		
Did not meet		4,898		
Total		15,117		

TABLE 7 (Continued)

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[a] Abnormal accruals are the difference between actual total accruals and estimates of total accruals based on the modified version of the Jones model (Jones 1991) for each firm-quarter.

[b] Unexpected forecasts are the difference between the last published consensus forecast reported on Zacks surprise file and an expected forecast based on the model reported in Table 6, Panel A.

[c] POSAA equals 1 if abnormal accruals are positive and 0 otherwise. DOWN equals 1 if unexpected forecasts are negative and 0 otherwise. See Table 2 for remaining variable definitions.

[d] p-values are one-tailed.

[e] The marginal effects are computed as $e^{\beta'X}/(1 + e^{\beta'X})^2$, where $\beta'X$ is computed at the mean values of X.

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shows the relation between MEET and POSAA, and the second shows the relation between MEET and DOWN. Fifty-three percent of the firm-quarters in which reported earnings meet or exceed analysts' expectations (MEET = 1) show positive abnormal accruals, compared to only 50 percent of the firm-quarters in which reported earnings fall short of expectations (MEET = 0). Although the magnitude of this difference is not large, a Chi-square test indicates that the association is significant ($\chi^2 = 21.35$, $p < 0.001$). A similar analysis shows a significantly positive association between meeting expectations and guiding analysts' forecasts downward. In firm-quarters that meet or exceed analysts' expectations, 54 percent show a negative unexpected forecast compared to only 49 percent of firm-quarters that missed expectations ($\chi^2 = 33.48$, $p < 0.001$).

Panel B of Table 7 reports the results of a logit analysis modeling the probability of meeting or exceeding expectations as a function of the sign of abnormal accruals, unexpected forecasts, and the control variables identified earlier (POSUE, INDPROD, LOGMV, and |FE|). The coefficient on DOWN is highly significant—firm-quarters with negative unexpected forecasts show an 8 percent higher probability of meeting expectations. This result suggests forecast guidance is an important mechanism for avoiding negative surprises. The coefficient on POSAA, however, is not significant. In a specification of the model that excludes the control variables (untabulated), the coefficient on POSAA is significant ($p < 0.001$); however, after controlling for various factors associated with meeting expectations, the sign of abnormal accruals is not associated with the probability of meeting expectations. Either managers are less likely to manipulate accruals to avoid negative earnings surprises, or my proxy for discretionary accruals is less effective at capturing managerial discretion over earnings.[21]

Tests of Incentives to Manage Earnings to Avoid Negative Surprises

To test whether cross-sectional differences in incentives to avoid negative earnings surprises are associated with managers' exercise of discretion to increase reported earnings, I examine the association between POSAA and the incentive variables examined previously, for the subsample of firm-quarters in which reported earnings meet or exceed analysts' forecasts. If the hypothesized variables provide incentives to manage earnings upward to avoid negative earnings surprises, then these firm characteristics should be associated with positive abnormal accruals for the subgroup that meet or exceed expectations.

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[21] Collins and Hribar (2000) suggest that using the balance sheet approach to compute total accruals results in measurement error because of events such as mergers and acquisitions and discontinued operations. If I compute total accruals using the difference between earnings and cash flow from operations for firm-quarters with the data available, then I obtain similar results for this analysis (POSAA is not significantly associated with MEET when control variables are included). The results are also not affected by the use of two-digit (rather than four-digit) SIC code groupings to estimate the abnormal accrual model.

The results of this analysis (see Table 8) suggest that three of the incentive variables are significantly related to the probability of positive abnormal accruals. Firms with higher institutional ownership (%INST) and higher long-term growth forecasts (LTG) are more likely, and firms with consistent prior losses (LOSS) are less likely, to report positive abnormal accruals ($p \leq 0.03$). In addition, when partitioning the percent of institutional ownership between transient and nontransient ownership, I find that firms with higher transient institutional ownership (%TRAN) are more likely to have positive abnormal accruals ($p < 0.001$), whereas the percent of nontransient institutional ownership is unrelated to the sign of abnormal accruals. The coefficient on the implicit claim variable is positive but only marginally significant ($p = 0.07$) as is the coefficient on the other proxy variable for value-relevance of earnings, EARNRET ($p = 0.06$). The coefficient on the litigation risk variable (LIT) is negative, contrary to my predictions; firms in high-litigation-risk industries are less likely to report positive abnormal accruals. Managing earnings upward may not be an effective mechanism for avoiding shareholder litigation because such actions could exacerbate the problem should the firm face shareholder litigation.[22]

Finally, two of the control variables, POSUE and |FE|, are significant. The coefficient on POSUE indicates that abnormal accruals are more likely to be positive in quarters with increases in earnings.[23] The coefficient on |FE| is negative, consistent with firms not managing earnings upward in highly uncertain environments.

In further (untabulated) analysis, I performed logit regressions on the subsample of firm-quarters that did not meet analysts' expectations. For this subset of firm-quarters, I do not expect the firm characteristics to be associated with the probability of positive abnormal accruals. The results support this conjecture; none of the coefficients on the incentive variables are significant, with the exception of %TRAN, which is marginally significant ($p = 0.04$, one-tailed).

Tests of Incentives to Guide Forecasts Downward to Avoid Negative Surprises

I now test whether cross-sectional differences in incentives to avoid negative surprises are associated with managers' propensity to guide analysts' forecasts downward. Specifically, I examine the association between the incentive variables and the probability of a negative unexpected forecast (DOWN) for the subsample of firm-quarters in which reported earnings meet or exceed analysts' forecasts. The results of this analysis, presented in Table 9, suggest that firms with greater institutional ownership (%INST), and in particular transient institutional ownership (%TRAN), are more likely to guide analysts' forecasts downward to avoid negative surprises. Firms that rely more heavily on implicit claims with their stakeholders (ICLAIM) and firms in industries in which earnings are more value-relevant (EARNRET) are also more likely to guide forecasts downward, whereas firms with consistent prior losses (LOSS) are less likely to do so. Firms with higher long-term growth forecasts (LTG) display some evidence of guiding forecasts downward (Model 1), but when I allow the coefficients on transient and nontransient institutional ownership to vary, the

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[22] I also performed this analysis using a proxy that computes total accruals as earnings less cash flow from operations (when these data are available). In this specification, %INST and LOSS are no longer significant, but %TRAN and LTG remain significant. It is possible that measurement error caused by nonarticulation problems affects the reported results; however, because the use of cash flows from operations to define total accruals is not a widely used procedure (particularly using quarterly data), it is also possible that the loss of significance is due to measurement error in total accruals using this definition. Nevertheless, conclusions related to %INST and LOSS should be interpreted cautiously.

[23] DSS (1995) find a positive relation between firm performance and proxies for discretionary accruals. To the extent POSUE reflects firm performance, one would expect a positive relation between this variable and the sign of abnormal accruals.

TABLE 8

Logit Analysis of the Association between Incentives to Avoid Negative Earnings Surprises and Positive Abnormal Accrual Proxy for Earnings Management (conditional on meeting or exceeding analysts' expectations)[a]

Model 1:

$$Prob(POSAA = 1) = F(\beta_0 + \beta_1 \%INST_i + \beta_2 ICLAIM_i + \beta_3 LOSS_i + \beta_4 EARNRET_i + \beta_5 LTG_i + \beta_6 LIT_i + \beta_7 POSUE_i + \beta_8 INDPROD_i + \beta_9 LOGMV_i + \beta_{10}|FE_i| + \varepsilon_i)$$

Model 2:

$$Prob(POSAA = 1) = F(\gamma_0 + \gamma_1 \%TRAN_i + \gamma_2 \%NONTRAN_i + \gamma_3 ICLAIM_i + \gamma_4 LOSS_i + \gamma_5 EARNRET_i + \gamma_6 LTG_i + \gamma_7 LIT_i + \gamma_8 POSUE_i + \gamma_9 INDPROD_i + \gamma_{10} LOGMV_i + \gamma_{11}|FE_i| + \varepsilon_i)$$

		Model 1			*Model 2*		
Variable[b]	*Predicted Sign*	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]
Intercept		−0.281	0.028		−0.135	0.307	
Incentives for Avoiding Negative Earnings Surprises:							
%INST (H1)	+	0.167	0.026	0.042			
%TRAN	+				1.119	0.001	0.279
%NONTRAN	+				−0.169	0.932	−0.042
ICLAIM (H2)	+	0.029	0.073	0.007	0.023	0.118	0.006
LOSS (H3)	−	−0.306	0.005	−0.076	−0.274	0.011	−0.068
EARNRET (H3)	+	0.011	0.055	0.003	0.010	0.072	0.002
LTG (H4)	+	0.018	0.001	0.004	0.013	0.001	0.003
LIT (H5)	+	−0.099	0.995	−0.025	−0.110	0.998	−0.028
Control Variables:							
POSUE	+	0.238	0.001	0.059	0.233	0.001	0.058
INDPROD	+	−0.828	0.722	−0.206	−0.734	0.699	−0.183
LOGMV	+	−0.016	0.903	−0.004	−0.019	0.939	−0.005
|FE|	−	−8.230	0.001	−2.048	−8.704	0.001	−2.166
Log Likelihood			−9,975.00			−9,964.72	
Chi-square			158.03			178.49	
p-value			0.001			0.001	
No. of Observations:							
Positive abnormal accruals			7,758			7,758	
Negative abnormal accruals			6,793			6,793	
Total			14,551			14,551	

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[a] This table presents the results of logit regressions that test the association between the incentive and control variables and the probability of positive abnormal accruals, conditional on the firm meeting or exceeding analysts' expectations. The dependent variable equals 1 if the abnormal accruals estimated for the firm-quarter using a modified Jones model (Jones 1991) is positive, 0 otherwise.

[b] See Table 2 for variable definitions.

[c] p-values are one-tailed.

[d] The marginal effects are computed as $e^{\beta'X}/(1 + e^{\beta'X})^2$, where $\beta'X$ is computed at the mean values of X.

TABLE 9
Logit Analysis of the Association between Incentives to Avoid Negative Earnings Surprises and Negative Unexpected Forecast Proxy for Forecast Guidance (conditional on meeting or exceeding analysts' expectations)[a]

Model 1:

$$Prob(DOWN = 1) = F(\beta_0 + \beta_1\%INST_t + \beta_2 ICLAIM_t + \beta_3 LOSS_t + \beta_4 EARNRET_t + \beta_5 LTG_t + \beta_6 LIT_t + \beta_7 POSUE_t + \beta_8 INDPROD_t + \beta_9 LOGMV_t + \beta_{10}|FE_t| + \varepsilon_t)$$

Model 2:

$$Prob(DOWN = 1) = F(\gamma_0 + \gamma_1\%TRAN_t + \gamma_2\%NONTRAN_t + \gamma_3 ICLAIM_t + \gamma_4 LOSS_t + \gamma_5 EARNRET_t + \gamma_6 LTG_t + \gamma_7 LIT_t + \gamma_8 POSUE_t + \gamma_9 INDPROD_t + \gamma_{10} LOGMV_t + \gamma_{11}|FE_t| + \varepsilon_t)$$

		Model 1			*Model 2*		
Variable[b]	*Predicted Sign*	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]	*Coefficient*	*p-value*[c]	*Marginal Effect*[d]
Intercept		−0.560	0.001		−0.378	0.017	
Incentives for Avoiding Negative Earnings Surprises:							
%INST (H1)	+	0.418	0.001	0.104			
%TRAN	+				1.433	0.001	0.356
%NONTRAN	+				0.045	0.371	0.011
ICLAIM (H2)	+	0.091	0.001	0.023	0.086	0.001	0.021
LOSS (H3)	−	−0.875	0.001	−0.218	−0.845	0.001	−0.210
EARNRET (H3)	+	0.053	0.001	0.013	0.052	0.001	0.013
LTG (H4)	+	0.006	0.017	0.002	0.000	0.457	0.001
LIT (H5)	+	−0.154	0.999	−0.038	−0.165	0.999	−0.041
Control Variables:							
POSUE	+	−0.357	0.999	−0.089	−0.364	0.999	−0.090
INDPROD	+	1.018	0.541	0.253	1.129	0.498	0.280
LOGMV	+	0.067	0.001	0.017	0.063	0.001	0.016
|FE|	−	8.535	0.999	2.124	7.843	0.998	1.947
Log Likelihood			−7,254.43			−7,245.74	
Chi-square			218.59			235.97	
p-value			0.001			0.001	
No. of Observations:							
Negative unexpected forecasts			5,778			5,778	
Positive unexpected forecasts			4,898			4,898	
Total			10,676			10,676	

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[a] This table presents the results of logit regressions that test the association between the incentive and control variables and the probability of negative unexpected forecasts, conditional on the firm meeting or exceeding analysts' expectations. The dependent variable equals 1 if the actual consensus forecast in that quarter is less than the forecast based on the model presented in Table 6, Panel A, and 0 otherwise.

[b] See Table 2 for variable definitions.

[c] p-values are one-tailed.

[d] The marginal effects are computed as $e^{\beta'X}/(1 + e^{\beta'X})^2$, where $\beta'X$ is computed at the mean values of X.

coefficient on LTG is no longer significant. Again, it appears that long-term growth forecasts may reflect the level of transient institutional ownership in Model 1. Finally, the coefficient on the litigation risk variable is negative, contrary to my predictions. It appears that guiding analysts' forecasts downward may not be an effective mechanism for avoiding negative earnings surprises for firms in highly litigious industries, perhaps because of potential negative stock price reactions to downward forecast revisions.

The coefficient on POSUE is negative, contrary to expectations. If earnings in the same quarter of the prior year are unusually low (resulting in a positive change in earnings), then it is more likely that the forecast estimated from my model will be lower than that produced by analysts (DOWN = 0) because my expected forecast model uses the year-ago earnings as a base. The significantly positive coefficient on LOGMV indicates that large firms are more likely to manage forecasts downward.

I also performed (untabulated) logit regressions on the subsample of firm-quarters that failed to meet analysts' expectations. The coefficients on most of the incentive variables are not significant. The two exceptions are the coefficient on %TRAN, which is significantly positive ($p = 0.03$), and the coefficient on LOSS, which is significantly negative ($p < 0.001$). These results suggest that firms with higher transient institutional ownership are more likely, and firms with consistent losses are less likely, to manage forecasts downward even in quarters in which they fail to meet expectations.

In summary, the results of these analyses are consistent with firms using both earnings management and forecast guidance to avoid negative earnings surprises. However, certain firm characteristics are more highly associated with one mechanism than the other. Firms that rely more heavily on implicit claims with their stakeholders (ICLAIM) and firms in industries in which earnings are more value-relevant (EARNRET) appear to guide forecasts downward rather than manage earnings upward to avoid negative surprises. In contrast, high-growth firms (LTG) appear to manage earnings upward rather than guide forecasts downward to avoid missing expectations.[24] Firms with more institutional ownership, particularly transient institutional ownership, appear to engage in both earnings management and forecast guidance. Similarly, firms with a history of prior losses appear to be less likely to engage in both earnings management and forecast guidance. Overall, the firm characteristics examined in this study, as a group, appear to be better predictors of forecast guidance than of earnings management.[25]

VII. SUMMARY

This study investigates recent allegations in the business press that managers avoid reporting negative earnings surprises at the earnings announcement. Although past academic research documents that analysts' forecasts are optimistic on average (i.e., the average surprise is negative), recent business press articles suggest that managers use either their discretion over reported earnings or their ability to guide analysts' forecasts downward to increase the probability of meeting or beating these forecasts at the earnings announcement. I explore a number of firm characteristics that I expect create greater incentives to avoid negative earnings surprises, and examine the relation between these characteristics and the probability of meeting or exceeding analysts' expectations at the earnings announcement.

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[24] These results are consistent with the analyses performed earlier in which I found ICLAIM and EARNRET to be highly associated with exceeding expectations (but not meeting expectations exactly), whereas I found the opposite for LTG. To the extent that meeting expectations exactly is an indication of managing earnings rather than guiding forecasts, the earlier results are consistent with those found using the discretionary accrual and forecast guidance proxies in this section.

[25] However, my analysis includes only proxies for the incentive to manage earnings and not the opportunity to do so (e.g., weak corporate governance). If the ability to manage earnings is more restricted than is the ability to guide forecasts, then the weaker results for earnings management could reflect the absence of control for the opportunity to engage in earnings management.

The results indicate that firms with higher transient institutional ownership, greater reliance on implicit claims with their stakeholders, and higher value-relevance of earnings are more likely to take actions to meet or exceed analysts' forecasts at the earnings announcement. These associations exist even after controlling for other factors associated with the probability of meeting or exceeding analysts' earnings expectations, including the seasonal change in earnings, growth in industrial production, firm size, and the magnitude of the initial forecast error.

I also examine the mechanisms managers use to avoid negative surprises. I measure abnormal accruals using the modified Jones model (Dechow et al. 1995). I also develop a measure of forecast guidance by modeling an expected forecast based on prior earnings changes and returns during the period and comparing this expected forecast to the consensus analyst forecast. Both proxies are associated with avoiding negative earnings surprises in a univariate analysis, but only the proxy for forecast guidance is associated with avoiding negative surprises after controlling for the other variables that are likely associated with the probability of meeting or exceeding analysts' expectations. Although this finding is consistent with forecast guidance dominating earnings management as a mechanism for avoiding negative surprises, it is also possible that the earnings management proxy suffers from more measurement error than does the forecast guidance proxy.

I also examine the relation between the incentives to avoid negative earnings surprises and the probability (1) that the firm reports positive abnormal accruals, and (2) that analysts issue forecasts that are lower than expected, both conditional on meeting analysts' expectations. The results suggest that firms with higher transient institutional ownership are more likely, and firms with a consistent pattern of prior losses are less likely, to both manage earnings upward and guide forecasts downward. Firms that rely more on implicit claims with their stakeholders, and firms in industries in which earnings are more value-relevant, appear to guide forecasts downward but not to manage earnings upward. High-growth firms appear to do the opposite—manage earnings upward but not guide forecasts downward. Overall, the results suggest that both mechanisms play a role in avoiding negative earnings forecasts. If the recent passage of Reg FD limits the extent to which managers are able to guide analysts' forecasts, then avoiding negative earnings surprises may prove more difficult in the future. The results of this study suggest firms that relied on forecast guidance in the past will be affected by the new regulation's limitations on their ability to manage expectations.

This study is one of the first to explore a comprehensive set of management incentives to avoid negative earnings surprises. However, the substantial data requirements skew the sample toward larger firms, so the results may not generalize to the broader population of firms. In addition, although this study is among the first to investigate and provide evidence consistent with managers guiding analysts' forecasts in order to avoid negative surprises, the persuasiveness of this evidence depends critically on my proxy for forecast guidance. Similarly, evidence on the use of earnings management to avoid negative surprises depends on the effectiveness of abnormal accruals as a proxy for earnings management. Moreover, I do not control for potential differences across firms in the opportunities to engage in earnings management and forecast guidance. Thus, readers should use caution in drawing conclusions on the relative importance of these two mechanisms for avoiding negative surprises.

This paper also does not provide evidence that managers (or their firms) are better off as a result of taking actions to avoid negative surprises. A natural extension of research to address this second question would examine the extent to which the stock market can undo the effect of any bias in the consensus forecast for a firm. Prior research suggests that the market seems to react to the bias in analysts' forecasts (La Porta 1996; Dechow et al. 2000), but recent reports in the business press suggest that the prevalence of positive earnings surprises has diluted market reactions to positive news when firms announce earnings (Ip 1997a). Finally, although this study provides evidence consistent with an increasing importance on meeting or beating analysts' forecasts in recent years, the academic literature has yet to explore the underlying cause of this shift.

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Expanded Disclosures and the Increased Usefulness of Earnings Announcements

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ABSTRACT: We investigate three explanations for prior studies' finding that the usefulness of earnings announcements, as measured by their absolute market responses, has increased over time. We confirm this increase for a sample of 426 relatively large, stable firms over 1980–1999. We find no evidence that this over-time increase in the magnitude of the market reaction to our sample firms' earnings announcements is attributable to increases in the absolute amount of unexpected earnings conveyed in the announcements or to increases in the intensity of investors' average reaction to unexpected earnings. To test the third explanation—an over-time expansion in the amount of concurrent (with bottom line earnings) information in earnings announcement press releases—we analyze and code the contents of 2,190 earnings announcement press releases made by 30 of our sample firms over 1980–1999. Concurrent disclosures increased significantly over this period and we find that these concurrent disclosures, especially the inclusion of detailed income statements, explain increases in the absolute market reactions to earnings announcements for our sample firms.

Keywords: *capital markets; earnings-return relation; earnings announcements; earnings response coefficients.*

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Data Availability: *The data used in this study are publicly available from the sources indicated in the text.*

I. INTRODUCTION

Recent studies infer the increasing usefulness of earnings announcements from over-time increases in the absolute or squared abnormal stock returns or abnormal trading volume at earnings announcement dates (e.g., Kross and Kim 2000; Landsman and Maydew 2002; Francis et al. 2002); the increase appears to be concentrated among large firms (Buchheit and Kohlbeck 2002; Lo and Lys 2001). We examine three explanations for this trend using a sample of 426 large firms: (1) over-time increases in the amount of unexpected earnings news; (2) over-time increases in investors' reaction to a unit of unexpected earnings news; and (3) over-time increases in the amount of other (besides "bottom line" earnings) information disclosed in the earnings announcement press release.[1]

It is important to distinguish among these three explanations because of their differing implications about the financial reporting information environment. The first explanation speaks to whether earnings are increasingly bringing news to investors (as opposed to summarizing or aggregating it); evidence in support of this explanation would suggest reconsidering claims that the summary earnings number is increasingly irrelevant to investors.[2] The second explanation speaks to behavioral changes among investors such that they respond more intensively to a given piece of earnings information. Such changes might result from properties of earnings (e.g., increasing persistence) or from economic or psychological factors unrelated to financial reporting practices, *per se*. The third explanation speaks to voluntary disclosure practices that are related to but distinct from financial reporting as it is regulated by generally accepted accounting principles. Evidence that the earnings *announcement* but not the summary earnings number itself is conveying increasing amounts of information to investors would suggest that discussions of changes in the usefulness of financial reporting should consider the package of information that the earnings announcement press release reveals to investors, including disaggregated and forward-looking earnings information.

The first explanation we consider is based on prior studies' findings that the magnitude of the market reaction to earnings announcements increases with the magnitude of unexpected earnings (e.g., Beaver et al. 1979), and that the relation is not linear (e.g., Freeman and Tse 1992; Basu 1997; Lipe et al. 1998). If the amount of absolute unexpected earnings revealed by earnings announcements has increased over time, then we expect to observe increases in the magnitude of the absolute market reactions to these announcements.

The second explanation is that the intensity of investors' reactions to a unit of unexpected earnings news, as measured by the earnings response coefficient (ERC), has increased over time. We investigate two sources of possible changes in ERCs: (1) increases in ERCs resulting from changes in factors known to affect ERCs—such as an increase in earnings persistence, or declines in systematic risk or risk-free interest rates (Easton and Zmijewski 1989; Collins and Kothari 1989); and (2) increases in ERCs resulting from shifts in the distribution of unexpected earnings to more steeply sloped regions of the unexpected returns-unexpected earnings curve. In terms of the latter, prior research shows that the ERC for extreme values of unexpected earnings (either positive or negative) is less than the ERC for moderate values of unexpected earnings, that the ERC for negative earnings surprises is less than the ERC for positive earnings surprises, and that the ERC for losses is smaller still. Consequently, if the distribution of unexpected earnings has become less extreme and/or characterized by fewer loss observations over time, then this might explain over-time increases in the absolute reactions to earnings announcements.[3] Our analysis of over-time changes in ERCs as an explanation for the increased market reactions to earnings announcements differs from Lev and Zarowin's (1999) analysis, which examines changes in ERCs from long-window association tests and does not investigate the sources of these changes.

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[1] A fourth explanation is suggested by Francis et al.'s (2002) finding that the absolute abnormal returns to earnings announcements are significantly positively correlated with the absolute abnormal returns to analyst reports. This complementary relation suggests increasing usefulness of analyst reports may induce increasing usefulness of earnings announcements. Further tests reported by Francis et al. (2002) do not support the conjecture that the increased usefulness of earnings announcements is driven by the use of analyst reports; in particular, they find that the reactions to earnings announcements *increased* even for a sample of firms where the reactions to analyst reports markedly *decreased*.

[2] For example, Lev and Zarowin (1999) and Francis and Schipper (1999) discuss the concern that financial statements in general (not just earnings) have lost relevance to investors.

The third explanation is based on the Hoskins et al. (1986) finding that earnings announcement press releases often convey other information (besides bottom line earnings) that explains a significant portion of the market reaction to earnings announcements. Our analysis of concurrent disclosures in earnings announcement press releases is consistent with Lo and Lys' (2001) conjecture that the disparity between results showing *increasing* information content of earnings announcements (as measured by absolute value or squared return responses and/or volume responses) and *decreasing* value-relevance of earnings (as measured, for example, by the R^2 from a regression of returns on earnings levels and changes) may be due to other disclosures released concurrently with the summary earnings number. Specifically, we examine whether concurrent disclosures have increased over time, and if so, whether this increase explains the increased price reactions to earnings announcements. Relative to Hoskins et al. (1986), we examine an expanded set of information items and we focus on trends in the disclosures over the past 20 years.

We begin by documenting the increased usefulness of earnings announcements for the 426 firms for which Compustat reports earnings announcement dates for every quarter from 1980 to 1999. Requiring a complete series of 80 Compustat earnings announcement dates ensures that the results are not due to changes in sample composition. This requirement also biases the sample toward long-surviving firms, which tend to be large and stable. Given prior studies' evidence that over-time increases in market reactions to earnings announcements are concentrated in large firms (Lo and Lys 2001; Buchheit and Kohlbeck 2002), this selection bias increases the likelihood that our sample exhibits the phenomenon under investigation. The focus on large firms also suggests that the results may not be generalizable to broader samples of firms, particularly if the propensity to include concurrent disclosures in earnings announcements is primarily a large-firm phenomenon.

To test the first explanation, we analyze the trend in the absolute magnitudes of unexpected earnings conveyed by earnings announcements. Results examining the absolute value of unexpected earnings (measured using a seasonal random walk [SRW] model and, separately, using analysts' forecasts) show a decline in the SRW measure and no pattern in the analyst measure. Both sets of results are inconsistent with the first possible explanation—that the increase in the magnitude of the market reaction is attributable to an increase in the magnitude of unexpected (bottom line) earnings. Tests of the second explanation—that the intensity of investors' reactions to a unit of unexpected earnings news has increased over time—examine the trend in investors' response to a unit of unexpected earnings. Regardless of whether we use SRW or analysts' forecasts to measure unexpected earnings, we find a significant decline in ERCs, inconsistent with the second explanation for the increased usefulness of earnings announcements.

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[3] Recent research by Givoly and Hayn (2000) is not consistent with fewer loss observations over time, even in broad samples of successful firms. Specifically, they find that firms listed on Compustat continuously from 1968 to 1998 report increasing frequencies of losses, with the frequency of loss observations increasing from 13.6 percent during 1981–1985 to 20.3 percent during 1991–1998. They also report an increase in the frequency of loss observations for the Compustat population as a whole (from 28.6 percent in 1981–1985 to 33.6 percent in 1991–1998).

Our analysis of earnings announcement press releases suggests that the missing piece to this puzzle is an over-time increase in the amount of concurrent information that firms disclose in these press releases. Specifically, for a sample of 2,190 press releases made by a subset of 30 (randomly selected from the 426) firms over 1980–1999, we find a significant expansion in the number of concurrent disclosures. For our sample, earnings announcement press releases are increasingly likely to include detailed financial statements, information about current operating data (such as shipments, orders, and margins), and information about nonrecurring earnings components (such as restructuring charges and foreign exchange gains or losses). These trends in concurrent disclosures explain the over-time increase in the absolute price reactions to earnings announcements for our sample firms. The most important concurrent disclosure, in terms of explaining the increasing reactions to earnings announcements, is a detailed income statement. We conclude that managers' voluntary decisions to expand concurrent disclosures in earnings announcement press releases (especially by including detailed income statements) drive the increased usefulness of those announcements to investors.

Long-window association tests reported in Francis and Schipper (1999) and Collins et al. (1997) suggest that the earnings number, as a single aggregate performance measure, declined in usefulness over 1980–1999. In contrast, our results suggest that the disclosure of earnings *components*—that is, the disaggregated elements that make up the earnings number—has increased the usefulness of the information package (the press release) whose central element is the summary earnings number. This latter finding, which is consistent with long-window research showing that earnings components provide information beyond that contained in the summary earnings number (e.g., Lipe 1986), suggests that discussions of changes in the usefulness of financial reports should approach the issue from the perspective of the bundle of information made available to investors in the financial reporting process, and not from the narrow perspective of bottom line earnings.

The rest of the paper is organized as follows. Section II details the sample and documents patterns in market reactions to earnings announcements similar to those shown in prior research (e.g., increasing usefulness and a positive association between unexpected returns and unexpected earnings for large, long-lived firms). Section III describes our investigations of over-time changes in unexpected earnings and in the intensity of investors' responses to unexpected earnings. Section IV reports the analysis of trends in the concurrent disclosures in earnings announcement press releases. Section V summarizes the results and concludes.

II. SAMPLE DESCRIPTION AND REPLICATION OF PRIOR RESULTS ON THE INCREASING USEFULNESS OF EARNINGS ANNOUNCEMENTS

Sample and Descriptive Statistics

Our sample includes all firms with four quarterly earnings announcements on Compustat (primary, secondary, tertiary, and research databases) for *each* calendar year t = 1980–1999 and with common stock returns on CRSP for the entire 20-year period. In total, 426 firms with 34,080 quarterly announcements meet these criteria. The benefits of this design choice include control over sample composition, and the increased likelihood of finding the phenomenon we wish to study because the increased usefulness of earnings announcements appears to be concentrated among larger firms (Buchheit and Kohlbeck 2002; Lo and Lys 2001). The cost is the inability to generalize results beyond the arguably large, surviving firms meeting our sample selection criteria.

Table 1 reports descriptive information about the average values of the sample firms' assets, market values, market-to-book ratios, and return-on-asset values for each year t = 1980–1999; we also report mean values calculated for the pooled sample (row labeled "All years") as well as for all Compustat firms with any available data (row labeled "All Compustat"). The rows labeled TREND in Table 1 show coefficient estimates and p-values from regressing the annual means for the sample and for the Compustat population on $TREND_{tq} = t - 1979$. Given the selection criteria, it is not surprising that the reported asset and market values indicate that, relative to the population of Compustat firms, our sample firms are larger and grew at a faster rate over 1980–1999 (differences, not reported, are significant at the 0.001 level). As we note in subsequent tests, controlling for firm size and for changes in firm size does not affect our inferences. However, these controls for within-sample size effects do not shed light on whether the results we report would generalize to other samples composed of significantly smaller firms.

Table 1 also shows that sample firms' market-to-book ratios increased over 1980–1999 (the trend slope coefficient is 0.08, significant at the 0.001 level), consistent with intuition that surviving firms perform well. The population of Compustat firms also experienced an increasing trend in market-to-book ratios, but at a smaller rate than the sample firms (0.06 per year vs. 0.08 per year, difference significant at the 0.05 level). Return-on-asset comparisons provide further evidence of our sample firms' superior performance. Our sample firms enjoy a substantially higher return on assets than the Compustat population (1.39 percent vs. −5.52 percent, difference significant at the 0.001 level).[4] Return on assets decreased over the sample period for both the sample firms and the Compustat population (the trend for the sample is –0.02 percent per year compared to –0.43 percent for the Compustat population, both trends significant at the 0.001 level). Givoly and Hayn (2000, Tables 1 and 2) report similar decreases in other earnings-based performance measures and attribute the trend to an increasing conservatism in financial reporting.

Because some of the subsequent analyses examine over-time changes in the relation between unexpected returns and unexpected earnings, we examine three factors prior research has shown to affect the intensity of this relation. Table 1 provides descriptive evidence on risk-free rates (measured as the compounded monthly short-term Treasury bill rate in year t), systematic risk or beta (measured by the slope coefficient from firm-specific market model regressions), and persistence (measured as the slope coefficient, ϕ_1, from the Foster (1977) seasonal first-order autoregressive model, estimated in cross-section for each year t). Annual estimates of risk free rates, betas and ϕ_1 are reported in the three rightmost columns of Table 1; the last row of Table 1 shows the trend in each variable. The significant (at the 0.001 level) declines in risk-free rates, and betas[5] suggest an increase in ERCs over 1980–1999; however, the significant (at the 0.003 level) decline in persistence suggests a decrease in ERCs. Given this mixed evidence, we do not predict whether the ERCs for our sample firms likely increased or decreased during 1980–1999.

Market Reactions to Earnings Announcements

We calculate beta-adjusted abnormal returns on each of the three days surrounding each quarterly earnings announcement; market-adjusted and size-adjusted abnormal returns yield similar results (not reported). The beta-adjusted abnormal return, AR_{jtq}, is $R_{jtq} - \beta_{jt-1} R_{Mtq}$ where R_{jtq} is firm j's raw return on the qth earnings announcement date of year t, R_{Mtq} is

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[4] The −5.52 percent return on assets for the Compustat population is similar to that calculated from Givoly and Hayn's (2000, Table 1) data where the average return on assets over 1980–1998 is −6.06 percent.

[5] The decline in beta is not sensitive to the inclusion of the t = 1999 observation, where the mean beta of 0.50 is significantly smaller than the betas for other years. We have no explanation for the decrease in beta in 1999.

TABLE 1
Descriptive Information about the 426 Firms for which Compustat Reports Earnings Announcement Dates for All Quarters, 1980–1999

Year	*Assets*	*Market Value of Equity*	*Market-to-Book Ratio*	*Return on Assets*	*Risk-Free Rate*	*Beta*	*Persistence*
1980	2,512	1,043	1.24	2.02%	11.26%	0.83	0.38
1981	2,780	1,245	1.47	1.85	14.72	0.83	0.41
1982	3,041	1,138	1.19	1.44	10.53	0.81	0.60
1983	3,248	1,510	1.54	1.35	8.80	0.82	0.21
1984	3,505	1,658	1.56	1.60	9.84	0.88	0.28
1985	3,720	1,728	1.57	1.30	7.72	0.88	0.24
1986	4,144	2,132	1.84	1.17	6.16	0.87	0.12
1987	4,483	2,574	2.04	1.29	5.47	0.94	0.18
1988	5,140	2,606	1.92	1.51	6.36	0.82	0.08
1989	6,031	2,691	1.92	1.47	8.38	0.81	0.11
1990	6,968	3,302	2.02	1.26	7.84	0.85	0.23
1991	7,203	3,235	1.86	0.94	5.60	0.85	0.25
1992	7,632	3,897	2.08	0.99	3.50	0.86	0.17
1993	8,225	4,285	2.34	1.09	2.90	0.80	0.06
1994	8,873	4,435	2.38	1.29	3.91	0.88	0.04
1995	9,522	4,866	2.26	1.49	5.60	0.78	0.34
1996	10,408	6,247	2.46	1.48	5.20	0.76	0.18
1997	11,376	7,742	2.60	1.53	5.25	0.71	0.13
1998	12,619	10,319	2.97	1.37	4.85	0.75	0.12
1999	14,589	12,117	2.70	1.29	4.69	0.50	0.03
All years[a]	6,813	3,967	2.00	1.39%	6.93%	0.81	0.19
TREND[b]	587	401	0.08	−0.02%	−0.40%	−0.01	−0.02
(p-value)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.003)
All Compustat[a]	925	554	2.66	−5.52%			
TREND[b]	24	33	0.06	−0.43%			
(p-value)	(0.001)	(0.001)	(0.001)	(0.001)			

(*Continued on next page*)

TABLE 1 (Continued)

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[a] The row labeled "All years" shows the mean value of the noted financial variable calculated for the pooled sample. The row labeled "All Compustat" shows similar information calculated for all firms on Compustat (primary, secondary, tertiary, and research databases) with available information on the noted financial variable.

[b] We report the slope coefficient estimates and related significance levels from regressing the year t value of each variable on $TREND_t$.

Variable definitions:

Financial variables are the average values of the variable measured at the beginning of each quarter of year t. The risk-free rate in year t is the compounded monthly Treasury bill rate. Beta for year t is the average of the 426 firm betas estimated using all calendar day returns in year t. Persistence is the AR1 parameter, ϕ_1, from estimating $A_{q+1} - A_{q-3} = \phi_0 + \phi_1 (A_q - A_{q-4}) + \zeta_q$ using all quarterly q earnings announcements in year t. $TREND_t = t - 1979$.

the value-weighted market return on that day, and β_{jt-1} is firm j's market model beta estimated using all trading day returns in year t − 1. We select the day with the largest (in absolute value) abnormal return in the three days surrounding the qth earnings announcement to measure the market's reaction. (Results based on the average of the absolute abnormal returns on days (−1, 0, +1) are similar and are not reported.)

Table 2 shows that the mean value of $|AR_{jtq}|$, calculated across all earnings announcements in year t, ranges from 2.5 percent in 1989 to 4.2 percent in 1999. For the pooled over-time sample announcements, the average absolute abnormal price response is 3.1 percent. A regression of $|AR_{jtq}|$ on $TREND_{tq} = t - 1979$, not tabulated, shows a significantly (at the 0.001 level) positive trend of 0.04 percent per year. We also examine the standardized absolute abnormal return, std$|AR_{jtq}|$, defined as $|AR_{jtq}|$ divided by the standard deviation of firm j's absolute abnormal returns on all trading days in year t. Deflating by the standard deviation of firm j's absolute abnormal returns on all trading days in year t controls for trends in $|AR_{jtq}|$ resulting from changes in the underlying volatility of the stock. Table 2 reports a pooled mean value of std$|AR_{jtq}|$ of 2.42, indicating that, on average, the absolute abnormal return to earnings announcements is more than two standard deviations away from the mean daily absolute abnormal return during the year. Tests of the trend in std$|AR_{jtq}|$ (not tabulated) show an increase of 0.026 percent per year, significant at the 0.001 level.

To benchmark the economic importance of the market responses to earnings announcements, we rank all daily absolute abnormal returns for each firm-year (there are approximately 250 trading days per firm-year) and calculate the decile associated with the $|AR_{jtq}|$ for quarterly earnings announcements. These results (not tabulated) show that the median $|AR_{jtq}|$ for earnings announcement dates is in the largest decile of market responses, with the mean $|AR_{jtq}|$ for earnings announcement dates falling between the largest and next largest deciles. An analysis of the frequency of earnings announcement reactions in the largest two decile responses (see Figure 1) shows a significant (at the 0.001 level, not reported) increase over 1980–1999. That is, over the sample period, it is increasingly likely that earnings announcements are associated with the largest price movements. Together with the results in Table 2, these findings indicate that the sample earnings announcements are highly significant events that became even more significant over the sample period.

To assess whether the over-time trend in the usefulness of earnings announcements is pervasive for the sample firms, we estimate the regression of $|AR_{jtq}|$ on $TREND_{tq}$ and std $|AR_{jtq}|$ on $TREND_{tq}$, for each firm (results not tabulated). We find that 77 percent of the estimated TREND coefficients are positive, and that the positive tail is longer and fatter than the negative tail. These results suggest that, not only are upward trends in market reactions to earnings announcements more common than downward trends, but the extreme upward trends are more steeply sloped than the extreme downward trends.

In unreported tests, we also examine whether the trends in $|AR_{jtq}|$ and std $|AR_{jtq}|$ are sensitive to firm size (measured as the log market capitalization for the prior quarter), and whether the trends differ for quarter q = 4 vs. q = 1–3 announcements and for positive vs. negative reactions. Inclusion of a size variable (and size interacted with trend) in the

TABLE 2
Market Reactions and Earnings Response Coefficients (ERCs), 1980–1999

	Absolute Abnormal Return Metric[a]		*Dependent Variable:* AR_{jtq}		*Dependent Variable:* $stdAR_{jtq}$	
Year	$\lvert AR_{jtq}\rvert$	$std\lvert AR_{jtq}\rvert$	*ERC*[b]	*(p-value)*	*ERC*[b]	*(p-value)*
1980	0.030	2.175	0.265	(0.001)	12.357	(0.001)
1981	0.029	2.253	0.383	(0.001)	18.132	(0.001)
1982	0.031	2.250	0.317	(0.001)	13.445	(0.001)
1983	0.029	2.302	0.207	(0.001)	9.880	(0.001)
1984	0.026	2.233	0.170	(0.001)	8.254	(0.001)
1985	0.025	2.271	0.173	(0.001)	8.350	(0.001)
1986	0.029	2.264	0.256	(0.001)	8.945	(0.001)
1987	0.036	2.311	0.153	(0.001)	5.125	(0.001)
1988	0.026	2.240	0.143	(0.001)	7.707	(0.001)
1989	0.025	2.310	0.194	(0.001)	8.492	(0.001)
1990	0.031	2.389	0.078	(0.039)	6.488	(0.002)
1991	0.034	2.534	0.233	(0.001)	8.042	(0.001)
1992	0.034	2.638	0.334	(0.001)	10.272	(0.001)
1993	0.033	2.682	0.219	(0.001)	9.068	(0.001)
1994	0.029	2.666	0.228	(0.001)	10.499	(0.001)
1995	0.029	2.609	0.316	(0.001)	9.202	(0.001)
1996	0.030	2.609	0.110	(0.007)	5.551	(0.011)
1997	0.030	2.584	0.192	(0.001)	5.927	(0.007)
1998	0.038	2.506	0.172	(0.033)	6.400	(0.008)
1999	0.042	2.564	0.181	(0.002)	6.046	(0.001)
All years	0.031	2.420				
All years: SRW			0.215	(0.001)	8.803	(0.001)
All years: Analyst			0.248	(0.001)	11.400	(0.001)

(Continued on next page)

TABLE 2 (Continued)

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[a] We report the mean $|AR_{jtq}|$ and std$|AR_{jtq}|$ calculated across all quarterly earnings announcements in year t. The "All years" row reports the mean values of these numbers across the entire sample period.

[b] We report the coefficient estimate (ERC) and significance level from regressing the signed market reaction to each quarterly earnings announcement (measured by AR_{jtq} or by stdAR_{jtq}) in year t on the seasonal random-walk (SRW) measure of UE_{jtq}:

$$AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \xi_{jtq}; \quad (1)$$

$$\text{std}AR_{jtq} = \varphi_0 + \varphi_1 UE_{jtq} + \zeta_{jtq}. \quad (2)$$

The "All years: SRW" row shows similar information estimated for the pooled sample, and the "All years: Analyst" row shows the pooled regressions for the analyst measure of unexpected earnings.

Variable definitions:

$|AR_{jtq}|$ = the mean absolute abnormal return to firm j's quarter q earnings announcement made in year t;

std $|AR_{jtq}|$ = $|AR_{jtq}|$ divided by the standard deviation of firm j's absolute abnormal returns on all trading days in year t;

AR_{jtq} = the signed abnormal return to firm j's quarter q earnings announcement made in year t;

stdAR_{jtq} = AR_{jtq} divided by the standard deviation of firm j's signed abnormal returns on all trading days in year t; and

UE_{jtq} = price-scaled unexpected earnings conveyed by firm j's quarter q earnings announcement made in year t.

SRW denotes unexpected earnings measured using the seasonal random-walk model.

Analyst denotes unexpected earnings measured using analysts' most recent forecast of the firm's earnings for quarter t.

trend regressions shows no evidence that the over-time increase in reactions to earnings announcements is driven by firm size or changes in firm size.[6] We find no significant difference in the trends in market reactions to q = 4 vs. q = 1–3 announcements. Tests that partition on the sign of the reaction show that while absolute price responses are larger for negative reactions than for positive reactions, the trends in return metrics are similar for the two groups.

Our final descriptive test confirms that our sample exhibits the well-known positive association between signed market reactions to earnings announcements, AR_{jtq}, and the sign and magnitude of unexpected earnings conveyed by these announcements (first documented by Beaver et al. [1979]). We measure unexpected earnings, UE_{jtq}, in two ways. The first measure, SRW-UE, is the seasonal random-walk difference in reported quarterly earnings-per-share before extraordinary items (Compustat item number 9), divided by share price at the beginning of quarter t. The second measure, Analyst-UE, is the difference between reported quarterly earnings-per-share before extraordinary items (Compustat item number 9) and the most recent analyst forecast from Zacks Investment Research database, divided by share price 10 days before the forecast date. Analysts' forecasts are not available for all earnings announcements, reducing the sample used for the analyst-based tests to 15,064 earnings announcements. We focus on SRW-UE for comparability with Lipe et al. (1998) and Teets and Wasley (1996); the SRW-UE is also consistent with the random walk models

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[6] The positive trend in price reactions to earnings announcements is evident both for firms below the sample median value of firm size and for firms above the sample median (results not reported). Because our analysis of size effects is within-sample, we cannot generalize these results to other samples of significantly smaller firms.

FIGURE 1
Percent of Absolute Abnormal Market Reactions to Earnings Announcements in Top Two Deciles of Daily Absolute Abnormal Returns

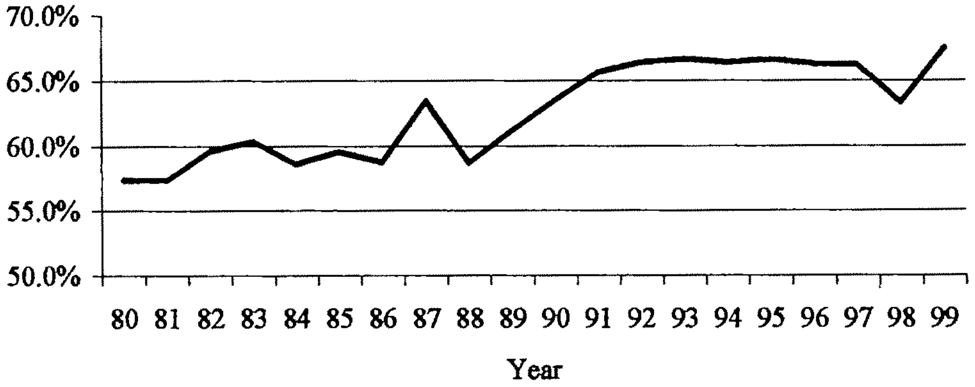

Figure 1 shows the percent of the sample firms' quarterly earnings announcements in year t = 1980–1999 with absolute abnormal returns ranked in the two deciles containing the largest daily absolute abnormal returns for each firm.

Hayn (1995) and Basu (1997) use to study annual earnings. We tabulate summary results for Analyst-UE, and where they differ from the SRW-UE results, we discuss them in the text. For comparability with prior ERC studies, we winsorize extreme positive and extreme negative values of the UE_{jtq} measures to the 99th and 1st percentile values, respectively, of the distribution. The results are not sensitive to the treatment of outliers.

Table 2 reports the coefficient estimates and significance levels for the following regressions, run separately for each year t, and also for the pooled sample:

$$AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \xi_{jtq}, \quad (1)$$

$$stdAR_{jtq} = \varphi_0 + \varphi_1 UE_{jtq} + \zeta_{jtq}, \quad (2)$$

where $stdAR_{jtq} = AR_{jtq}$ divided by the standard deviation of firm j's *signed* abnormal returns on all trading days in year t.

The pooled sample results show earnings response coefficient estimates for Equation (1) of 0.215 for SRW-UE and 0.248 for Analyst-UE; both are significantly positive at the 0.001 level.[7] The SRW-ERC value compares to a pooled ERC of 0.11 documented by Freeman and Tse (1992) for quarterly earnings announcements made between 1984–1987, and to a pooled ERC of 0.04 reported by Lipe et al. (1998) for quarterly earnings announcements made between 1985–1995. The SRW-ERC and Analyst-ERC based on the standardized market reactions (Equation (2)) are also significantly positive at the 0.001 level. All individual year estimations of Equations (1) and (2) show a positive relation between unexpected returns and unexpected earnings, with p-values ranging from 0.001 to 0.039.

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[7] Results in Teets and Wasley (1996) and Lipe et al. (1998) indicate that cross-sectional ERC estimates are biased downward relative to estimates from firm-specific time-series regressions. Our research question—which pertains to over-time changes in ERCs—is more directly addressed by using cross-sectional estimations. In addition, our inferences about *changes* in ERCs should not be affected unless the bias in the cross-sectional estimation changes systematically over time.

In summary, we find an increasing trend in market reactions to earnings announcements and a positive relation between unexpected returns and unexpected earnings for our sample firms. In the next section, we explore two possible causes of the increased market reactions: over-time increases in absolute earnings surprises and shifts in one or more elements of the returns-earnings relation reported in Table 2.

III. TESTING FOR OVER-TIME CHANGES IN ABSOLUTE EARNINGS SURPRISES AND SHIFTS IN THE UNEXPECTED RETURNS-UNEXPECTED EARNINGS RELATION

We first test whether the increased usefulness of earnings announcements derives from over-time shifts in the distribution of the magnitude of unexpected earnings. Beaver et al.'s (1979) finding of a significant positive association between AR_{jtq} and UE_{jtq} suggests that for a given positive ERC, $|AR_{jtq}|$ will be larger for larger values of $|UE_{jtq}|$. However, prior studies suggest that ERCs themselves are related to the magnitude of unexpected earnings (Freeman and Tse 1992), the sign of unexpected earnings (Basu 1997) and the sign (profit or loss) of the earnings number (Hayn 1995; Lipe et al. 1998; Butler 2000). In the remainder of this section, we examine whether changes in unexpected earnings and changes in ERC interactions with unexpected earnings explain the trends in reactions to earnings announcements.

Magnitude of Unexpected Earnings

For a given level of ERC, larger values of $|UE_{jtq}|$ will lead to larger market reactions. To probe the effect of the magnitude of earnings surprises on market reactions, we investigate whether the absolute amount of unexpected earnings conveyed in our sample earnings announcements changed over 1980–1999. Table 3 shows a pooled mean SRW-$|UE_{jtq}|$ of 1.4 percent of share price and a median of 0.5 percent; for Analyst-$|UE_{jtq}|$, the pooled mean and median values are 0.8 percent and 0.2 percent, respectively. Results of regressing $|UE_{jtq}|$ on $TREND_{tq}$ are reported in the bottom of Table 3. Using SRW-$|UE_{jtq}|$, we find significant (at the 0.008 level) declines in the mean and median absolute earnings surprises; results using Analyst-$|UE_{jtq}|$ show no trend in the mean, but a significant (at the 0.014 level) decline in median values.

Additional evidence on over-time changes in the distribution of $|UE_{jtq}|$ is reported in the remaining columns in Table 3 where we show the percent of earnings announcements each year with values of SRW-$|UE_{jtq}|$ in the six ranges examined by Freeman and Tse (1992): $|UE_{jtq}| \leq 0.001$, $0.001 < |UE_{jtq}| \leq 0.005$, $0.005 < |UE_{jtq}| \leq 0.01$, $0.01 < |UE_{jtq}| \leq 0.05$, $0.05 < |UE_{jtq}| \leq 0.10$, and $|UE_{jtq}| > 0.10$. For the pooled sample, these data show that 11 percent of the announcements convey the smallest amount of unexpected earnings news, whereas only 3 percent of the announcements convey the largest amount of unexpected earnings news. The modal unexpected earnings is $0.001 < |UE_{jtq}| \leq 0.005$, with 37 percent of the observations. The $|UE_{jtq}|$ distribution of our pooled sample is similar to that reported by Freeman and Tse (1992) with two exceptions. First, we find a smaller percentage of announcements in the smallest $|UE_{jtq}|$ category; 11 percent of our sample fall into the $|UE_{jtq}| \leq 0.001$ range, compared with 22 percent in Freeman and Tse (1992). Second, we find a larger percent of announcements in the $0.01 < |UE_{jtq}| \leq 0.05$ range with 25 percent vs. 19 percent in Freeman and Tse (1992). Results using Analyst-$|UE_{jtq}|$ show

TABLE 3
Distribution of Absolute Unexpected Earnings in Quarterly Earnings Announcements, 1980–1999

Year	$\lvert UE_{jtq}\rvert^a$ Mean	$\lvert UE_{jtq}\rvert^a$ Median	*Percent of Quarterly Earnings Announcements by* $\lvert UE_{jtq}\rvert^a$ *Range*					
			$\lvert UE_{jtq}\rvert \leq 0.001$	$0.001 < \lvert UE_{jtq}\rvert \leq 0.005$	$0.005 < \lvert UE_{jtq}\rvert \leq 0.01$	$0.01 < \lvert UE_{jtq}\rvert \leq 0.05$	$0.05 < \lvert UE_{jtq}\rvert \leq 0.10$	$0.1 < \lvert UE_{jtq}\rvert$
1980	0.015	0.007	5.8	31.0	23.2	34.1	3.6	2.3
1981	0.014	0.007	7.2	30.5	24.5	33.2	3.4	1.2
1982	0.018	0.009	5.3	28.3	20.6	37.6	5.4	2.8
1983	0.015	0.007	6.8	32.6	24.5	29.3	4.0	2.8
1984	0.015	0.007	6.5	33.6	22.8	30.3	4.2	2.5
1985	0.015	0.006	10.2	37.0	21.2	24.2	3.8	3.6
1986	0.015	0.005	10.9	40.1	17.8	23.3	3.9	4.0
1987	0.016	0.005	10.4	41.1	18.8	20.9	4.0	4.8
1988	0.016	0.006	8.2	38.7	20.8	24.1	4.0	4.3
1989	0.015	0.005	10.4	42.6	16.9	23.6	3.8	2.7
1990	0.014	0.005	12.8	38.7	16.4	25.3	4.2	2.6
1991	0.019	0.006	10.5	33.7	17.2	27.1	6.5	5.1
1992	0.016	0.005	12.3	37.7	18.0	23.4	4.6	4.0
1993	0.014	0.005	12.6	38.8	17.7	23.9	3.7	3.4
1994	0.012	0.004	11.8	42.2	18.9	22.4	2.3	2.4
1995	0.013	0.005	10.3	40.0	19.7	24.5	3.5	2.1
1996	0.011	0.004	11.8	43.3	19.1	20.9	3.0	1.8

(Continued on next page)

TABLE 3 (Continued)

			Percent of Quarterly Earnings Announcements by $\lvert UE_{jtq}\rvert^a$ Range					
Year	$\lvert UE_{jtq}\rvert^a$ *Mean*	$\lvert UE_{jtq}\rvert^a$ *Median*	$\lvert UE_{jtq}\rvert \le 0.001$	$0.001 < \lvert UE_{jtq}\rvert \le 0.005$	$0.005 < \lvert UE_{jtq}\rvert \le 0.01$	$0.01 < \lvert UE_{jtq}\rvert \le 0.05$	$0.05 < \lvert UE_{jtq}\rvert \le 0.10$	$0.1 < \lvert UE_{jtq}\rvert$
1997	0.011	0.004	12.5	44.7	18.2	20.2	2.6	1.8
1998	0.010	0.004	16.2	42.1	17.9	20.3	2.3	1.3
1999	0.014	0.004	13.3	39.5	16.6	23.2	4.9	2.5
All years: SRW	0.014	0.005	11.3	37.4	19.3	25.2	3.8	2.9
All years: Analyst	0.008	0.002	69.6	15.8	5.8	7.2	1.5	NA
TREND: SRW[b] (p-value)	−0.000 (0.008)	−0.000 (0.001)	0.417% (0.001)	0.615% (0.001)	−0.314% (0.001)	−0.639% (0.001)	−0.046% (0.244)	0.033% (0.456)
TREND: Analyst[b] (p-value)	0.000 (0.694)	−0.000 (0.014)	−0.534% (0.101)	0.159% (0.192)	0.039% (0.246)	−0.040% (0.487)	0.018% (0.521)	NA NA

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[a] We report mean and median values of $\lvert UE_{jtq}\rvert$ calculated across all earnings announcements in year t, as well as the percent of announcements with values of $\lvert UE_{jtq}\rvert$ in the noted range. The "All years" row reports the mean values of these numbers across the entire sample period. NA denotes the fact that none of the sample announcements had $\lvert UE_{jtq}\rvert > 0.1$ for unexpected earnings measured using analysts' forecasts.

[b] For the rows labeled "TREND: SRW" and "TREND: Analyst," we first report the slope coefficient estimates from regressing the mean and median SRW-based and analyst-based measures of $\lvert UE_{jtq}\rvert$ on $TREND_t$. p-values for these slope coefficients appear beneath each estimate. The remaining columns in these rows show the slope coefficients and related significance levels from regressing the percent of observations in each category each year on $TREND_t$.

Variable definitions:

$\lvert UE_{jtq}\rvert$ = price-scaled absolute unexpected earnings for firm j's quarter q earnings announcement made in year t.

SRW denotes unexpected earnings measured using seasonal random-walk model.

Analyst denotes unexpected earnings measured using analysts' most recent forecast of the firm's earnings for quarter t.

a larger fraction of the announcements convey small amounts of unexpected earnings news, with 70 percent of the sample announcements falling in the $|UE_{jtq}| \leq 0.001$ range of the $|UE_{jtq}|$ distribution.

Over-time trends in the frequency of observations in each range appear at the bottom of Table 3. The SRW-$|UE_{jtq}|$ results show that the frequency of earnings announcements conveying the smallest unexpected earnings news is increasing, with average increases of 0.4 percent and 0.6 percent per year (significant at the 0.001 level) in the frequency of announcements in the $|UE_{jtq}| \leq 0.001$ and $0.001 < |UE_{jtq}| \leq 0.005$ ranges. These increases come at the expense of significant declines in the two middle categories; the two largest categories show no significant trend. Together, these results suggest that the decreases in the mean and median SRW-$|UE_{jtq}|$ are driven by an increased proportion of announcements conveying the smallest amounts of unexpected earnings. With the exception of $|UE_{jtq}| \leq 0.001$, where we find a decline in frequency (significant at the 0.10 level), the results using Analyst-$|UE_{jtq}|$ show no evidence of trends in any category.

The analyses in Table 3 are silent on the cause of the shift in the distribution of $|UE_{jtq}|$. Building on Freeman and Tse's (1992) premise that the magnitude of the absolute value of unexpected earnings reflects the degree of earnings persistence (that is, the greater the earnings persistence, the smaller the absolute value of unexpected earnings), we arrive at the following conjecture. To the extent the shift toward greater frequencies of smaller unexpected earnings is due to an increase in the proportion of earnings changes that are mostly persistent, we expect to see concomitant increases in ERCs (because the density of observations on the more steeply sloped portion of the return-earnings relation has increased). However, if shifts in the distribution of unexpected earnings are due to over-time increases in the intensity and pervasiveness of earnings management, then smaller values of $|UE_{jtq}|$ might contain more transitory components. Evidence roughly consistent with this latter conjecture is provided by research that documents higher-than-expected frequencies of reported earnings just slightly above zero, consistent with earnings management to avoid losses (e.g., Burgstahler and Dichev 1997). To the extent the shift toward smaller values of unexpected earnings stems from earnings management, we would not expect an increase in ERCs. We probe this issue in the next section, where we test for a relation between over-time changes in ERCs and over-time changes in the $|UE_{jtq}|$ distribution.

ERCs Partitioned by Absolute Unexpected Earnings

As a basis for analyzing over-time changes in ERCs, Table 4 reports the coefficient estimates for the returns-earnings regression (1) for each year, as well as the trend in these coefficient estimates, as measured by α_2 in Equation (3):

$$AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \alpha_2 UE_{jtq} \times TREND_{tq} + \xi_{jtq}. \qquad (3)$$

The full sample results reported in the first column of Table 4, Panel A are the same as those reported in Table 2 and show a pooled sample SRW-ERC of 0.215. Results of estimating Equation (3) reported in Panel B show a *negative* estimate of the $UE_{jtq} \times TREND_{tq}$ interaction, α_2, using either SRW-UE or Analyst-UE. Specifically, the SRW-ERC declined at the rate of 0.003 per year (significant at the 0.068 level), while the Analyst-ERC declined at the rate of 0.013 per year (significant at the 0.001 level). These declines in short-window ERCs are consistent with declines in long-window ERCs reported by Lev and Zarowin (1999, Table 1) and Cheng et al. (1992, Table 1). However, this result does not support our second possible explanation—that the intensity of investors' reactions per unit of unexpected earnings increased over time.

TABLE 4
Earnings Response Coefficients Partitioned by Levels of Absolute Unexpected Earnings, 1980–1999

Panel A: Earnings Response Coefficients Partitioned by Amount of Absolute Unexpected Earnings

Year[a]	*Full Sample*	$\lvert UE_{jtq}\rvert \le 0.001$	$0.001 < \lvert UE_{jtq}\rvert \le 0.005$	$0.005 < \lvert UE_{jtq}\rvert \le 0.01$	$0.01 < \lvert UE_{jtq}\rvert \le 0.05$	$0.05 < \lvert UE_{jtq}\rvert \le 0.10$	$0.1 < \lvert UE_{jtq}\rvert$
1980	0.265	10.584	1.514	0.686	0.590	0.203	0.142
1981	0.383	2.329	1.661	1.271	0.630	0.396	0.040
1982	0.317	2.008	1.550	1.053	0.587	0.246	0.216
1983	0.207	−3.370	0.967	0.591	0.550	0.504	−0.003
1984	0.170	0.175	1.455	1.168	0.394	0.116	0.064
1985	0.173	1.487	1.761	0.873	0.396	0.137	0.108
1986	0.256	−0.369	1.630	1.724	0.513	0.254	0.187
1987	0.153	2.461	1.928	1.392	0.358	0.203	0.046
1988	0.143	−2.743	1.289	0.438	0.473	0.201	0.044
1989	0.194	−2.082	0.920	0.800	0.430	0.235	0.076
1990	0.078	−2.308	1.635	1.095	0.688	0.310	−0.246
1991	0.233	−1.681	2.574	1.429	0.457	0.398	0.139

(Continued on next page)

TABLE 4 (Continued)

Panel A: Earnings Response Coefficients Partitioned by Amount of Absolute Unexpected Earnings

Year[a]	*Full Sample*	$\lvert UE_{jtq} \rvert \leq 0.001$	$0.001 < \lvert UE_{jtq} \rvert \leq 0.005$	$0.005 < \lvert UE_{jtq} \rvert \leq 0.01$	$0.01 < \lvert UE_{jtq} \rvert \leq 0.05$	$0.05 < \lvert UE_{jtq} \rvert \leq 0.10$	$0.1 < \lvert UE_{jtq} \rvert$
1992	0.334	−1.608	2.166	1.301	0.373	0.385	0.270
1993	0.219	0.093	1.201	1.269	0.450	0.356	0.090
1994	0.228	2.736	1.877	0.772	0.352	0.215	0.173
1995	0.316	−8.617	1.245	0.497	0.284	0.214	0.357
1996	0.110	0.310	1.554	0.305	0.307	−0.025	0.071
1997	0.192	3.954	1.162	0.769	0.345	0.066	0.133
1998	0.172	−6.451	0.640	0.544	0.653	−0.001	0.074
1999	0.181	3.803	0.918	0.716	0.433	0.236	0.048
All years: SRW (p-value)	0.215 (0.001)	−0.159 (0.883)	1.438 (0.001)	0.885 (0.001)	0.469 (0.001)	0.238 (0.001)	0.108 (0.001)
All years: Analyst (p-value)	0.248 (0.001)	6.873 (0.004)	2.523 (0.001)	1.215 (0.001)	0.358 (0.001)	0.159 (0.001)	NA NA

(Continued on next page)

TABLE 4 (Continued)

Panel B: Regression Results[b]

$AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \alpha_2 UE_{jtq} \times TREND_{tq} + \xi_{jtq}$

	Full Sample	$\lvert UE_{jtq}\rvert \le 0.001$	$0.001 < \lvert UE_{jtq}\rvert \le 0.005$	$0.005 < \lvert UE_{jtq}\rvert \le 0.01$	$0.01 < \lvert UE_{jtq}\rvert \le 0.05$	$0.05 < \lvert UE_{jtq}\rvert \le 0.10$	$0.1 < \lvert UE_{jtq}\rvert$
SRW-UE							
Intercept	0.003	−0.001	−0.000	0.003	0.004	0.006	0.010
(p-value)	(0.001)	(0.113)	(0.531)	(0.001)	(0.001)	(0.001)	(0.001)
UE	0.243	0.224	1.127	0.927	0.556	0.295	0.088
(p-value)	(0.001)	(0.927)	(0.001)	(0.001)	(0.001)	(0.001)	(0.048)
UE × TREND	−0.003	−0.030	0.029	−0.004	−0.009	−0.006	0.002
(p-value)	(0.068)	(0.861)	(0.117)	(0.691)	(0.013)	(0.163)	(0.634)
Analyst-UE							
Intercept	0.003	0.001	0.003	0.004	0.004	0.007	NA
(p-value)	(0.001)	(0.431)	(0.001)	(0.001)	(0.001)	(0.014)	NA
UE	0.390	4.875	2.129	1.010	0.422	0.343	NA
(p-value)	(0.001)	(0.458)	(0.001)	(0.001)	(0.001)	(0.001)	NA
UE × TREND	−0.013	0.156	0.035	0.019	−0.006	−0.016	NA
(p-value)	(0.001)	(0.745)	(0.326)	(0.401)	(0.393)	(0.018)	NA

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[a] For the noted category of SRW unexpected earnings, Panel A reports the earnings response coefficient (ERC) for yearly and pooled estimations of $AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \xi_{jtq}$. We also report the pooled ERC using the analyst-based measure of unexpected earnings.

[b] Panel B reports the results of estimating the following regression for the noted category of unexpected earnings:

$$AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \alpha_2 UE_{jtq} \times TREND_{tq} + \xi_{jtq}.$$

Results are reported separately for SRW and analyst-based measures of unexpected earnings.

Variable definitions:

AR_{jtq} = the signed abnormal return to firm j's quarter q earnings announcement made in year t;

$stdAR_{jtq}$, = AR_{jtq} divided by the standard deviation of firm j's signed abnormal returns on all trading days in year t; and

UE_{jtq} = price-scaled unexpected earnings conveyed by firm j's quarter q earnings announcement made in year t.

SRW denotes unexpected earnings measured using seasonal random walk model.

Analyst denotes unexpected earnings measured using analysts' most recent forecast of the firm's earnings for quarter t.

Previous research (e.g., Freeman and Tse 1992) suggests that extreme (positive and negative) values of unexpected earnings are less persistent than moderate values, so that moderate values of UE_{jtq}, rather than extreme (positive or negative) values of UE_{jtq}, are associated with larger ERCs. We test this result for our sample by estimating ERCs for each of the six categories of $|UE_{jtq}|$. The results, displayed in the remaining columns of Table 4, Panel A, generally show an increase in SRW-ERCs as we move from extreme to modest earnings surprises: the average SRW-ERC is 0.11 for $|UE_{jtq}| > 0.10$, 0.24 for $0.05 < |UE_{jtq}| \leq 0.10$, 0.47 for $0.01 < |UE_{jtq}| \leq 0.05$, 0.89 for $0.005 < |UE_{jtq}| \leq 0.01$, and 1.44 for $0.001 < |UE_{jtq}| \leq 0.005$. The exception is the SRW-ERC for the smallest $|UE_{jtq}|$ category, where the results show no significant association between abnormal returns and unexpected earnings. Analyst-ERCs, reported in the bottom two rows of Panel A of Table 4, show a pattern of monotonically increasing coefficients (consistent with Freeman and Tse's [1992] results) as we move from the largest to the smallest earnings surprises. In summary, with the exception of no relation found for the smallest SRW-$|UE_{jtq}|$,[8] the pattern of ERCs for our sample firms is similar to that documented in prior studies.

More importantly, given our research question, tests of trends in ERCs partitioned by absolute unexpected earnings show no evidence that the ERC *for a given amount of unexpected earnings* increased over time. Panel B of Table 4 shows that the only statistically significant trends in $|UE_{jtq}|$ are downward. The 0.009 annual decline (significant at the 0.013 level) in the SRW-ERC for the $0.01 < |UE_{jtq}| \leq 0.05$ range and the 0.016 annual decline (significant at the 0.018 level) in the Analyst-ERC for the $0.05 < |UE_{jtq}| \leq 0.10$ category are inconsistent with our second possible explanation for the documented increase in market reactions to earnings announcements; there is no evidence that the intensity of investors' response to a unit of $|UE_{jtq}|$ has increased over time.

In summary, the results in Panel B of Table 4 show significant declines in earnings response coefficients for the sample as a whole. Table 3 shows that for the SRW-based tests, this result may be in part attributable to shifts in the SRW-$|UE_{jtq}|$ distribution to the region of weakest relation, SRW-$|UE_{jtq}| \leq 0.001$, where the ERC is statistically indistinguishable from zero. However, there is an offsetting shift in the SRW-$|UE_{jtq}|$ distribution to the region of strongest relation, $0.001 < |UE_{jtq}| \leq 0.005$. For the analyst-based tests, Table 3 reveals some evidence of a shift in the distribution of Analyst-$|UE_{jtq}|$ *from* the region where the unexpected returns-unexpected earnings relation is strongest; in our sample, the region of strongest relation occurs for Analyst-$|UE_{jtq}| \leq 0.001$, where the ERC is 6.87. For both measures of unexpected earnings, there is limited evidence of a decline in the ERC holding constant the magnitude of unexpected earnings: for SRW tests, the decline occurs for announcements with $0.01 < |UE_{jtq}| \leq 0.05$; for Analyst tests, the decline occurs for announcements with $0.05 < |UE_{jtq}| \leq 0.10$. Importantly, none of these results is consistent with the either the first or second possible explanation (i.e., increases in the amount of unexpected earnings news or increases in investors' reaction to a unit of unexpected earnings) for the increasing trend in market reactions to earnings announcements.

Sensitivity Tests

We perform three sensitivity checks on the results reported in Table 4. Our first test is based on Basu's (1997) finding that the coefficient relating two-month abnormal returns to the change in annual earnings is significantly larger for positive earnings surprises than for negative earnings surprises. We examine whether the frequency of negative earnings news has declined over our sample period and, if so, whether such a decline, coupled with an asymmetric response to good vs. bad news, contributes to explaining the increasing trend in market reactions. Across all sample years, the mean frequency of negative SRW forecast errors (untabulated) is about 35 percent, ranging from 23 percent in 1984 to 50 percent in 1991. There is no trend in the proportion of negative (or, therefore, positive) SRW forecast errors over 1980–1999. Consistent with Basu's (1997) results, Table 5, Panel A shows significantly smaller ERCs when UE < 0 than when UE > 0 for both SRW-ERCs and

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[8] Because our focus is on the effects, not the causes, of nonlinearities in ERCs, we do not probe the flat relation found for the smallest SRW-UE measure. One possible reason for the difference between this result and Freeman and Tse's (1992) finding is differences in samples: our sample covers 1980–1999 vs. third quarter 1984 to third quarter 1987 for Freeman and Tse (1992).

TABLE 5

Sensitivity Analysis of Factors Influencing Earnings Response Coefficients (ERC)

Panel A: ERCs Conditioned on the Sign of Unexpected Earnings[a]

	$UE_{jtq} < 0$		$UE_{jtq} > 0$	
Pooled[b]	*ERC*	*(p-value)*	*ERC*	*(p-value)*
All years: SRW	0.044	(0.002)	0.220	(0.001)
All years: Analyst	0.062	(0.010)	0.240	(0.001)
SRW-UE[c]	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*
Intercept	0.005	(0.001)	0.005	(0.001)
UE	0.090	(0.001)	0.238	(0.001)
UE × TREND	−0.005	(0.030)	−0.002	(0.407)
Analyst-UE[c]				
Intercept	−0.003	(0.001)	0.007	(0.001)
UE	0.256	(0.001)	0.199	(0.025)
UE × TREND	−0.017	(0.001)	0.004	(0.607)

Panel B: ERCs Conditioned on Profit or Loss[a]

	Losses		*Profits*	
Pooled[b]	*ERC*	*(p-value)*	*ERC*	*(p-value)*
All years: SRW	0.136	(0.001)	0.473	(0.001)
All years: Analyst	0.203	(0.001)	0.327	(0.001)
SRW-UE[c]	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*
Intercept	0.002	(0.024)	0.003	(0.001)
UE	0.097	(0.001)	0.549	(0.001)
UE × TREND	0.004	(0.125)	−0.008	(0.001)
Analyst-UE[c]				
Intercept	0.003	(0.007)	0.003	(0.001)
UE	0.367	(0.001)	0.414	(0.001)
UE × TREND	−0.015	(0.017)	−0.008	(0.151)

(Continued on next page)

TABLE 5 (Continued)

Panel C: ERCs Holding Constant Both the Firm and the Magnitude of Unexpected Earnings[a]

	$0.001 < \lvert UE_{jtq}\rvert \le 0.005$ (n = 241 firms)		$0.005 < \lvert UE_{jtq}\rvert \le 0.01$ (n = 47 firms)		$0.01 < \lvert UE_{jtq}\rvert \le 0.05$ (n = 119 firms)	
Pooled[b]	*ERC*	*(p-value)*	*ERC*	*(p-value)*	*ERC*	*(p-value)*
All years: SRW	1.262	(0.001)	0.898	(0.001)	0.462	(0.001)
SRW-UE[c]	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*
Intercept	0.000	(0.838)	0.003	0.016)	0.003	(0.001)
UE	0.733	(0.025)	0.525	(0.126)	0.470	(0.001)
UE × TREND	0.052	(0.056)	0.036	(0.212)	−0.001	(0.917)

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[a] We estimate earnings response coefficients (ERCs) for three partitions of the sample. Panel A reports results conditioning on the sign of unexpected earnings; Panel B reports results conditioning on whether the earnings announcement is a loss observation (following Lipe et al. [1998], a loss observation is defined as a loss in either quarter q or q − 4); Panel C examines three categories of absolute unexpected earnings where we hold the firm constant.

[b] The pooled results in each panel show the earnings response coefficient (ERC) and significance level from the pooled regression: $AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \xi_{jtq}$ (using the SRW measure of UE_{jtq}) for the noted partitions of unexpected earnings. Panels A and B also report results for the analyst-based measure of unexpected earnings (an insufficient number of forecasts precludes estimating the analyst-based unexpected earnings regression for the Panel C partitions).

[c] We report the coefficient estimates and p-values from estimating the following regression for the noted partition of unexpected earnings variable: $AR_{jtq} = \alpha_0 + \alpha_1 UE_{jtq} + \alpha_2 UE_{jtq} \times TREND_{tq} + \xi_{jtq}$.

Analyst-ERCs. However, tests of trends in the ERCs show no pattern in ERCs for positive observations and a significant (at the 0.03 level or better) *decline* in ERCs for negative observations.

Our second sensitivity check draws on Hayn (1995), Lipe et al. (1998), and Butler's (2000) finding that investors react less to earnings and/or earnings surprises involving losses than profits. The mean frequency of loss announcements in our sample is 15.6 percent, and ranges from 5.4 percent in 1980 to 21.6 percent in 1992.[9] An untabulated regression of the frequency of quarterly losses on a trend variable shows a significant (at the 0.02 level) increase in the frequency of losses for our sample. Consistent with prior studies, Table 5, Panel B shows that the pooled ERC for the Loss sample is smaller than the ERC for the Profit sample (SRW-ERC of 0.14 vs. 0.47, and Analyst-ERC of 0.20 vs. 0.33, differences significant at the 0.001 level using two sample t-tests). We find little evidence of over-time increases in the ERCs for Loss or for Profit firms, and some evidence of decreases. The results of estimating Equation (3) show a positive coefficient on the UE × TREND interaction term only for the SRW-Loss subsample, where the coefficient is 0.0037, not significant at conventional levels (p = 0.125). The UE × TREND interaction is negative for the other three subsamples at significance levels ranging from 0.151 (the Analyst-Profit subsample) to 0.001 (the SRW-Profit subsample).

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[9] We adopt the classification rules for loss observations developed by Lipe et al. (1998), in order to facilitate comparisons with that study and with others (e.g., Hayn 1995) who have investigated the earnings-return relation when earnings is negative. The Lipe et al. (1998) classification rule designates unexpected SRW earnings as a loss observation if earnings is negative in either quarter t or quarter t − 4. As Lipe et al. (1998, 200) point out, this classification rule is consistent with the use of the SRW model.

Our final sensitivity check controls for across-year differences in the sample firms represented *in a given* $|UE_{jtq}|$ *category* (e.g., announcements with $|UE_{jtq}| > 0.10$ used to estimate the Table 4 ERC of 0.14 in 1980 are not made by the same firms whose $|UE_{jtq}| > 0.10$ announcements are used to estimate the ERC of 0.04 in 1981). In this analysis, we repeat the tests in Table 4 holding constant *both* the $|UE_{jtq}|$ category *and* the firm; these controls permit a focus on changes in ERCs driven by over-time changes in the determinants of ERCs reported in Table 1—beta, persistence, and risk-free rates. To be included in these tests, we require a firm to have at least one earnings announcement, in each of at least 15 years, in the same SRW-$|UE_{jtq}|$ interval.[10] Three intervals—$0.001 < |UE_{jtq}| \leq 0.005$, $0.005 < |UE_{jtq}| \leq 0.01$, and $0.01 < |UE_{jtq}| \leq 0.05$—contain 241, 47, and 119 firms, respectively, that meet these criteria. For each of these $|UE_{jtq}|$ intervals, we estimate the returns-unexpected earnings regression, Equation (1), for the pooled sample. The results, reported in Panel C of Table 5, show similar magnitudes of SRW-ERCs for the three categories as reported for the full sample in Table 4. Investigation of the trends in the ERCs for these categories reveals a significant (at the 0.056 level) increase in the ERC for only one category, $0.001 < |UE_{jtq}| \leq 0.005$; however, the magnitude of the increase is not large enough or sufficiently pervasive across the sample to offset either the declines or no changes in ERCs documented for other partitions in Table 4, Panel B.

In summary, the results in Table 5 generally support those in Table 4 that show that ERCs have declined over 1980–1999 for the sample as a whole. The overall decline in ERCs and shifts in the $|UE_{jtq}|$ distribution toward smaller earnings surprises move *opposite* to the increases in absolute market reactions to earnings announcements documented in Section II. That is, increases in the usefulness of earnings announcements occurred *despite* declines in the magnitude of unexpected earnings and declines in the intensity of investors' reactions to a unit of unexpected earnings as measured by earnings response coefficients. We conclude that the increased usefulness of earnings announcements must be due to something other than the unexpected earnings content of these announcements or the relation between returns and unexpected earnings.

IV. OVER-TIME CHANGES IN CONCURRENT INFORMATION IN EARNINGS ANNOUNCEMENTS

In this section we investigate whether the amount of concurrent information disclosed in the press releases of quarterly earnings announcements increased over time, and if so, whether these increases explain the increased usefulness of earnings announcements. We randomly select 30 firms (from our 426 sample firms) with at least one earnings announcement press release in the Lexis-Nexis newswire archives for *each* calendar year t = 1980–1999. For each of these 30 firms, we collect *all* earnings announcement press releases made during calendar years t = 1980–1999. In total, there are 2,190 earnings announcement press releases, an average of about 73 per firm (or 3.65 per firm-year). We read and code each press release, using the coding scheme described in the Appendix.

Before turning to the investigation of concurrent disclosures and their ability to explain the increased usefulness of earnings announcements, we first examine whether this subsample of 30 firms exhibits the upward trend in the abnormal return metrics documented for the full sample. Untabulated analyses reveal that the subsample has a mean $|AR_{jtq}|$ of 3.15 percent and an upward trend in $|AR_{jtq}|$ of 0.06 percent per year (comparable to 3.08 percent and 0.04 percent, respectively, for the full sample). The subsample mean std$|AR_{jtq}|$ is 2.51

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[10] We use SRW − $|UE_{jtq}|$ because too few firms have a sufficiently long time-series of announcements with homogeneous values of Analyst − $|UE_{jtq}|$.

and the upward trend in std$|AR_{jtq}|$ is 0.04 per year (comparable to 2.42 and 0.026, respectively, for the full sample). Both upward trends are significant at the 0.001 level, and neither trend is explained by firm size or over-time changes in firm size.

Table 6 displays descriptive information on the sample firms' concurrent disclosures in their earnings announcement press releases. This table summarizes the mean number of pieces of information in three main categories, and the frequency with which five other pieces of information appear in the press releases. The three main categories are nonrecurring income components (e.g., discontinued operations, asset impairments, restructuring charges); current operating data (e.g., shipments, R&D spending, margins); and forecast operating data. The five pieces of information whose frequencies appear in Table 6 are detailed income statements, detailed balance sheets, detailed cash flow statements, officer comments about current periods, and officer comments about future periods.

As indicated in the second column of Table 6, earnings announcement press releases grew steadily in length, from an average of 517 words in 1980 to an average of 2,427 words in 1999 (the average increase is 92 words per year, significant at the 0.001 level). Concurrently, the average number of nonrecurring income components and the average number of pieces of current operating data also grew, with trends significant at the 0.001 level. Specifically, the mean number of nonrecurring income components was at a low of about 0.23 per announcement in 1980, increasing nonmonotonically to about 0.52 in 1999; the average number of current operating items increased nonmonotonically from 1.48 in 1980 to 2.04 in 1999. We observe no significant trend in the number of forecast operating items.

Disclosure of detailed income statements increases monotonically from 8.5 percent of the sample announcements in 1980 to 77.8 percent in 1999. The increase in frequency of 4 percent per year is significant at the 0.001 level. In contrast, detailed balance sheets appear in none of the sample press releases until 1984, but the frequency increases significantly (but not monotonically) thereafter. Balance sheets appear more frequently than do cash flow statements, but never more than about one-third of the time. Finally, qualitative comments by officers about the current period appeared in only 23 percent of the press releases in 1980, with the frequency increasing to over 50 percent by 1999; the trend is significant at the 0.001 level. Officers' prospective qualitative comments are somewhat less frequent (33 percent for the pooled sample) than are comments about the current period (37 percent), and exhibit no reliable trend.

Table 6 indicates that over 1980–1999, earnings announcement press releases exhibited a material increase in the inclusion of information about nonrecurring income components, about operations, and about detailed financial statements. Our next set of tests examines the statistical association between each type of concurrent disclosure and the market responses to the press releases. For these tests, we measure the type and amount of concurrent news, as indicated below:

$|UE_{jtq}|$ = absolute value of firm j's unexpected earnings, measured as reported earnings for quarter q less earnings for quarter $q - 4$, scaled by market value of equity at the end of quarter $q - 1$. (Similar inferences are obtained using Analyst-$|UE_{jtq}|$ and are not reported);

$|US_{jtq}|$ = absolute value of firm j's unexpected sales, measured as reported sales for quarter q less sales for quarter $q - 4$, scaled by the market value of equity at the end of quarter $q - 1$;

IS_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly income statement, 0 otherwise;

TABLE 6
Descriptive Information on Concurrent Disclosures Made with Quarterly Earnings Announcements for a Sample of 30 Firms for which Compustat Reports Earnings Announcement Dates for All Quarters, 1980–1999

					Percent of Press Releases Containing				
Year	*Mean # Words*	*Mean # of Nonrecurring Earnings Components*	*Mean # of Pieces of Current Operating Data*	*Mean # of Pieces of Forecast Operating Data*	*Detailed Income Statement*	*Detailed Balance Sheet*	*Detailed Cash Flow Statement*	*Officer Comments Current*	*Officer Comments Forecast*
1980	517	0.23	1.48	0.22	8.5	0.0	0.0	23.4	27.7
1981	761	0.36	1.39	0.26	11.2	0.0	0.0	35.7	49.0
1982	700	0.31	1.49	0.20	11.8	0.0	0.9	31.8	41.8
1983	837	0.26	1.38	0.18	11.8	0.0	0.0	31.9	35.3
1984	794	0.28	1.34	0.17	13.0	0.9	0.9	35.7	32.2
1985	828	0.38	1.56	0.16	16.1	0.9	0.0	23.2	31.3
1986	879	0.47	1.25	0.14	20.8	3.0	1.0	29.7	28.7
1987	930	0.48	1.43	0.20	21.9	6.7	0.0	35.2	39.0
1988	1,010	0.46	1.41	0.12	31.0	6.9	0.0	35.3	32.8
1989	1,159	0.39	1.70	0.13	34.0	11.3	0.0	33.0	22.6
1990	1,213	0.33	1.60	0.11	41.1	11.2	0.9	34.6	24.3
1991	1,369	0.32	1.56	0.16	42.6	17.4	2.6	36.5	35.7
1992	1,382	0.44	1.73	0.17	44.6	15.2	2.7	38.4	33.0
1993	1,561	0.56	1.74	0.14	47.4	16.7	2.6	39.5	36.0
1994	1,495	0.33	1.81	0.15	53.1	16.8	1.8	43.4	32.7

(*Continued on next page*)

TABLE 6 (Continued)

					Percent of Press Releases Containing				
Year	*Mean # Words*	*Mean # of Nonrecurring Earnings Components*	*Mean # of Pieces of Current Operating Data*	*Mean # of Pieces of Forecast Operating Data*	*Detailed Income Statement*	*Detailed Balance Sheet*	*Detailed Cash Flow Statement*	*Officer Comments Current*	*Officer Comments Forecast*
1995	1,795	0.33	1.81	0.23	64.0	28.8	4.5	45.0	29.7
1996	1,943	0.40	1.82	0.22	67.3	27.4	8.8	44.2	38.1
1997	2,086	0.51	1.91	0.23	72.2	32.4	9.3	44.4	28.7
1998	2,259	0.55	1.83	0.30	74.3	28.3	8.0	44.2	33.6
1999	2,427	0.52	2.04	0.26	77.8	25.9	7.4	51.9	30.6
All years	1,297	0.39	1.61	0.19	38.2	12.5	2.6	36.9	33.1
TREND (p-value)	92 (0.001)	0.01 (0.001)	0.03 (0.001)	0.00 (0.176)	4.0% (0.001)	1.9% (0.001)	0.5% (0.001)	1.1% (0.001)	−0.3% (0.146)

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[a] Results in this table are based on 2,190 earnings announcement press releases made by 30 firms randomly selected from the full sample of 426 firms.
Variable definitions: See Appendix.

BS_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly balance sheet, 0 otherwise;

SCF_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly statement of cash flows, 0 otherwise;

$EARN\_COMP_{jtq}$ = the number of types of nonrecurring income components explicitly mentioned in the text of firm j's press release for quarter q of year t; $EARN\_COMP_{jtq}$ ranges between 0 (no types provided) and 11 (the maximum number of earnings component types we gathered);

$GOOD\_CURR_{jtq}$ = the number of operating statements and of officer qualitative comments about the current quarter that convey good news;

BAD_CURR_{jtq} = the number of operating statements and of officer qualitative comments about the current quarter that convey bad news;

$GOOD\_FORE_{jtq}$ = the number of operating statements and of officer qualitative comments about a future period that convey good news; and

BAD_FORE_{jtq} = the number of operating statements and of officer qualitative comments about a future period that convey bad news.

Panel A, Table 7 reports the results of univariate regressions of $|AR_{jtq}|$ on each of these variables. With the exception of $|US_{jtq}|$ and $EARN\_COMP_{jtq}$, all variables are positively

TABLE 7
The Association between Absolute Abnormal Returns to Press Releases and the Unexpected Earnings and Concurrent Information in the Press Releases, 1980–1999[a]

Panel A: Regressions of Absolute Abnormal Returns on Unexpected Earnings and Concurrent Information

	Univariate Regressions[b]			*Multiple Regression*[b]	
	Coefficient Estimate	*(p-value)*	*R^2*	*Coefficient Estimate*	*(p-value)*
Intercept	—	—	—	0.023	(0.001)
$\|UE_{jtq}\|$	0.237	(0.001)	0.021	0.238	(0.001)
$\|US_{jtq}\|$	0.006	(0.364)	0.000	−0.004	(0.605)
IS_{jtq}	0.006	(0.001)	0.012	0.006	(0.001)
BS_{jtq}	0.007	(0.001)	0.006	0.004	(0.166)
SCF_{jtq}	0.017	(0.001)	0.008	0.011	(0.007)
$EARN\_COMP_{jtq}$	0.001	(0.277)	0.001	−0.000	(0.746)
$GOOD\_CURR_{jtq}$	0.001	(0.004)	0.004	0.001	(0.019)
BAD_CURR_{jtq}	0.003	(0.001)	0.005	0.002	(0.031)
$GOOD\_FORE_{jtq}$	0.003	(0.001)	0.005	0.003	(0.001)
BAD_FORE_{jtq}	0.017	(0.001)	0.014	0.016	(0.001)
Adjusted R^2	—	—	—	0.065	(0.001)

(*Continued on next page*)

TABLE 7 (Continued)

Panel B: Trends in the Association between Absolute Abnormal Returns and Unexpected Earnings and Concurrent Information

	Model 1^c		*Model 2*^{c}		*Model 3*^{c}	
Independent Variable	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*	*Coefficient Estimate*	*(p-value)*
Intercept	0.025	(0.001)	0.022	(0.001)	0.023	(0.001)
$TREND_t$	0.001	(0.001)	0.001	(0.001)	0.000	(0.994)
$\lvert UE_{jtq}\rvert$	—	—	0.248	(0.001)	0.174	(0.019)
$\lvert UE_{jtq}\rvert \times TREND_t$	—	—	0.001	(0.933)	0.005	(0.416)
$\lvert US_{jtq}\rvert$	—	—	—	—	0.028	(0.042)
IS_{jtq}	—	—	—	—	−0.015	(0.001)
BS_{jtq}	—	—	—	—	0.009	(0.271)
SCF_{jtq}	—	—	—	—	0.045	(0.014)
$EARN\_COMP_{jtq}$	—	—	—	—	−0.001	(0.586)
$GOOD\_CURR_{jtq}$	—	—	—	—	0.000	(0.881)
BAD_CURR_{jtq}	—	—	—	—	0.002	(0.423)
$GOOD\_FORE_{jtq}$	—	—	—	—	0.003	(0.109)
BAD_FORE_{jtq}	—	—	—	—	0.004	(0.446)
$\lvert US_{jtq}\rvert \times TREND_t$	—	—	—	—	−0.003	(0.007)
$IS_{jtq} \times TREND_t$	—	—	—	—	0.002	(0.001)
$BS_{jtq} \times TREND_t$	—	—	—	—	−0.001	(0.304)
$SCF_{jtq} \times TREND_t$	—	—	—	—	−0.002	(0.077)
$EARN\_COMP_{jtq} \times TREND_t$	—	—	—	—	0.000	(0.569)
$GOOD\_CURR_{jtq} \times TREND_t$	—	—	—	—	0.000	(0.428)
$BAD\_CURR_{jtq} \times TREND_t$	—	—	—	—	0.000	(0.769)
$GOOD\_FORE_{jtq} \times TREND_t$	—	—	—	—	0.000	(0.870)
$BAD\_FORE_{jtq} \times TREND_t$	—	—	—	—	0.002	(0.001)
Adjusted R^2	0.015	(0.0001)	0.039	(0.001)	0.096	(0.001)

(Continued on next page)

TABLE 7 (Continued)

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[a] Results in this table are based on 2,190 earnings announcement press releases made by 30 (of the 426) sample firms.

[b] The columns labeled "Univariate Regressions" in Panel A report the results of regressing $|AR_{jtq}|$ on $|UE_{jtq}|$ or on the concurrent disclosure variables. (The univariate regressions include intercepts, but their values are not tabulated.) The columns labeled "Multiple Regression" show results of regressing $|AR_{jtq}|$ on all of the variables.

[c] We report the results of estimating Models 1–3:

$$\text{Model 1: } AR_{jtq} = \rho_0 + \rho_1 \text{TREND}_t + \psi_{jtq}$$

$$\text{Model 2: } |AR_{jtq}| = \rho_0 + \rho_1 \text{TREND}_t + \rho_2 |UE_{jtq}| + \rho_3 |UE_{jtq}| \times \text{TREND}_t + \zeta_{jtq}$$

$$\text{Model 3: } |AR_{jtq}| = \rho_0 + \rho_1 \text{TREND}_t + \rho_2 |UE_{jtq}| + \rho_3 |UE_{jtq}| \times \text{TREND}_t + \sum_{k=1}^{9} \psi_k \text{CONC\_DISC}^k_{jtq} + \sum_{k=1}^{9} \theta_k \text{CONC\_DISC}^k_{jtq} \times \text{TREND}_t + \zeta_{jtq}$$

where CONC_DISC^k_{jtq} = vector of the k = 9 concurrent disclosure variables defined in the Appendix.

Variable definitions:

$|AR_{jtq}|$ = the absolute abnormal return to firm j's press release announcing quarter q earnings for year t;

$|UE_{jtq}|$ = difference between firm j's reported earnings for quarter q year t and earnings for quarter q – 4, scaled by market value of equity at the end of the quarter q – 1;

$|US_{jtq}|$ = difference between firm j's reported sales for quarter q and sales for quarter q – 4, scaled by market value of equity at the end of quarter q – 1; and

TREND_t = t – 1979.

Concurrent disclosure variables are defined in the Appendix.

associated (at the 0.004 level or better) with $|AR_{jtq}|$. R^2s indicate that disclosures of detailed income statements and of bad news forecasts explain 1.2 percent and 1.4 percent, respectively, of the variation in market reactions; this compares to 2.1 percent explanatory power provided by $|UE_{jtq}|$.

The adjusted R^2 of a multiple regression including all variables is 6.5 percent—a threefold increase over the 2.1 percent from the simple regression of $|AR_{jtq}|$ on $|UE_{jtq}|$ only. We conclude from these results that several types of concurrent disclosures contribute materially to investors' responses to earnings announcement press releases—specifically, the presence of detailed income statements and statements of cash flows as well as our four composite variables for operating statements and comments by officers about both current and forecast operations.

Together with the increasing incidence of concurrent disclosures reported in Table 6, these findings suggest that the increasing reaction to earnings announcements stems from the increasing tendency of earnings announcement press releases to include other information. We test this hypothesis in Panel B, Table 7, where we report the OLS coefficient estimates and significance levels associated with equations (4)–(6):

$$\text{Model 1: } |AR_{jtq}| = \rho_0 + \rho_1 \text{TREND}_t + \psi_{jtq} \tag{4}$$

$$\text{Model 2: } |AR_{jtq}| = \rho_0 + \rho_1 \text{TREND}_t + \rho_2 |UE_{jtq}| + \rho_3 |UE_{jtq}| \times \text{TREND}_t + \zeta_{jtq} \tag{5}$$

$$\text{Model 3:}\quad |AR_{jtq}| = \rho_0 + \rho_1 TREND_t + \rho_2|UE_{jtq}| + \rho_3|UE_{jtq}| \times TREND_t + \sum_{k=1}^{9} \psi_k CONC\_DISC^k_{jtq} + \sum_{k=1}^{9} \theta_k CONC\_DISC^k_{jtq} \times TREND_t + \xi_{jtq} \tag{6}$$

where $CONC\_DISC^k_{jtq}$ = vector of the k = 9 concurrent disclosure variables previously defined.

Our test of whether concurrent information explains the trend in market reactions focuses on whether the coefficient on $TREND_t$, ρ_1, becomes insignificant when we add concurrent information independent variables. Comparing ρ_1 across Models 1 and 2 reveals that including $|UE_{jtq}|$ and $|UE_{jtq}| \times TREND_t$ does not eliminate the increasing trend in $|AR_{jtq}|$, and the estimate of ρ_1 remains significant at the 0.001 level. Comparing the estimates of ρ_1 in Models 2 and 3 shows the effect of including concurrent disclosures, $CONC\_DISC^k_{jtq}$, and the interaction of concurrent disclosures with trend, $CONC\_DISC^k_{jtq} \times TREND_t$. $CONC\_DISC^k_{jtq}$ captures how the presence and/or amount of concurrent disclosures affects market reactions, while $CONC\_DISC^k_{jtq} \times TREND_t$ captures over-time changes in investors' responses to concurrent disclosures. We note first that the concurrent disclosure variables add significantly to explaining market reactions, as evidenced by the increase in R^2s from 3.9 percent (Model 2) to 9.6 percent (Model 3). More importantly, ρ_1 in Model 3 is indistinguishable from zero, indicating that the concurrent disclosure variables eliminate the trend in $|AR_{jtq}|$. Because the concurrent disclosure measures are correlated, we believe that considering their explanatory power as a group is most appropriate. An F-test (untabulated) indicates that the set of concurrent disclosure variables is significant at the 0.001 level in explaining variation in $|AR_{jtq}|$. With this caveat, we note that the most significant concurrent disclosures (when interacted with TREND) are detailed income statements (significance level of 0.001), detailed statements of cash flows (with an unexpected negative sign, significant at the 0.077 level), and bad news forecasts (significance level of 0.001).

A potential concern with Model 3 is that one or more of the interaction variables is highly correlated with $TREND_t$.[11] In separate tests (not reported), we examined the pairwise correlations between $CONC\_DISC^k_{jtq} \times TREND_t$ and $TREND_t$, and find large correlations between $TREND_t$ and IS × $TREND_t$ (correlation is 0.67) and between $TREND_t$ and GOOD_CURR × $TREND_t$ (correlation is 0.47). While removing GOOD_CURR × $TREND_t$ from Model 3 does not alter the conclusion that concurrent disclosures eliminate the trend in $|AR_{jtq}|$, removing IS × $TREND_t$ does. Specifically, re-estimating Model 3 excluding IS × $TREND_t$ results in an estimate of $\rho_1 = 0.004$, significant at the 0.04 level. (In unreported tests, we verify that no other concurrent disclosure variable affects the significance of ρ_1.) We conclude from these analyses that the inclusion of detailed income statements plays an important role in explaining the over-time increase in $|AR_{jtq}|$ for our sample firms.

Given the relative importance of IS × $TREND_t$, we probe its relation with $|AR_{jtq}|$ further by partitioning earnings announcements based on whether the press release contains a detailed income statement. In untabulated tests, we find larger price reactions to earnings announcement press releases that include detailed income statements (mean $|AR_{jtq}|$ of 3.5 percent) than to press releases that exclude them (mean $|AR_{jtq}|$ of 2.9 percent), difference significant at the 0.001 level. Moreover, in a regression of $|AR_{jtq}|$ on $TREND_t$ for each partition, we find no over-time pattern in absolute price reactions to earnings announcements *without* detailed income statements, but a positive trend of 0.10 percent per year (significant at the 0.001 level) in reactions to announcements *with* detailed income statements.

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[11] In unreported tests, we also examine whether firm size or over-time changes in firm size are correlated with the concurrent disclosure variables. Inclusion of size and size-trend interaction variables in equations (4)–(6) does not affect our inferences.

Overall, analyses of the press release data indicate that over-time increases in the amount of concurrent disclosures and over-time increases in investors' reactions to concurrent disclosures explain the increase in $|AR_{jtq}|$. Further tests show that the increasing trend toward including detailed income statements in earnings announcement press releases and the increasing investor response to such statements explain the upward trend in $|AR_{jtq}|$.

V. SUMMARY AND CONCLUSION

Building on prior research that documents over-time increases in investor responses to larger firms' earnings announcements, we investigate three potential explanations for the increased usefulness of these earnings announcements over 1980–1999. We find no evidence that over-time changes in the amount of unexpected earnings (explanation 1) or that over-time changes in investors' reaction to a unit of unexpected earnings (explanation 2) contribute to an upward trend in absolute price reactions to earnings announcements for our sample firms. If anything, the results show the upward trend in reactions exists despite declines in the magnitude of absolute unexpected earnings and in earnings response coefficients.

The third explanation recognizes the fact that the earnings number is not released in isolation; rather it is part of a detailed and informative press release containing both "bottom line" earnings and other, concurrent disclosures, some of which disaggregate the earnings number (e.g., a detailed income statement) and some of which provide current and/or forward-looking nonearnings information. Our analysis of concurrent disclosures in a sample of 2,190 earnings announcement press releases issued by a randomly selected subset of 30 of our sample firms during 1980–1999 supports the third explanation—that increases in absolute market reactions to earnings announcements are driven by over-time increases in the amount of concurrent disclosures. On nearly all of the dimensions we code concurrent information, we find significant increases over time. For example, press releases get longer (in terms of the number of words); they provide increasing detail about nonrecurring items and about current operating data; and they are increasingly likely to include detailed income statements, balance sheets, and cash flow statements. These concurrent disclosures are significantly positively related to the absolute market reactions to press releases, adding almost 6 percent to the explanatory power of unexpected earnings.

Our results indicate that the most important concurrent disclosure for our sample is a detailed income statement, and that a combination of an increasing trend toward including such statements and an increasing investor response to these income statements explains the increases in market responses to our sample firms' earnings announcement press releases. We interpret our results as supporting the importance of earnings components to investors. While "bottom line" earnings as a single summary performance measure has apparently experienced a decline in usefulness to investors, our tests do not support a decline in the usefulness of the earnings measure when it is considered not as a single-number summary but rather in terms of its components.

Our research design, which is intended to hold sample composition constant and to include firms most likely to exhibit an increase in absolute price responses to earnings announcements (the phenomenon we wish to explain), necessarily focuses on large, stable firms. An issue to be resolved by future research is whether our inferences generalize to samples of significantly smaller firms.

APPENDIX
CODING SCHEME FOR PRESS RELEASES[12]

For each earnings announcement press release between 1980 and 1999, for our randomly selected subset of 30 sample firms, we collected the following information:

Nonrecurring Earnings Components

We code the presence of 11 types of nonrecurring items if the press release explicitly mentions the item: (1) discontinued operations, (2) extraordinary gains or losses, (3) unusual gains or losses, (4) restructuring charges, (5) asset impairments, (6) foreign exchange gains or losses, (7) securities gains or losses, (8) LIFO liquidations or adoptions, (9) accounting changes, (10) divestitures, and (11) other. We sign the item as conveying good news if it has a positive effect on earnings; as bad news if it has a negative effect on earnings; and as neutral news if it has no effect on earnings. Mean number of earnings components (in Table 6) for year t = 1980–1999 is the average number, per firm, of nonrecurring components reported in year t.

We form the following variable, used in Table 7:

$EARN\_COMP_{jtq}$ = the number of types of nonrecurring earnings components explicitly mentioned in the text of firm j's press release for quarter q of year t. $EARN\_COMP_{jtq}$ ranges between 0 (no types provided) and 11 (the maximum number of earnings component types we gathered).

Current and Forecast Operating Data

We code the disclosure of current (pertaining to the quarter being reported on) and forecast (pertaining to future periods) operating information about ten items: (1) orders/contracts, (2) shipments/production levels, (3) capital spending, (4) R&D spending, (5) market share, (6) margins, (7) cash flows, (8) segment data, (9) new products, and (10) other. We use subjective judgment to sign the item as conveying good news if it has a positive effect on earnings; as bad news if it has a negative effect on earnings; and as neutral news if it has no effect on earnings.

Mean numbers of pieces of current and forecast operating data (in Table 6) for year t = 1980–1999 are the average numbers, per firm, of pieces of current and forecast operating information, respectively, included in year t.

Detailed Financial Statements

We code the presence of detailed income statements, balance sheets, and statements of cash flows. Percent of press releases containing detailed income statement, detailed balance sheet and detailed cash flow statement (in Table 6) for year t=1980–1999 is the percent of all press releases examined for year t that contained the given statement.

We combine information on the disclosure of detailed financial statements to form the following variables used in Table 7:

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[12] Our coding scheme extends the categories used by Hoskins et al. (1986) to include the most common types of information firm managers communicate to securities analysts, as documented by Francis et al. (1997).

IS_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly income statement, 0 otherwise;

BS_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly balance sheet, 0 otherwise; and

SCF_{jtq} = 1 if firm j's press release for quarter q of year t contains a detailed quarterly statement of cash flows, 0 otherwise.

Officer Comments

We code the disclosure of current (pertaining to the quarter being reported on) and forecast (pertaining to a future period) qualitative comments made by management. We use subjective judgment to sign the item as conveying good news if it has a positive effect on earnings; as bad news if it has a negative effect on earnings; and as neutral news if it has no effect on earnings.

Percent of press releases containing current officer comments and officer forecasts (in Table 6) for year t = 1980–1999 is the percent of all press releases examined for year t that contained any qualitative officer comments about the current period or about the future.

We combine information revealed by statements concerning operating data and by officers' qualitative comments and form the following four variables used in Table 7:

$GOOD\_CURR_{jtq}$ = the number of operating statements and of officer qualitative comments about the current quarter that convey good news;

BAD_CURR_{jtq} = the number of operating statements and of officer qualitative comments about the current quarter that convey bad news;

$GOOD\_FORE_{jtq}$ = the number of operating statements and of officer qualitative comments about a future period that convey good news; and

BAD_FORE_{jtq} = the number of operating statements and of officer qualitative comments about a future period that convey bad news.

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Earnings Management to Avoid Earnings Declines across Publicly and Privately Held Banks

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ABSTRACT: This study compares samples of publicly and privately held bank holding companies to examine whether the high frequency of small earnings increases relative to small earnings decreases reported by public firms is attributable to earnings management. We expect public banks' shareholders to be more likely than private banks' shareholders to rely on simple earnings-based heuristics in evaluating firm performance, so we expect public banks to have more incentives to report steadily increasing earnings. Consistent with this expectation, we find that relative to private banks, public banks: (1) report fewer small earnings declines, (2) are more likely to use the loan loss provision and security gain realizations to eliminate small earnings decreases, and (3) report longer strings of consecutive earnings increases. These results suggest that the asymmetric pattern of more small earnings increases than decreases, first documented by Burgstahler and Dichev (1997), is attributable to earnings management and is not simply a reflection of the underlying distribution of earnings changes.

Keywords: *earnings management; private vs. public firms; financial institutions.*

Data Availability: *All data are available from public sources.*

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I. INTRODUCTION

Burgstahler and Dichev (1997) and Degeorge et al. (1999) document that publicly held firms report small declines in earnings less often than small increases in earnings. Although this pattern is consistent with managers using accounting discretion to avoid small earnings declines, an alternative explanation is that this pattern simply reflects the underlying distribution of earnings changes.

Our study investigates the validity of the earnings management explanation by examining the stream of earnings changes and the components of these changes for publicly vs. privately held bank holding companies (banks) during 1988–1998.[1] We focus on the public vs. private distinction because we argue that shareholders of public banks are more likely than those of private banks to rely on simple earnings-based heuristics, such as comparisons of current vs. prior period earnings, in evaluating bank performance. Therefore, we expect public bank managers to face more pressure to report consistently increasing earnings.

We focus on the banking industry because it provides comparable earnings data for a large number of both public and private firms. Data for private firms are rarely available in other industries. In addition, by focusing on the banking industry we can use industry-specific models of accounting discretion in the loan loss provision and realized security gains and losses (e.g., Beatty et al. 1995; Collins et al. 1995) to develop powerful tests of earnings management associated with small earnings changes.

We begin by replicating the analysis of small earnings changes that Burgstahler and Dichev (1997) conducted on a heterogeneous sample of public nonfinancial firms, to ascertain whether their inferences also hold in our samples of banks. We find that public banks report more small increases and fewer small decreases in earnings than expected. We find only weak evidence that private banks report fewer small decreases in earnings than expected. Furthermore, we find that public banks report more small increases and fewer small declines in earnings than private banks, even after controlling for differences in the operations of public vs. private banks, measured by bank size, asset growth, cash flows, loan characteristics, and geographic region.

If public banks' lower incidence of small declines in earnings is attributable to earnings management, then public banks should be more likely than private banks to use discretionary accounting choices to avoid reporting small declines in earnings. Consistent with this prediction, we find that public banks are more likely than private banks to use income-increasing discretionary loan loss provisions and realized security gains and losses to transform small declines in earnings before discretion to small increases in reported earnings.

Finally, if public banks are more likely than private banks to manage earnings to avoid reporting small decreases in earnings, then public banks, on average, should enjoy longer strings of consecutive earnings increases. Our tests confirm this expectation, even after controlling for differences in operating characteristics.

This study's results are important because we exploit a powerful setting to provide more direct and compelling evidence that the unusual pattern of small earnings increases of publicly held firms observed by prior research is due to earnings management and not to the underlying characteristics of the distribution of earnings changes. Our findings should be of interest to the SEC in its ongoing initiative against earnings management. In addition, our finding that private banks do not exhibit an abundance of small increases in earnings adds to our understanding of earnings management in private firms. Investors and auditors of private firms should be interested to learn that private firms are not as strongly inclined to report consistently increasing earnings.

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[1] Burgstahler and Dichev (1997), Degeorge et al. (1999), and Hayn (1995) report evidence suggesting that firms report fewer small losses than expected. Furthermore, in concurrent research, Beaver et al. (2000) find that both stock and mutual property-casualty insurers avoid reporting small losses by understating loss reserve accruals. We focus on changes in, rather than levels of, earnings because few banks report losses.

We have organized the paper as follows. Section II compares public and private banks' incentives to manage earnings to achieve simple earnings-based benchmarks. Section III describes the research design, while Section IV describes the sample and provides descriptive statistics. Section V presents the results of the empirical tests, and conclusions appear in Section VI.

II. EARNINGS-BASED BENCHMARKS

Ownership of publicly traded firms in the United States is diffuse (Shleifer and Vishny 1997). Black (1992) argues that diffuse owners only obtain a small fraction of the benefits from monitoring firms' activities due to their small shareholdings. These benefits likely fall short of the costs of storing, retrieving, and processing all information available to assess financial performance. As a result, Burgstahler and Dichev (1997) conjecture that investors in publicly held firms rely on simple low-cost heuristics, such as earnings-based benchmarks, in firm valuation.[2]

Empirical evidence suggests that investors in publicly traded firms use simple earnings-based benchmarks in making firm valuation decisions.[3] Barth et al. (1999) demonstrate that firms with longer strings of consecutive earnings increases are priced at a premium, and that, when these firms experience declines in earnings, the premiums fall substantially. Similarly, DeAngelo et al. (1996) find that a break in a pattern of consistent earnings growth is associated with a substantial decline in stock price. The stock price penalties for falling short of prior earnings combined with the effect of stock price on manager's wealth (Core et al. 2000), gives managers of publicly held firms an incentive to report a pattern of increasing earnings.[4]

In contrast to publicly held firms, ownership of privately held firms is concentrated. Privately held firms usually have a small number of shareholders and substantial majority shareholder participation in the management, directions, and operations of the firm. For example, Nagar et al. (2001) find more than 84 percent of their sample of 2,776 privately held, small, nonfarm, nonfinancial corporations as of year-end 1992 have four or fewer owners. In addition, for nearly 75 percent of the firms, the manager is an owner. Shareholders of private firms, therefore, have a relatively low marginal cost of acquiring and disseminating information and reap a large share of the benefits. In addition, shares of private companies are rarely traded, leaving private shareholders with little incentive to assess firm value continuously. As a result, private investors are likely to use a fairly rich information set, rather than relying on simple earnings-based heuristics. Consistent with this argument, results in Ke et al. (1999) suggest that shareholders in private companies directly monitor management rather than rely on explicit earnings-based compensation contracts to determine compensation.

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[2] Burgstahler and Dichev (1997) also suggest that prospect theory provides an alternative related explanation. Prospect theory (Kahneman and Tversky 1979) suggests that individuals' value functions are concave in gains and convex in losses. Therefore, if zero is a natural reference point for change in earnings, then managers will manipulate earnings so the change is positive.

[3] Evidence in Bhattacharya (2001) suggests that at least one segment of the public market relies on a simple earnings-based benchmark. Specifically, small traders in small publicly held firms with little-to-moderate analyst following appear to use a simple seasonal random-walk model to form earnings expectations.

[4] Ke (2001) examines how these earnings management incentives vary within a sample of publicly held firms drawn from Execucomp. Specifically, he finds that the frequency of small earnings increases and the duration of strings of consecutive earnings are both increasing in the market-to-book ratio and CEO equity incentives from stock and exercisable stock options.

Relatedly, Beatty and Harris (1999) argue that private firms have both less information asymmetry and a greater proportion of long-run investors than public firms and, therefore, less incentive to manage earnings. Beatty and Harris (1999) provide evidence that private firms manage earnings less aggressively, but they do not consider the use of simple earnings-based heuristics and therefore do not examine small changes in earnings and the duration of consecutive earnings increases.

Based on these arguments, we predict that private banks report more small declines in earnings, fewer small increases in earnings, and shorter strings of consecutive earnings increases than do public banks. We test these predictions by comparing the distribution of changes in earnings around zero and the duration of strings of consecutive earnings increases for public and private banks.

Our analysis should not be affected by other incentives for earnings management. For example, regulators and depositors provide additional incentives for banks to manage earnings. However, FDIC insurance protects most depositors of public and private banks, and explicit regulatory reporting and capital requirements are the same for public and private banks. Incentives to manage earnings for these stakeholders should be similar across public and private banks, and therefore should not explain observed differences in earnings management.

Banks may have an incentive to manage earnings to minimize taxes, but we see no reason for taxation to increase the incentive to avoid small declines in earnings. In addition, tax rules are generally the same for public and private banks during our sample period. The one notable exception is that banks with less than $500 million in assets were allowed to take a tax deduction for the loan loss provision, while banks with more than $500 million in assets tax deduct their loan losses on a cash basis. Because private banks are likely to be smaller, this difference may confound our analysis of differential earnings management. To address this difference, we perform sensitivity tests of our loan loss provision analysis on both large and small private banks.

Public and private banks differ in their access to equity markets. Because public banks have more access to external equity financing, they are likely to be larger, to grow faster, and to be more profitable. Our empirical tests control for these differences by including measures of size, asset growth, and cash from operations. These controls should help mitigate concerns that the differences between public and private banks that we document are driven by differential economic performance, rather than by differences in earnings management incentives.

III. RESEARCH DESIGN

Univariate Analysis of Small Earnings Changes

Our first set of tests examines small changes in earnings for public and private banks. Similar to Burgstahler and Dichev (1997), we first examine histograms of the change in return on assets (ΔROA), which we calculate as the current year's net income less the previous year's net income, divided by total assets at the beginning of the previous year. In constructing our histograms we use a bin width of twice the interquartile range of the variable multiplied by the negative cube root of the sample size, as Degeorge et al. (1999) recommend. Based on this formula, the bin width we apply in the histograms is 0.0004.

To test whether the distribution of earnings changes around the zero threshold is smooth, we next replicate Burgstahler and Dichev's (1997) primary analysis by calculating the standardized difference for the two intervals adjacent to zero for our public and private banks. The standardized difference for an interval is the difference between the observed and expected number of observations in the interval, standardized by the estimated standard deviation of the difference. For this analysis we consider ΔROA as small if the absolute value is less than 0.0008; we therefore analyze ΔROA within two intervals, one that lies between −0.0008 and 0, and one that lies between 0 and 0.0008. The interval size, which is twice the bin width used in the histograms of ΔROA, results from a trade-off between sample size and feasible earnings management amounts. Using interval sizes that are one and three times the bin width does not affect our results. The expected number of observations in each interval is the average of the number of observations in the two immediately adjacent intervals. We calculate the standard deviation of the standardized difference using the entire distribution of earnings changes. If the intervals around zero are smooth, then the standardized differences will be distributed approximately normal, with a 0 mean and standard deviation of 1.

Probit Analysis of Small Earnings Changes for Public vs. Private Banks

To test for differences in the management of small earnings changes between public and private banks, we next conduct a probit regression of the sign of small earnings changes on an indicator variable for public vs. private banks, controlling for differences between these two types of banks that may affect the sign of the change in premanaged earnings (i.e., the nondiscretionary changes in earnings). Specifically, we estimate the following year and regional fixed-effects model on all bank-years with earnings changes near zero (i.e., with the absolute value of ΔROA less than or equal to 0.0008):

$$\begin{aligned}\Delta ROAPOS_{it} = {} & \alpha_{tr} + \beta_1 PUBLIC_i + \beta_2 \Delta ASSET_{it} + \beta_3 LASSET_{it} + \beta_4 \Delta CF_{it} \\ & + \beta_5 \Delta^2 NPL_{it} + \beta_6 \Delta LOANR_{it} + \beta_7 \Delta LOANC_{it} \\ & + \beta_8 \Delta LOAND_{it} + \beta_9 \Delta LOANA_{it} + \beta_{10} \Delta LOANI_{it} \\ & + \beta_{11} \Delta LOANF_{it} + \varepsilon_{it}, \end{aligned} \tag{1}$$

where:

i = bank holding company index;
t = year index for 1988–1998;
r = geographic region index for the eight regions defined by the U.S. Department of Commerce;
ΔROAPOS = dummy variable, taking the value 1 if the bank has ΔROA in the interval between 0 (exclusive) and 0.0008 (inclusive), and 0 otherwise; and
PUBLIC = dummy variable, taking the value 1 if the bank is publicly held, and 0 otherwise.

The control variables are the following:

ΔASSET = first difference in total assets, divided by total assets at the end of the previous year;
LASSET = natural log of total assets;
ΔCF = first difference in cash flows, divided by total assets at the end of the previous year, where cash flow is defined as net income plus the loan loss provision and non-interest expenses associated with fixed assets;

Δ^2NPL = change in the first difference in nonperforming loans, divided by the average of beginning and ending total loans;
ΔLOANR = first difference in loans secured by real estate as a percentage of total loans;
ΔLOANC = first difference in commercial and industrial loans as a percentage of total loans;
ΔLOAND = first difference in loans to depository institutions as a percentage of total loans;
ΔLOANA = first difference in loans to finance agricultural production as a percentage of total loans;
ΔLOANI = first difference in loans to individuals as a percentage of total loans;
ΔLOANF = first difference in loans to foreign governments as a percentage of total loans; and
ε = error term.

ΔASSET controls for growth and LASSET controls for bank size. If larger and higher-growth banks are increasingly more profitable or more likely to manage earnings to avoid reporting a decline in earnings, then the coefficients on ΔASSET and LASSET should be positive. ΔCF controls for changes in profitability. We expect banks with more positive changes in cash flows to be more likely to report small increases in earnings rather than small decreases; therefore we expect the coefficient on ΔCF to be positive. Δ^2NPL controls for the effect of changes in the quality of the loan portfolio on nondiscretionary changes in earnings. The change in nonperforming loans (ΔNPL) is an important predictor of the loan loss provision, which is a major component of earnings. We use the Δ^2NPL because our dependent variable is the change in earnings. An increase in nonperforming loans should lead to an increase in the loan loss provision and a decrease in earnings; therefore we predict a negative coefficient on Δ^2NPL.

The other variables (ΔLOANR, ΔLOANC, ΔLOAND, ΔLOANA, ΔLOANI, and ΔLOANF) control for changes in the characteristics of the loans in each bank's portfolio because these characteristics may affect nondiscretionary changes in earnings. We do not predict how these variables are likely to affect the sign of small changes in earnings.

After controlling for these differences in bank characteristics, the change in earnings should not differ significantly across public vs. private banks, absent earnings management. PUBLIC is the explanatory variable we use to test our hypothesis that public banks are more likely than private banks to manage small changes in earnings above the threshold of zero. Thus, we predict that the coefficient on PUBLIC will be positive.

Controlling for all important differences between public and private banks is essential to interpret the coefficient on PUBLIC as (albeit indirect) evidence that the previously documented pattern of earnings changes in publicly held firms reflects earnings management rather than the underlying distribution of earnings changes. Essentially, if private banks have little incentive to manage earnings changes around zero, then the distribution of their earnings changes around zero can serve as a benchmark for assessing the distribution of small earnings changes in public banks. If the asymmetry of small earnings changes around zero arises only for public banks, then earnings management becomes a more plausible explanation.

Discretionary Accounting Choices

To provide more direct evidence of earnings management, we examine two components of banks' earnings that researchers have shown are subject to manipulation: loan loss provisions and realized security gains and losses (e.g., Moyer 1990; Beatty et al. 1995; Collins et al. 1995; Ahmed et al. 1999). Bank managers can potentially avoid reporting small declines in earnings by underestimating the loan loss provision. Bank managers may also be able to avoid a small decline in earnings by realizing more security gains or fewer security losses.

We use the following regression model, similar to that of Beatty et al. (1995), to estimate the nondiscretionary portion of the loan loss provision:

$$\begin{aligned} LOSS_{it} = {} & \alpha_{tr} + \beta_1 LASSET_{it} + \beta_2 \Delta NPL_{it} + \beta_3 LLR_{it} + \beta_4 LOANR_{it} + \beta_5 LOANC_{it} \\ & + \beta_6 LOAND_{it} + \beta_7 LOANA_{it} + \beta_8 LOANI_{it} + \beta_9 LOANF_{it} + \varepsilon_{it}, \end{aligned} \tag{2}$$

where:

LOSS = loan loss provision as a percentage of the average of beginning and ending total loans;
ΔNPL = change in nonperforming loans as a percentage of the average of beginning and ending total loans;
LLR = loan loss reserve as a percentage of total loans at the beginning of the year;
LOANR = loans secured by real estate as a percentage of total loans;
LOANC = commercial and industrial loans as a percentage of total loans;
LOAND = loans to depository institutions as a percentage of total loans;
LOANA = loans to finance agricultural production as a percentage of total loans;
LOANI = loans to individuals as a percentage of total loans; and
LOANF = loans to foreign governments as a percentage of total loans.

We control for region effects and loan characteristics because we expect both to affect the loan loss provision. We expect the loan loss provision to be higher when there is an increase in nonperforming loans and when the beginning loan loss reserve is high, so we predict that the coefficients on ΔNPL and LLR will be positive. We make no prediction for the other variables.

We use the following regression model, similar to that of Beatty and Harris (1999), to estimate the nondiscretionary portion of the realized security gains and losses:

$$RSGL_{it} = \alpha_t + \beta_1 LASSET_{it} + \beta_2 UNGL_{it} + \varepsilon_{it}, \tag{3}$$

where:

RSGL = realized security gains and losses as a percentage of total assets at the beginning of the year; and
UNGL = unrealized security gains and losses at the beginning of the year as a percentage of total assets at the beginning of the year.

We expect realized security gains and losses to be higher when there are higher unrealized security gains and losses, so we predict that the coefficient on UNGL will be positive.[5]

To estimate the two models, we pool across all bank-years with available data. We then use the residuals from the regression models as our estimates of *abnormal accruals.* We compute the change in earnings before the abnormal loan loss provision and the change in earnings before abnormal realized security gains and losses for each bank-year, and refer to these earnings changes as *premanaged.*

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[5] We do not control for regional effects in the realized security gains and losses model because banks' investments are largely in U.S. government bonds, so investment market values should not vary by region.

We identify bank-years with premanaged changes in return on assets that lie in the interval just below zero (i.e., between −0.0008 and 0) or in the interval just above zero (i.e., between 0 and 0.0008). We conduct tests on the abnormal accruals across the two intervals and across bank type. If public banks manage earnings to avoid reporting small declines in earnings more so than private banks, then we expect the abnormal loan loss provision and security gains and losses to be more income-increasing for the public bank-years with premanaged earnings in the interval just below zero than for the private bank-years with premanaged earnings in the interval just below zero. We make no prediction about the sign of the abnormal accruals for banks with small increases in premanaged earnings because we have no theory of earnings management behavior for these observations.

Differences in the Duration of the String of Consecutive Earnings Increases

Our final analysis examines whether the duration of the string of consecutive earnings increases differs between public and private banks. If public banks have a stronger incentive to avoid declines in reported earnings, then we predict they will have longer strings of consecutive earnings increases than private banks. This test is distinct in two respects from the regression-based test of small earnings changes for public vis-à-vis private banks. First, the pooled cross-sectional regression-based test of small earnings changes does not directly consider the time-series pattern of earnings for the same firm. Second, the test of small earnings changes includes only bank-years with earnings changes close to the zero threshold. In contrast, our test of the duration of strings of consecutive earnings increases includes all eligible bank-year observations.

To test for differences in duration, we adopt an event history analysis approach. Although not widely used in accounting, researchers in labor economics, biomedical sciences, industrial engineering, political science, and sociology have used event history analysis to analyze the length of time that elapses between two specific events.[6]

Because event history analysis is relatively new in the accounting literature, we illustrate the basic concepts in our setting. Figure 1 depicts the time line of a hypothetical firm that entered our sample in 1987 and exited in 1998. We cannot observe the change in earnings until 1988. The hypothetical firm reported positive earnings changes in 1990, 1991, 1992, and 1993, but the consecutive string of earnings increases broke in 1994.[7] The firm did not report any earnings increases after 1994. In our context, the failure event is a break in the string of consecutive earnings increases. Thus, the sample firm first faced the risk of the failure event in 1991 and remained at risk until the failure in 1994. If a firm-year does not report an earnings increase in the prior period, then the current firm-year is not at risk of the failure event and thus is excluded from the analysis. Therefore, the event history analysis would use only the observations from 1991 to 1994 for our hypothetical firm.

Although firms that report negative earnings (losses) could also have consecutive earnings increases, it is unclear whether such strings would convey the same message to investors as would strings of consecutive earnings increases from profitable firms. Prior research (Hayn 1995) indicates that losses convey less information than positive earnings.

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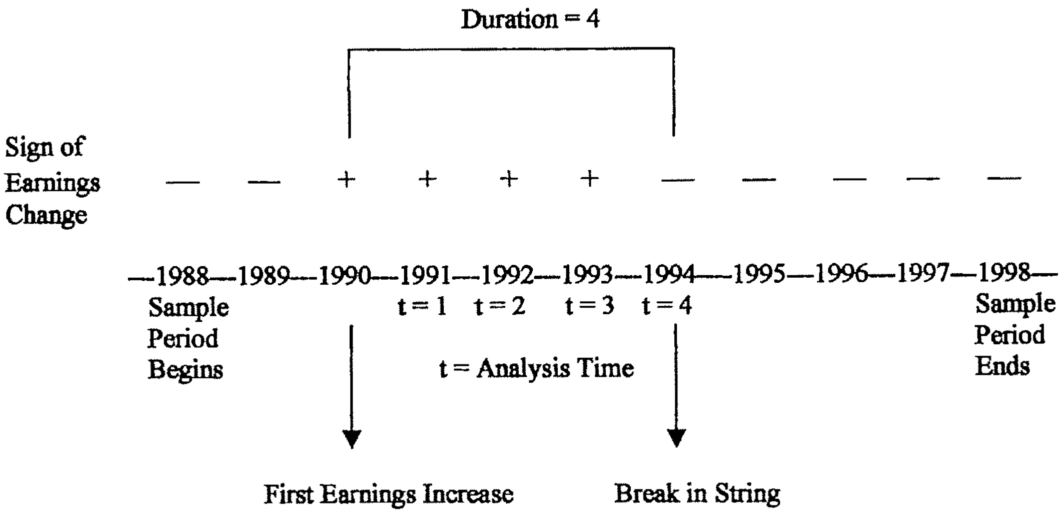
[6] Allison (1984) and Kiefer (1988) provide excellent reviews of event history analysis, which is also called survival analysis or hazards analysis.

[7] A firm can have more than one string of consecutive earnings increases; the maximum number of possible earnings increase strings over our sample period is five.

FIGURE 1
A Hypothetical Firm's Time Line for an Event History Analysis of a String of Consecutive Earnings Increases



Degeorge et al. (1999) argue that firms reporting negative earnings have no incentive to manage earnings upward. For this reason, our measure of earnings increases considers only those firm-years with positive current earnings.

An important concept in event history analysis is the hazard rate (denoted h(t)), which, in discrete time, is the probability that an at-risk firm experiences a failure event at time t. Thus, in our context, h(t) is the probability that a bank breaks a string of earnings increases in year t. We refer to time t as analysis time and measure it relative to the year when the bank first became at risk of the failure event (i.e., the beginning of 1991 for our hypothetical firm in Figure 1). Thus, for our sample firm, in 1992, analysis time $t = 2$. The duration, to which we refer as T or DURATION, is the maximum analysis time, which is almost always the number of consecutive earnings increases ($T = 4$ for our hypothetical firm).[8] Duration is right-censored if an earnings string is ongoing at the end of our sample period.

Event history analysis examines how the hazard rate h(t) evolves over time as a function of time t and other explanatory variables. To examine the effect of time t and other explanatory variables on the probability of breaking a string of earnings increases, i.e., the hazard rate h(t), this study adopts the parametric Weibull hazard model, which takes the following form:

$$h(t|x(t)) = h_o(t)\exp[x(t)'\beta], \tag{4}$$

where x(t) is a vector of time-varying explanatory variables; β is a vector of unknown parameters to be estimated; and $h_0(t)$, called the baseline hazard rate, is pt^{p-1} (called Weibull) with shape parameter p that we estimate from the data. When $p > 1$ the hazard model exhibits positive duration dependence, meaning that the hazard rate h(t) increases over time t. When $p < 1$, the hazard rate is a decreasing function of t; and when $p = 1$, the hazard rate is constant over time and the hazard model is exponential.[9] To compare the duration of consecutive earnings increases for public and private banks, we rewrite Equation (4) (see Kiefer 1988, 665) as follows:

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[8] DURATION equals the number of consecutive earnings increases unless an initial earnings increase is the last observation for the bank. In this case, we do not include this string with one consecutive earnings increase in DURATION because there are no analysis time observations related to this earnings increase (i.e., the maximum analysis time is 0).

$$\ln(T) = x(t)' \beta^* + e, \tag{5}$$

where ln(T) is the log of the observed duration of a string of consecutive increases in earnings; e is the error term that is independent of x(t) and is scaled by $1/p$; and $\beta^* = -\beta/p$. We estimate Model (5) using maximum likelihood. Coefficients β^* measure the percentage change in the expected duration of consecutive earnings increases T for a one-unit change in an explanatory variable. A positive coefficient on an explanatory variable means that the duration of consecutive earnings increases is an increasing function of the explanatory variable. The independent variables in Equation (5) are the same variables we include in our probit model of ΔROAPOS.

Hazard models offer advantages relative to traditional regression for analyzing breaks in strings of consecutive earnings increases.[10] First, hazard models incorporate both the occurrence and timing of breaks in strings of consecutive earnings increases, which provides more insights about the phenomenon of earnings declines. Second, these models correct for any right-censoring of the strings of consecutive earnings increases (i.e., our inability to observe the length of the strings of consecutive earnings increases that are still running when the firm exits the sample period). Finally, they allow time-varying explanatory variables, which are important in studying the dynamics of declines in earnings.

IV. SAMPLE AND DESCRIPTIVE ANALYSES

Sample

To compile our sample we start with all annual bank holding company information filed with the Federal Reserve System on form FR Y-9 C. The accounting data reported on this form are based on generally accepted accounting principles. Beginning in 1986, all top-tier bank holding companies with at least $150 million in total consolidated assets and all bank holding companies with more than one subsidiary must file this form. We obtained these data from two sources: For 1986–1990 we used the data from the Chicago Federal Reserve Bank web site, whereas, for 1991–1998 we used data purchased from Sheshenoff.[11] Only the Sheshenoff database provides information on the trading status of the bank holding companies, so we assume that trading status does not change during 1986–1990. This assumption seems reasonable, given the low rate of change in trading status during 1991–1998.

The original sample contained 2,838 bank holding companies. We omitted 205 banks that were 100 percent owned by another bank, 426 banks without ownership information in the Sheshenoff database, 69 banks with ownership changes during 1991–1998, and 271 banks missing data for at least one variable used in the later analysis. The final sample had 707 public banks and 1,160 private banks, for a total of 1,867 banks. Because we scale earnings by lagged assets, our change variables include observations from 1988–1998.

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[9] Our results are similar using the Cox proportional hazards model. The Cox model is similar to Equation (4) except that it does not impose structure on the baseline hazard rate, and therefore is immune from misspecification of the baseline hazard rate. We use the Weibull model because it enables us to calculate the difference in the expected duration of consecutive earnings increases for public and private banks, whereas the Cox model can give us only the relative hazard rates for public vs. private banks.

[10] See Peterson (1991) for a general discussion of the advantages of hazard models.

[11] To verify the validity of the regulatory data, we estimated the correlation between net income from our regulatory data and from Compustat for banks included in both data sets during 1986–1992. On average, there were 135 observations per year and the average correlation was 99.34 percent.

Descriptive Statistics

Panel A of Table 1 provides descriptive data about our sample of privately held and publicly held banks and two-sample Wilcoxon rank-sum tests for differences across these two groups. As expected, the public banks are significantly larger than the private banks. The mean and median assets of the 5,504 public bank-years are $5,066 million and $487

TABLE 1
Descriptive Statistics by Bank Type, 1988–1998
Mean (Median) [Standard Deviation] {Bank-Years}

Panel A: Descriptive Statistics for all Bank-Years

Variable Name[a]	*707 Public Banks*	*1,160 Private Banks*	*Rank-Sum Z for Difference between Public and Private Banks (two-tailed p-value)*
ASSETS (millions)	5,066 (487) [21,594] {5,504}	307 (200) [607] {6,202}	58.93 (0.001)
ΔASSET	0.122 (0.081) [0.194] {5,504}	0.090 (0.061) [0.216] {6,202}	14.35 (0.001)
ROA	0.011 (0.011) [0.008] {5,504}	0.010 (0.011) [0.009] {6,202}	9.10 (0.001)
ΔROA	0.002 (0.002) [0.007] {4,762}	0.001 (0.001) [0.007] {4,940}	6.55 (0.001)
ΔCF	0.003 (0.002) [0.006] {4,762}	0.002 (0.002) [0.007] {4,940}	10.33 (0.001)
Δ^2NPL	−0.0003 (0.0004) [0.014] {4,762}	−0.0001 (0.0005) [0.031] {4,940}	1.03 (0.304)
DURATION	3.281 (2) [2.484] {949}	2.282 (2) [1.677] {1,113}	9.64 (0.001)

(Continued on next page)

TABLE 1 (Continued)

Panel B: Descriptive Statistics for the Bank-Years used in the Probit Regressions of Table 3

Variable Name[a]	*413 Public Banks*	*559 Private Banks*	*Rank-Sum Z for Difference between Public and Private Banks (two-tailed p-value)*
ASSETS (millions)	3,655 (411) [19,865] {878}	305 (208) [561] {985}	22.66 (0.001)
ΔASSET	0.088 (0.065) [0.098] {878}	0.074 (0.059) [0.096] {985}	3.14 (0.002)
ROA	0.0107 (0.0108) [0.0035] {878}	0.0102 (0.0103) [0.0041] {985}	3.53 (0.001)
ΔROA	0.0002 (0.0003) [0.0004] {878}	0.0001 (0.0001) [0.0005] {985}	5.80 (0.001)
ΔCF	0.0010 (0.0010) [0.0020] {878}	0.0006 (0.0006) [0.0023] {985}	5.69 (0.001)
Δ^2NPL	0.0007 (0.0006) [0.0011] {878}	−0.000 (0.0006) [0.0013] {985}	0.79 (0.430)
ΔROAPOS	0.730 {878}	0.596 {985}	6.10 (0.001)

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[a] Variable definitions:

ASSETS = total assets;

ΔASSET = first difference in total assets, divided by total assets at the end of the previous year;

ROA = net income divided by total assets at the beginning of the year;

ΔROA = current year's net income less previous year's net income, divided by total assets at the beginning of the previous year;

ΔCF = first difference in cash flows, divided by total assets at the end of the previous year, where cash flow is defined as net income plus loan loss provisions and non-interest expenses associated with fixed assets;

Δ^2NPL = change in the first difference of nonperforming loans, divided by the average of beginning and ending total loans;

ΔROAPOS = dummy variable, taking the value 1 if the bank has ΔROA in the interval between 0 (exclusive) and 0.0008 (inclusive), and 0 otherwise; and

DURATION = number of years in a string of consecutive increases in earnings for banks with at least three consecutive years of nonnegative earnings.

million, respectively, whereas the mean and median assets for the 6,202 private bank-years are only \$307 million and \$200 million, respectively. Public banks also have higher asset growth than private banks. The mean growth in assets for public banks is 12.2 percent per year, while the mean growth for private banks is 9 percent. The public banks have significantly higher return on assets (ROA) and change in return on assets (ΔROA). Consistent with public banks' higher profitability, the change in cash flows (ΔCF) is significantly more positive for public banks.

Since, on average, public banks perform better than private banks, it is critical to our interpretation of the incidence of small earnings increases that our tests adequately control for differences in economic performance. Our tests include controls for asset growth, size, cash flow, loan portfolio characteristics, year, and geographic region. To assess the adequacy of these controls we regressed ROA and ΔROA on all of these variables. The overall distributions of the residuals from the two regressions (i.e., the unexpected ROA and ΔROA) are not significantly different across the public and private banks. This suggests that our controls are successful in explaining differences in overall economic performance between public and private banks.

Public banks report longer strings of consecutive positive earnings changes than do private banks, as measured by DURATION. A string may range from one increase in earnings to 11 increases. We measure DURATION only for banks with at least three consecutive years of nonnegative earnings. The mean DURATION of strings of consecutive earnings increases is 3.3 years for public banks and 2.3 years for private banks. This difference is significant, which we expected because public banks have higher ROA and ΔROA. DURATION is censored, however, so one should interpret it cautiously. There are 768 right-censored earnings strings that were still running when the bank exited the sample period. Similarly, there are 970 left-censored earnings strings that began before the first year of the sample period.

Panel B of Table 1 reports descriptive statistics for the subsample of bank-years with small earnings changes that we use in our probit analysis. Differences across bank type are similar to those for the full sample. Again, as expected, public banks report small positive earnings changes more frequently than private banks. Specifically, 73 percent of public bank-years with small ΔROA have a positive ΔROA, compared to 59.6 percent for private banks. This difference is significant at $p < 0.001$. Since the differential profitability of public vs. private banks is not controlled in this univariate comparison, reliable inferences on differences in the incidence of small increases in earnings across public vs. private banks cannot be drawn until completion of the probit analysis that includes controls for profitability.

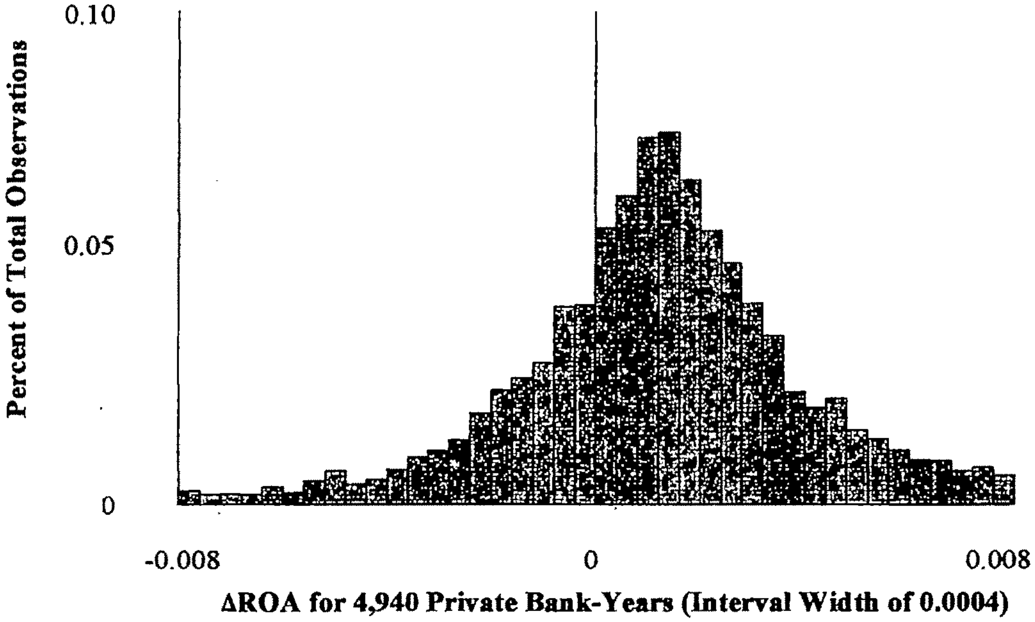
V. RESULTS

Univariate Tests of Differences of Small Earnings Changes

Figure 2 plots ΔROA for private and public banks. In both cases there is a large increase in the percentage of observations just above zero. This increase is much more dramatic for public banks, suggesting that public and private banks differ in their reporting of small earnings declines.

In Table 2 we report the results of our Burgstahler and Dichev (1997) analysis. As predicted, public banks report fewer small declines in earnings than expected (standardized difference of -10.07). It is inappropriate to interpret the standardized difference as a t-statistic for assessing significance because the distributions of our earnings changes are not smooth (i.e., the assumption that the earnings changes are normally distributed is rejected

FIGURE 2
Current Year's Net Income Less Previous Year's Net Income Divided by Total Assets at the Beginning of the Previous Year (ΔROA) for Public and Private Banks, 1988–1998



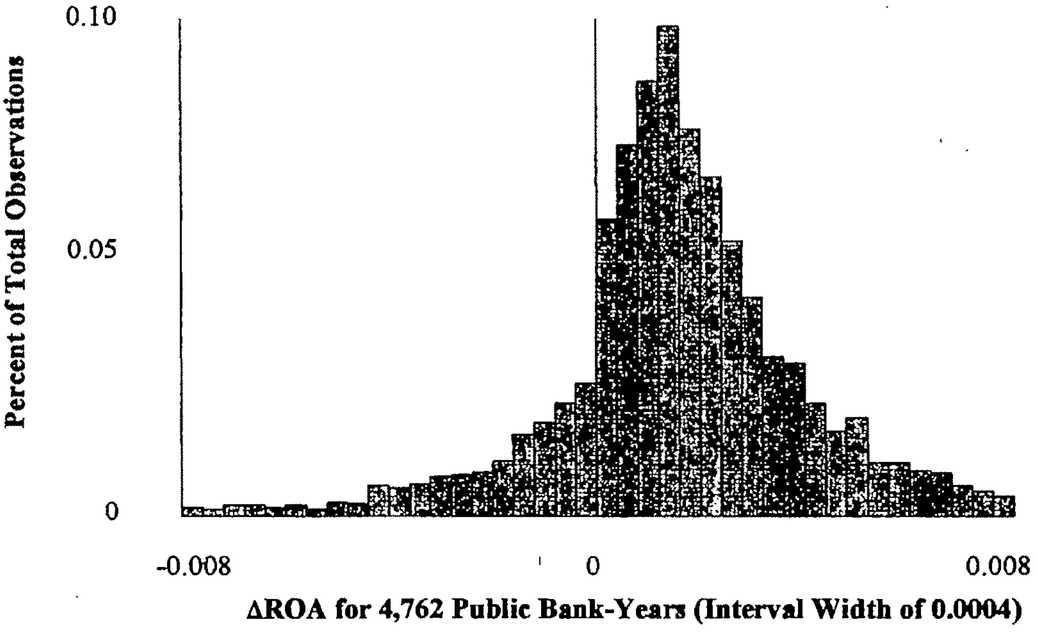


TABLE 2
Bank-Years with Changes in Return on Assets in the Interval Just Below Zero and Just Above Zero by Bank Type, 1988–1998

	Standardized Difference[a] *(Actual Number of Bank-Years in Interval) [Expected Number of Bank-Years in Interval]*	
(Total Observations)	*Interval Just Below Zero (ΔROA*[b] *between 0 and −0.0008)*	*Interval Just Above Zero (ΔROA between 0 and 0.0008)*
Public Banks (4,762)	−10.07 (238) [404.5]	4.72 (640) [562]
Private Banks (4,940)	−2.08 (403) [425.5]	0.92 (582) [572]

[a] The standardized difference is the difference between the observed and expected number of bank-years in an interval, standardized by the estimated standard deviation of the difference.

[b] ΔROA is current year's net income less previous year's net income, divided by total assets at the beginning of the previous year.

at the 0.001 level for both samples).[12] However, this standardized difference is the most negative of the standardized differences across all 316 public bank bins. This increases our confidence that the number of public bank-years with small declines in earnings is significantly smaller than expected. Consistent with the presence of few small declines in earnings, we also find more small increases in earnings than expected (standardized difference of 4.72, which is the second largest across all 316 bins).

We find weak evidence that private banks also report fewer small decreases in earnings than expected (standardized difference of −2.08). This standardized difference is the fifth most negative across all 248 private bank bins. Because this result appears only marginally significant, we are reluctant to draw the conclusion that private banks avoid reporting small decreases in earnings. In addition, we do not find significantly more small increases in earnings than expected (standardized difference of 0.92). Differences in sample sizes prevent us from comparing the standardized differences of the public vs. private banks (Burgstahler and Dichev 1997, 110–111), but recall that in Panel B of Table 1, we report that the proportion of small earnings increases relative to small earnings decreases is significantly greater at $p < 0.001$ for public banks than for private banks.

Probit Tests of Differences in Small Earnings Changes

Table 3 reports the probit results for the differential likelihood of reporting small increases in earnings vs. small declines, across the public/private dichotomy. The analysis includes all 1,863 bank-years for 972 different banks with absolute ΔROA no greater than 0.0008.[13] The coefficient on PUBLIC is significantly positive at less than the 0.001 level (we report two-sided statistics throughout the paper). Thus, public banks report significantly

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[12] The assumption of a smooth distribution was more reasonable for Burgstahler and Dichev (1997) because their sample size was at least five times larger than ours.

[13] Regulatory capital requirements for banks changed in 1990 (see Ahmed et al. 1999). To assess whether this is confounding our analysis, we re-estimated the probit model including only observations from 1991–1998, and found results nearly identical to those based on the full sample period.

TABLE 3
Probit Model of Earnings Management for Banks with Small Earnings Changes, 1988–1998[a] (n = 1,863)[b]

$$\Delta ROAPOS_{it} = \alpha_{tr} + \beta_1 PUBLIC_i + \beta_2 \Delta ASSET_{it} + \beta_3 LASSET_{it} + \beta_4 \Delta CF_{it} + \beta_5 \Delta^2 NPL_{it} + \beta_6 \Delta LOANR_{it} + \beta_7 \Delta LOANC_{it} + \beta_8 \Delta LOAND_{it} + \beta_9 \Delta LOANA_{it} + \beta_{10} \Delta LOANI_{it} + \beta_{11} \Delta LOANF_{it} + \varepsilon_{it}$$

Independent Variable[c]	*Coefficient Estimate*	*Two-Tailed p-value*
PUBLIC	0.307	0.000
ΔASSET	−0.486	0.154
LASSET	0.051	0.119
ΔCF	76.553	0.001
Δ^2NPL	−5.468	0.045
ΔLOANR	2.970	0.090
ΔLOANC	3.043	0.098
ΔLOAND	1.226	0.586
ΔLOANA	1.522	0.542
ΔLOANI	1.110	0.538
ΔLOANF	−25.038	0.274

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[a] Variable Definitions:
i = bank holding company index;
t = year index for 1988–1998;
r = geographic region index for the eight regions defined by the U.S. Department of Commerce;
ΔROAPOS = dummy variable, taking the value 1 if the bank has ΔROA in the interval between 0 (exclusive) and 0.0008 (inclusive), and 0 otherwise;
PUBLIC = dummy variable, taking the value 1 if the bank is publicly held, and 0 otherwise;
ΔASSET = first difference in total assets, divided by total assets at the end of the previous year;
LASSET = natural log of total assets;
ΔCF = first difference in cash flows, divided by total assets at the end of the previous year, where cash flow is defined as net income plus loan loss provision and non-interest expenses associated with fixed assets;
Δ^2NPL = change in the first difference of nonperforming loans, divided by the average of beginning and ending total loans;
ΔLOANR = first difference in loans secured by real estate as a percentage of total loans;
ΔLOANC = first difference in commercial and industrial loans as a percentage of total loans;
ΔLOAND = first difference in loans to depository institutions as a percentage of total loans;
ΔLOANA = first difference in loans to finance agricultural production as a percentage of total loans;
ΔLOANI = first difference in loans to individuals as a percentage of total loans;
ΔLOANF = first difference in loans to foreign governments as a percentage of total loans; and
ε = error term.

[b] The estimation sample includes only bank-years with the absolute value of ΔROA less than or equal to 0.0008.

[c] Year and region intercepts are untabulated.

more small increases and fewer small decreases in earnings than private banks do; even after controlling for bank size, asset growth, cash flows, loan characteristics, and geographic regions. Bank size and growth do not provide explanatory power. As expected, banks with increases in cash flows are more likely to report small increases in earnings, evidenced by a positive significant coefficient on ΔCF. In addition, a significant negative coefficient on Δ^2NPL suggests that increases in the change in nonperforming loans are associated with declines in earnings.[14]

Discretionary Accounting Choices

Table 4 reports results of our estimation of the abnormal loan loss provision and abnormal security gains and losses models.[15] The loan loss provision and security gains and losses models have an R^2 of 21 percent and 10 percent, respectively. The results for the loan loss provision regression are generally consistent with prior research (e.g., Moyer 1990; Wahlen 1994; Beatty et al. 1995; Collins et al. 1995; Ahmed et al. 1999). The significant positive coefficients on LLR and ΔNPL indicate that the loan loss provision is increasing as the bank's problem loans increase and in the amount of the loan loss reserve at the beginning of the year. In addition, the provision increases in the proportion of commercial and individual loans and the provision decreases in loans to other depository institutions. In the security gains and losses regression, the significant positive coefficient on UNGL indicates that realized security gains and losses are increasing in unrealized gains and losses as of the beginning of the year, consistent with Scholes et al. (1990) and Beatty and Harris (1999). We estimate the abnormal components as the residuals from the regressions and, for brevity, refer to these as abnormal provisions and abnormal security gains.

Table 5, Panel A reports the abnormal provisions for bank-years with small changes in *premanaged* earnings. For the 505 public bank-years with small declines in premanaged ROA (i.e., between 0 and −0.0008), the mean (median) abnormal provision is −0.0017 (−0.0019). For the 535 public bank-years with a small increase in premanaged ROA (i.e., between 0 and 0.0008), the mean (median) abnormal provision is −0.0009 (−0.0012). A reduction in the loan loss provision expense increases reported income, therefore, the significantly more negative abnormal provision for bank-years with premanaged ΔROA in the interval below zero vs. those in the interval above zero ($p = 0.001$) is consistent with public banks using the loan loss provision to avoid reporting small declines in income. Further analysis reveals that this manipulation was successfully used to avoid an earnings decline for 84.4 percent of our public banks that had small declines in earnings before the abnormal provision.

These results are consistent with public banks using accounting discretion to transform small declines in ROA into small increases. However, this test is biased in favor of rejecting the null if measurement error in the abnormal component of the loan loss provision is correlated with the classification of premanaged ΔROA into the intervals just below or just above zero. This is likely since premanaged ΔROA is determined using an estimate of the abnormal provision. However, to the extent that such measurement error exists equally for

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[14] To provide further evidence that these results are not confounded by size, we also re-estimate the probit model on a size-matched subset of our sample. Specifically, we include only those public banks with average total assets over the 1988–1998 period that are less than the public sample median, and only those private banks with total assets that are above their sample median. This subset includes 933 bank-years with median assets of 275 for the public banks, and 286 million for private banks. The coefficient on PUBLIC is positive and highly significant. We perform a similar analysis on a growth-matched sample and also find similar results.

[15] Conclusions are unaltered if we estimate the nondiscretionary amounts using separate models for private and public banks.

TABLE 4
OLS Regression Models of the Loan Loss Provision and Realized Security Gains and Losses, 1988–1998[a]

$$LOSS_{it} = \alpha_t + \beta_1 LASSET_{it} + \beta_2 \Delta NPL_{it} + \beta_3 LLR_{it} + \beta_4 LOANR_{it} + \beta_5 LOANC_{it} + \beta_6 LOAND_{it} + \beta_7 LOANA_{it} + \beta_8 LOANI_{it} + \beta_9 LOANF_{it} + \varepsilon_{it}$$

$$RSGL_{it} = \alpha_t + \beta_1 LASSET_{it} + \beta_2 UNGL_{it} + \varepsilon_{it}$$

Independent Variable[b]	*Loan Loss Provision Coefficient Estimate (two-tailed p-value)*	*Realized Security Gains and Losses Coefficient Estimate (two-tailed p-value)*
LASSET	0.0003	0.00005
	(0.000)	(0.000)
ΔNPL	0.048	
	(0.000)	
LLR	0.072	
	(0.000)	
LOANR	0.001	
	(0.708)	
LOANC	0.005	
	(0.004)	
LOAND	−0.012	
	(0.001)	
LOANA	−0.003	
	(0.120)	
LOANI	0.003	
	(0.066)	
LOANF	0.049	
	(0.388)	
UNGL		0.004
		(0.000)
Bank-Years[c]	9,160	9,247
Adjusted R^2	0.21	0.10

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[a] Variable Definitions:
LOSS = loan loss provision as a percentage of the average of beginning and ending total loans;
LASSET = natural log of total assets;
ΔNPL = change in nonperforming loans as a percentage of the average of beginning and ending total loans;
LLR = loan loss reserve as a percentage of total loans at the beginning of the year;
LOANR = loans secured by real estate as a percentage of total loans;
LOANC = commercial and industrial loans as a percentage of total loans;
LOAND = loans to depository institutions as a percentage of total loans;
LOANA = loans to finance agricultural production as a percentage of total loans;
LOANI = loans to individuals as a percentage of total loans;
LOANF = loans to foreign governments as a percentage of total loans;
RSGL = realized security gains and losses as a percentage of total assets at the beginning of the year; and
UNGL = unrealized security gains and losses at the beginning of the year as a percentage of total assets at the beginning of the year.

[b] Year intercepts are untabulated.

[c] Influential observations are deleted using Cook's (1977) distance criteria.

TABLE 5

Abnormal Accruals for Banks with Small Premanaged Changes in Return on Assets across Bank Type, 1988–1998[a]

Panel A: Abnormal Loan Loss Provision (mean (median) [standard deviation])

	Premanaged ΔROA in the Interval Just Below Zero	*Premanaged ΔROA in the Interval Just Above Zero*	*Mean Difference across Intervals {two-tailed p-value}*[b]
Public Banks	−0.0017 (−0.0019) [0.0025] n = 505	−0.0009 (−0.0012) [0.0027] n = 535	−0.0008 {0.001}
Private Banks	−0.0011 (−0.0015) [0.0031] n = 434	−0.0007 (−0.0010) [0.0028] n = 531	−0.0004 {0.090}
Mean Difference between Public and Private Banks {p-value}	−0.0006 {0.002}	−0.0002 {0.353}	−0.0004 {0.088}

Panel B: Abnormal Realized Security Gains and Losses (mean (median) [standard deviation])

	Premanaged ΔROA in the Interval Just Below Zero	*Premanaged ΔROA in the Interval Just Above Zero*	*Mean Difference across Intervals {two-tailed p-value}*
Public Banks	0.0003 (0.0001) [0.0007] n = 291	0.0001 (0) [0.0005] n = 544	0.0002 {0.001}
Private Banks	0 (−0.0001) [0.0007] n = 382	0 (−0.0001) [0.0005] n = 518	0 {0.579}
Mean Difference between Public and Private Banks {p-value}	0.0003 {0.001}	0.0001 {0.023}	0.0002 {0.001}

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[a] Premanaged ΔROA is ΔROA before the abnormal loan loss provision in Panel A and before the abnormal security gains and losses in Panel B. The abnormal accruals are the residuals from the regressions reported in Table 4. Changes in Premanaged ΔROA are small if they are less than or equal to 0.0008 in magnitude.

[b] The p-values are adjusted for serial correlation across bank-years for each bank and heteroskedasticity as described by Rogers (1993).

both public and private banks, it will not affect a comparison of differences across public and private banks. Thus, we focus on whether these findings are more pronounced for public banks than for private banks.

Similar to public banks, the abnormal loan loss provisions for private banks with small decreases in premanaged ROA are more income-increasing than the abnormal provisions for private banks with small increases in premanaged ROA.[16] However, the magnitude of the difference across the two intervals is, at best, marginally significant ($p = 0.09$) and is only half the magnitude of the difference observed for public banks. In addition, the abnormal provision for private banks with a small decline in premanaged ROA is significantly less income-increasing than that of public banks with a similar decline in premanaged ROA ($p = 0.002$). Furthermore, only 64.3 percent of the private banks with small declines in earnings before the abnormal provision successfully manipulated the loan loss provision to ultimately report an increase in ROA. This percentage is significantly less than the corresponding 84.4 percent for the public banks ($p < 0.001$). These differences between public and private banks suggest that the results for public banks are not simply attributable to bias in the measurement of the abnormal component of the loan loss provision.

Panel B of Table 5 repeats the analysis for realized gains and losses from security transactions. The mean (median) abnormal security gain for public bank-years with small declines in premanaged ROA in the interval just below zero is 0.0003 (0.0001). This is significantly more income-increasing than for public bank-years with small increases in premanaged ROA ($p < 0.001$). Again, this is consistent with public banks using discretion to make income-increasing choices to avoid reporting a small decline in ROA. The mean abnormal security gain is essentially zero for private banks regardless of the sign of premanaged ΔROA. This suggests that private banks do not use security transactions to manage small earnings changes.

The abnormal security gains for banks with small decreases in premanaged ROA are significantly more income-increasing for public banks than for private banks ($p < 0.001$). There is also evidence that the abnormal security gains for public banks with small *increases* in premanaged ROA are more income-increasing than for private banks ($p = 0.023$). However, the difference in the abnormal security gain between banks with small declines vs. small increases in pre-managed ROA is significantly greater for public banks than for private banks ($p < 0.001$). Consistent with only public banks using security transactions to avoid small declines in earnings, we find that 42.3 percent of public banks vs. 18.8 percent of private banks with small earnings declines before the abnormal security gains ultimately report an earnings increase. This difference in the proportion of bank-years that successfully use security transactions to report an increase in ROA across public and private banks is significant ($p < 0.001$).[17]

Differences in the Duration of Consecutive Earnings Increases

Table 6 reports the maximum likelihood estimates of our Weibull hazard Model (5), with the log of DURATION as the dependent variable. DURATION represents 2,062 strings of consecutive earnings increases for 1,261 banks. Within these strings are 5,654 analysis

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[16] To determine whether differences in taxation of the loan loss provision for small vs. large banks confounds our analysis, we compare abnormal provisions of private banks having more than $500 million in assets with those having less. The abnormal provisions do not differ significantly across the two samples, so differences in taxation are not likely to confound our analysis.

[17] If our public (private) banks with small declines in premanaged earnings before both the abnormal loan loss provision and abnormal security gains, 82.3 percent (61.6 percent) ultimately reported increases in earnings. The difference across bank type is significant ($p < 0.001$).

TABLE 6
Weibull Hazard Model Estimation of the Determinants of the Duration of Strings of Consecutive Earnings Increases, 1989–1998 (n = 2,062)[a]

Independent Variable[b]	*Coefficient*	*Robust Two-Tailed p-value*[c]
PUBLIC	0.369	(0.000)
ΔASSET	0.216	(0.188)
LASSET	0.076	(0.000)
ΔCF	26.583	(0.000)
Δ^2NPL	−0.139	(0.770)
ΔLOANR	1.731	(0.019)
ΔLOANC	2.353	(0.003)
ΔLOAND	0.512	(0.682)
ΔLOANA	2.514	(0.016)
ΔLOANI	0.219	(0.808)
ΔLOANF	6.974	(0.128)
Shape parameter p	1.66	
Log likelihood	−1,935	
Model χ^2	928	

[a] The sample includes all bank-years after an increase in nonnegative earnings and with at least three years of data.
[b] The dependent variable is the log of the observed duration of a string of consecutive earnings increases. Other variable definitions are in Table 3. Year and region intercepts are untabulated.
[c] The p-values are robust as described by Lin and Wei (1989).

time bank-years (i.e., bank-years at risk of a failure event). We calculate the standard errors for the coefficients using a robust estimation procedure (Lin and Wei 1989). One may interpret the regression coefficients like ordinary regression coefficients. The coefficients for calendar-year and region dummies are untabulated for brevity, although most are significant. The Weibull hazard model shape parameter p is greater than 1 and highly significant, suggesting that the likelihood of breaking a string of consecutive increases in earnings escalates as analysis time t increases.

The positive LASSET and ΔCF coefficients indicate that larger firms and those with increases in cash flows enjoy longer strings of consecutive earnings increases. The coefficient on ΔASSET is not significant.

As expected, the coefficient on PUBLIC is significantly positive, indicating that the duration of consecutive earnings increases is longer for public banks even after controlling for other determinants of bank profitability (e.g., growth, size, loan characteristics, and geographic region).[18] Holding everything else constant, the expected duration of a string of consecutive earnings increases is 45 percent ($e^{0.369} - 1$) longer for public banks. Given a median expected duration of approximately 3.86 years for private banks, this suggests that the duration of earnings increases would be about 1.7 years greater for public banks.[19]

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[18] We performed several sensitivity tests. We obtain similar results for size-matched and growth-matched samples, and for samples that omit the 970 left-censored earnings strings and banks with incomplete panels of data.

VI. CONCLUSIONS

This paper compares small changes in earnings for public vs. private banks during 1988–1998 to increase our understanding of why public firms' reported earnings changes are asymmetric around zero, with more small increases than decreases in reported earnings. We argue that shareholders of public banks are more likely than shareholders of private banks to rely on simple earnings-based heuristics in making firm valuation decisions. Thus, we predict that public banks are more likely than private banks to manage earnings to achieve simple benchmarks, such as increases in earnings.

Our results support this hypothesis. We find that public banks report fewer small declines in earnings and more small increases in earnings than expected. In contrast, we find that private banks report only marginally fewer small declines in earnings than expected. Even after controlling for potential differences between the operations of public and private banks, we find that public banks are significantly less likely than private banks to report small declines in earnings. Further analysis suggests that public banks use discretion in their loan loss provisions and in their recognition of security gains and losses to avoid reporting small declines in earnings. We find that private banks have a lesser propensity than public banks to use discretion in their loan loss provisions to avoid reporting small declines in earnings, and we find no evidence that private banks use discretionary security gains and losses to avoid reporting small declines in earnings. Furthermore, we find that, on average, the duration of strings of consecutive earnings increases is greater for public banks.

Documenting differences in the earnings streams of private vs. public banks achieves three goals. First, it helps validate that the lower-than-expected incidence of small decreases in earnings of public firms is not primarily attributable to some unidentified underlying economic condition, but rather to earnings management. Second, our results are consistent with public shareholders' reliance on simple earnings-based heuristics serving as a driver of earnings management. Third, it increases our understanding of the role of accounting in private firms. Specifically, it demonstrates that private firms appear less apt to manage earnings toward a simple earnings-based benchmark.

This study has several limitations. First, because we focus on only one highly regulated industry, the results may not generalize to other industries. Second, our tests of the discretionary components of the loan loss provision and security gains and losses are biased toward finding earnings management for both public and private banks. To the extent that these tests are not more biased for public than for private banks, however, we can still draw inferences based on the observed *differences* between the two groups. Tests on the differences in the abnormal loan loss provision and the abnormal security gains and losses both support the hypothesis that public banks are more likely than private banks to engage in earnings management to transform small decreases in ROA into small increases. Third, other unidentified (and therefore uncontrolled) differences between public and private banks may affect the observed differences in the asymmetry of earnings changes around zero, as well as differences in the duration of the strings of consecutive earnings increases.

Finally, we argue that the differences we have documented in the frequency of small earnings increases result from variation in the reliance on simple earnings-based heuristics by public vs. private investors; however, our tests document only an association. We do not directly examine whether investors' reliance on heuristics causes differential earnings management.

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[19] The expected median exceeds the observed median reported in Table 1, because the expected median accounts for right-censoring of the data.

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Negotiated Transfer Pricing: Is Fairness Easier Said than Done?

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ABSTRACT: Questionnaire responses reported by Luft and Libby (1997) reveal that transfer price negotiators expect fairness-based price concessions that moderate the influence of an outside market price when the outside market price strongly favors one of the parties. We examine whether these expectations of fairness extend to the actual prices that result from real-cash negotiations. Findings indicate that expectations of fairness-based price concessions do not survive actual negotiations when participants negotiate over a computer network with no communication other than bids, asks, and acceptances. Conversely, both expectations and actual negotiated outcomes reflect fairness-based price concessions when participants negotiate in a face-to-face setting with unrestricted communication. Together, these results imply that the extent to which questionnaire-based judgments of social behavior generalize to actual behavior depends on the whether the competitive environment suppresses or reinforces the social presence necessary to sustain phenomena such as preferences for fairness.

Keywords: *transfer pricing; fairness; negotiation.*

Data Availability: *The experimental data are available for purposes of replication. Please contact the authors.*

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I. INTRODUCTION

Firms often allow their divisions to negotiate transfer prices (Eccles 1985; Emmanuel and Mehafdi 1994). When divisions have the option to transact in outside markets if they cannot reach agreement internally, conventional economic reasoning holds that a selling division would hesitate to accept less and a buying division would hesitate to pay more than the external market price. However, if the outside market price generates widely different profits across divisions, then interdivisional concerns for fairness could lead the two divisions to agree on a price that distributes profits more evenly. Such fairness pressures could constitute an important source of the demand for accounting disclosures that reveal relative profit information (Luft 1997).

Luft and Libby (1997) use a case-based questionnaire to investigate preferences for fairness in negotiated transfer pricing. They find that executive M.B.A.s expect transfer prices to be significantly lower than the outside market price when the outside price favors sellers, but not when the outside price results in equal profits for the buying and selling divisions. However, Luft and Libby's (1997) participants did not actually negotiate transfer prices, nor did they face monetary consequences. The applicability of Luft and Libby's (1997) findings to practice hinges on whether fairness pressures influence not only the transfer prices people *expect* to negotiate, but also the transfer prices they *actually* negotiate. Our study addresses this question.

Comparing fairness effects in expected and actual transfer price negotiations requires us to consider *how* parties negotiate transfer prices. Borrowing from Bazerman et al. (2000), we contrast two ends of a social presence continuum. At one end, we use a computerized negotiation mechanism in which the only communications are bids, asks, and acceptances. This setting typifies the experimental economics literature (Friedman and Sunder 1994), and reflects the increasing use of depersonalized, technology-driven negotiation mechanisms in practice (Moore et al. 1999). Notwithstanding this trend, face-to-face interaction will always play an important role in organizations, limiting the practical insights from a strictly computerized setting restricted to bids, asks, and acceptances. Accordingly, to capture the other end of the social presence continuum, we also investigate a setting in which the parties negotiate face-to-face, with no restrictions on communication.

These two settings yield different conclusions regarding the linkage from initial expectations to actual transfer prices. In the restricted, computerized setting, participants indicate that they expect fairness-based price concessions similar to those documented by Luft and Libby (1997) when the outside market price favors sellers, but these expectations do not materialize in actual negotiations. By contrast, in the unrestricted, face-to-face setting, we find a pronounced fairness effect in both initial expectations and subsequent negotiations. Prior transfer pricing studies have employed computerized negotiations (e.g., Ghosh 1994, 2000), written negotiations (e.g., DeJong et al. 1989; Greenberg et al. 1994), and face-to-face negotiations (e.g., Chalos and Haka 1990; Ravenscroft et al. 1993), with little consideration of the possible behavioral influences of these different negotiation mechanisms. Our results suggest that at least in some settings, different negotiation processes can yield different conclusions.

In addition to offering a better understanding of negotiated transfer pricing, these findings underscore the importance of merging psychology-based behavioral accounting research with the tools of experimental economics (e.g., Kachelmeier 1996; Haynes and Kachelmeier 1998; Moser 1998; Waller 2001; Libby et al. 2002; Sprinkle 2002). Specifically, we find that the extent to which questionnaire-based judgments (typifying the cognitive-behavioral tradition) generalize to actual decisions in compensated, competitive settings (typifying the experimental economics tradition) depends on environmental features, such as the means of negotiation, that can either suppress or reinforce phenomena such as preferences for fairness.

Section II develops the hypotheses. Section III describes our experiment. Section IV presents tests of the primary hypotheses, and Section V presents supplemental analyses to corroborate the primary findings and to examine the incremental effects of negotiation experience and relative profit information. Section VI concludes and discusses the future research directions suggested by the study's limitations.

II. THEORY AND HYPOTHESES

Baseline Hypotheses for Expected Transfer Prices

Because our goal is to compare Luft and Libby's (1997) findings for questionnaire-based transfer pricing expectations to the results from actual cash negotiations, we must first establish a common baseline. Luft and Libby (1997) develop a case that highlights the friction between self-interest and organizational equity in negotiated transfer pricing. A selling division ("Parts") can supply a product at a cost of \$20 to a buying division ("Assembly") that values the product at \$80. An outside market also exists, in which the price of the same product is either \$70 (the treatment condition, favoring Parts) or \$50 (a control condition, equalizing profits between the two divisions). If the organization grants divisions the autonomy to go to the outside market as an alternative to transacting internally, then economic self-interest dictates that the parties should not settle for a transfer price less favorable than that available from the outside market, after adjusting for any transaction costs from transacting externally.

From a strict market perspective, one might question the need for negotiation, because the firm could simply impose the outside market price as the internal transfer price. But the applicability of market logic to the social organization of a firm is limited: "Market exchange tends predominantly to encourage calculative relations of a transaction-specific sort between the parties.... Internal organization, by contrast, is often better able to make allowance for quasimoral involvements among the parties" (Williamson 1975, 38). Negotiated transfer pricing captures these trade-offs, providing a mechanism whereby the firm's interdependent subunits can balance market factors against the broader social factors that affect long-term organizational effectiveness (Watson and Baumler 1975; Chalos and Haka 1990).

One such social factor is interdivisional equity. In the Luft and Libby (1997) scenario, a transfer price of \$70 would yield a much larger profit for Parts (\$70 − \$20 cost = \$50 profit) than for Assembly (\$80 − \$70 = \$10). Negotiators who have access to relative profit information might resist the perceived inequity of this split, consistent with anecdotal evidence that firms consider fairness repercussions in establishing transfer pricing policies.[1] For example, Eccles (1985, 8) summarizes evidence from interviewing 54 managers at 13 firms, concluding:

> The fundamental difficulty in managing transfer pricing involves establishing practices that will lead to decisions that enhance corporate performance, while at the same time measuring, evaluating, and rewarding performance in light of these practices in a way that managers perceive as fair.

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[1] By way of analogy, anecdotes abound in which complaints about salaries refer to perceptions of inequity relative to others, rather than dissatisfaction with the absolute level of pay.

Luft (1997) reviews the theoretical framework of models that have begun to incorporate preferences for fairness in bargaining (e.g., Kahneman et al. 1986; Bolton 1991; Rabin 1993), tying this framework to the role of management accounting as the means by which fairness pressures can arise. Drawing on this framework, Luft and Libby (1997) find that if it is reasonable to assume divisions have approximately equal entitlements in all other aspects, then negotiators expect greater price concessions below the outside market price when the outside price favors parts ($70 treatment condition) than when it splits profits evenly between Parts and Assembly ($50 control condition). To establish a common baseline, we hypothesize the same prediction:

H1: The transfer price negotiators expect will fall shorter of the outside market price when the outside price would result in significantly higher profits for Parts than when the outside price would result in equal profits for Parts and Assembly.

Evidence also suggests that expectations in negotiation are *egocentric*, meaning that each party overweights the view most beneficial to itself (Bazerman et al. 2000). Egocentrism is closely related to motivated reasoning (Kunda 1990), whereby an individual's preferences lead to biased expectations and judgments. In transfer pricing, even if the instrument that elicits expected transfer prices gives participants no reason to bias their responses, motivated reasoning and egocentrism suggest that participants will be unable to detach themselves from their assigned roles in forming such expectations. We posit this effect in our second hypothesis, based on the similar hypothesis predicted and detected by Luft and Libby (1997).

H2: Parts negotiators (sellers) will expect higher transfer prices than will Assembly negotiators (buyers).

Together, H1 and H2 imply that the largest departure of price expectations below the outside market price will be for *the price Assembly expects when the outside price favors Parts*. That is, when the outside price is equitable ($50), egocentrism simply means that buyers (sellers) are likely to expect a transfer price lower (higher) than $50. However, when the outside price favors sellers ($70), buyers' preference to be treated fairly (H1) compounds their egocentrism (H2), such that buyers are likely to expect to negotiate a price well below market. For sellers, any fairness effect (H1) may offset their natural egocentrism (H2), such that sellers expect a price close to the market price from which they would benefit. Equivalently, sellers may rationalize a different view of fairness, reasoning that they deserve the market price due to assumed cost efficiencies. In general, perceptions that one deserves a bigger slice of the pie tend to work against interpreting fairness as "equality" (Thompson and Loewenstein 1992; Hoffman et al. 1994; Kagel et al. 1996). Thus, buyers' expected price concessions might not materialize in actual negotiations. This is the question we consider next.

From Expectations to Outcomes

The first two hypotheses replicate Luft and Libby's (1997) tests of the transfer prices people *expect* to negotiate—but what would happen in actual negotiations with cash payment of obtained profits? One view is typified by Franciosi et al. (1995, 938), who characterize fairness as "expectations that are not sustainable." In a laboratory market study, they show that although fairness pressures influence initial expectations, incentives for wealth maximization eventually overpower fairness expectations in favor of classical economic equilibrium price predictions. Extended to transfer pricing, the Franciosi et al. (1995) argument suggests that when the outside price favors sellers, the negotiating parties (especially buyers) may initially expect price concessions that reduce the perceived inequity. But sellers may not agree to a more equitable price in actual negotiation, especially if they develop a more egocentric view of what is fair, rationalizing that they deserve the outside market price.

The validity of this argument is likely sensitive to the negotiation environment. Franciosi et al. (1995) demonstrated the fragility of fairness effects using a computerized trading mechanism that focused attention on monetary profits and restricted all other interaction. Experimental economists often restrict communication and face-to-face contact among participants, on the grounds that face-to-face cues and an unrestricted message space can jeopardize the experimentalist's control over the dominance of the induced monetary incentives (Friedman and Sunder 1994). This control comes at a cost, however, because these suppressed features can be pivotal to sustaining nonmonetary constructs such as fairness. Especially in an organizational context, limiting social interaction to a bid-ask book could restrict the very "quasimoral involvements" that Williamson (1975, 38) emphasized in differentiating organizations from markets.

Bazerman et al. (2000, 293–296) describe a "social presence continuum" of negotiations, varying in the degree to which the setting exerts social pressure on the negotiators. A computerized bid-ask book captures one end of this continuum, which we use to determine if there is any setting in which the Franciosi et al. (1995) argument applies to negotiated transfer pricing. This end of the continuum also captures an extreme form of the recent technology-enabled shift toward depersonalized, computer-assisted negotiation in practice (Moore et al. 1999). However, even organizations that capitalize fully on technology are unlikely to avoid all face-to-face contact and all communication beyond bids, asks, and acceptances. Thus, we also consider the other end of the social presence continuum, involving unrestricted, face-to-face communication. We expect face-to-face negotiation to strengthen negotiators' fairness preferences due to a more salient identification with the other party (which we term *humanization*) and due to the opportunity for persuasive communication.

The humanization effect of face-to-face contact is present even without expanded communication. For example, Drolet and Morris (2000) found that face-to-face negotiators reached more cooperative solutions in a conflict resolution setting than did parties who were able to communicate the same information side by side or by telephone. Arunachalam and Dilla (1995), Sally (1995), and Bohnet and Frey (1999) reported similar gains in cooperation among face-to-face bargainers. Conversely, other studies have reported that face-to-face bargaining leads to greater *confrontation* (e.g., Carnevale et al. 1981; Croson 1999) or a mixture of cooperation and confrontation (e.g., Valley et al. 1998). Although the conclusions are setting-specific, a common theme is that face-to-face contact elevates "social presence" (Bazerman et al. 2000), with different consequences in different environments.

For fixed-sum transfer pricing with fairness repercussions, the consequences of social presence likely hinge on the broader social context of "fairness." Among others, Isaac et al. (1991) and Luft (1997) emphasize that it is simplistic to view fairness only in terms of altruism. Rather, people may act fairly because they fear retribution from the aggrieved party.[2] That is, if Parts takes advantage of Assembly this year, then the tables might be turned next year. Neither Luft and Libby (1997) nor our study provided for explicit multiperiod retribution opportunities. However, there is likely some social discomfort akin to retribution when one treats a peer unfairly in a face-to-face setting.

Face-to-face negotiation can also lead to a *communication* effect by broadening the available message space (Wichman 1970; Roth 1995; Sally 1995; Bohnet and Frey 1999). As in most experimental economics studies, our computerized negotiation condition allows parties only to propose and accept prices. By contrast, face-to-face negotiation affords participants the opportunity to convince a counterpart, both in words and in nonverbal cues.

Prior studies have found that both the humanization and communication effects of face-to-face bargaining are influential (Wichman 1970; Bohnet and Frey 1999). However, these and the other studies cited in this section differ in several important ways from the transfer price setting we examine. In contrast to fixed-sum competitive transfer price negotiations, most face-to-face bargaining studies involve gains from cooperation similar to the gains in the familiar prisoners' dilemma.[3] In addition, we are aware of only two prior studies of alternative negotiation media in transfer pricing. Arunachalam and Dilla (1995) found that face-to-face group interaction facilitated Pareto-superior cooperative solutions in transfer pricing, but their experiment required participants to choose among a matrix of discrete payoff outcomes rather than to negotiate prices, *per se*. They did not manipulate relative profits to examine fairness pressures between buyers and sellers. Mahenthiran et al. (1993) found that computer-assisted negotiation facilitated quicker transfer price agreement relative to written (not face-to-face) negotiation, but their study did not involve fairness-based pressures. Neither of these studies compared participants' transfer price expectations to negotiated outcomes, which is our primary goal.

Given these differences and the setting-specific nature of prior conclusions, we examine the effects of perceived fairness on actual transfer price outcomes in the Luft and Libby (1997) transfer price setting, using negotiation media from both ends of Bazerman et al.'s (2000) social presence continuum. Our third hypothesis sets forth what we expect to find if face-to-face negotiation reinforces (and computerized negotiation suppresses) preferences for fairness in actual negotiation.

> **H3:** The manipulation of fairness through a \$70 or \$50 outside market price will influence the difference between the negotiated transfer price and the outside market price to a greater extent when negotiations are face to face with unrestricted communication than when negotiations are restricted to computerized bids, asks, and acceptances.

We also report several supplemental analyses of a more exploratory nature. These include (1) a variance test motivated from Bohnet and Frey (1999) to examine the relative influences of humanization and communication effects in the face-to-face setting, (2) tests of *movement* from expectations to outcomes, (3) investigation of decisions that occur after repeated negotiation experience, and (4) a supplemental experimental condition to examine explicitly the dependence of fairness expectations on the disclosure of relative profits.

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[2] One might view retribution more as a manifestation of self-interest than as a preference for fairness. That is, a credible retribution threat, if it affects the behavior of a negotiation partner, can increase profits. However, the concepts of fairness and retribution are linked to the extent that concerns for fairness influence outcome expectations, because the threat of retribution arises from violating these expectations. Thus, retribution reinforces fairness-based arguments (Luft 1997).

[3] An exception is that Bohnet and Frey (1999) also examined a dictator game, which is fixed-sum by definition. However, there is no negotiation, *per se*, in a dictator game, as one party simply dictates the outcome that the other party must accept.

III. METHOD AND DESIGN

Participants and Task

We recruited 48 M.B.A. student volunteers, 32 of whom participated in the primary experiment described below and the remaining 16 of whom participated in a supplemental condition to examine the incremental effect of relative profit information, as we describe in Section V.

The primary experiment consisted of four sessions of 90 minutes each, with eight participants in each session. Upon arrival, participants drew cards for random assignment to one of eight positions, four of which were for sellers (Parts) and four for buyers (Assembly). Instructions informed participants that each of the four Parts managers would negotiate independently with each of the four Assembly managers. This required four rounds of negotiation in which each participant was paired with a new negotiation partner in each round, following the so-called turnpike design. This design ensures that no participant faces the same negotiation partner twice, nor does he or she face any partner who has previously faced a former partner.

We began the experiment by distributing a transfer pricing case patterned as closely as possible after the case used by Luft and Libby (1997), including the same contextual descriptions (see the Appendix). The case provided common disclosure of cost and value information, allowing participants to compare relative profits across the spectrum of possible transfer prices. One unavoidable difference between our case and Luft and Libby's is that we specified the transaction costs that would apply under the outside market option. In practice, the uncertainty associated with transaction costs may be another reason why firms use negotiation instead of dictating internal transfer at the outside market price, which explains why Luft and Libby (1997, 224) described transaction costs qualitatively but did not specify them numerically. However, we had to parameterize these costs so participants could use them in the real-cash negotiations to follow. Although the specification of transaction costs is somewhat arbitrary, we chose a \$3.00 cost for both Parts and Assembly, consistent with an example suggested by Luft and Libby (1997). Thus, the case indicated that if the negotiators did not reach agreement, then Parts would get the outside market price less \$3.00 as revenue, and Assembly would pay the outside market price plus \$3.00 as cost. The \$6.00 transaction cost window (\$3.00 for each party) provides both parties an incentive to trade internally rather than go to the outside market.[4]

After reading the case, participants answered the same two questions asked in the Luft and Libby (1997) instrument, eliciting (1) the expected transfer price and (2) the expected lowest reservation price that Parts would likely be willing to accept. We discuss only the results from the first question because we obtain similar treatment effects from the second question. After participants answered both questions, we began the negotiation phase that extends Luft and Libby (1997). The negotiation followed directly from the case information. We repeated this process (i.e., expectation questionnaire followed by negotiation) for four rounds. The negotiation phase lasted three minutes each round, during which participants could propose transfer prices or accept the other party's proposed price.

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[4] Explicit transaction costs could potentially dampen the fairness effect reported by Luft and Libby (1997), because their participants may have been reluctant to rely on the outside market price without knowing its transaction cost ramifications. In that case, however, price concessions would simply reflect risk aversion to uncertain information, rather than fairness preferences, *per se*. Accordingly, we disclosed all payoff-relevant parameters, including transaction costs. To the extent that we continue to find the fairness effect reported by Luft and Libby (1997) in transfer price *expectations*, this ensures that our results are not driven solely by explicit transaction costs.

At the beginning of the session, we informed participants that they would be paid their profit for one part from one of the four rounds, determined randomly. At the end of the session, a volunteer participant drew a numbered chip to determine the payment round. Payments ranged from $7.00 (a buyer in the high outside-market-price condition whose negotiation pair failed to reach agreement and therefore used the out-option) to $52.00 (a seller in the same condition who managed to extract nearly all of the available surplus). The experiment did not involve deception in any form.

Experimental Manipulations

We manipulated two experimental factors. The first, outside price, is the same manipulation of fairness pressures used in Luft and Libby's (1997) questionnaire study. For each negotiation round, the case specified an outside market price of either $70 (favoring Parts) or $50 (dividing profits evenly between Parts and Assembly, as a control condition). The four negotiation rounds alternated between outside price conditions, counterbalanced across sessions to control for order effects (i.e., either $50-$70-$50-S70 or $70-$50-$70-$50).

The second experimental manipulation is the use of either computerized or face-to-face negotiation. In the computerized negotiation condition, participants completed the experiment in a computer research laboratory. Participants walked in together and knew the identity of each round's bargaining partner.[5] However, seats at each station faced the wall and were separated by partitions, so there was no face-to-face contact during the experiment. Participants negotiated over a computer network, using a tailored version of the experimental economics trading software developed by Plott (1991). This software consisted of a bid-ask book that allowed participants to propose or accept buying or selling prices, with no other communication.[6] To maintain independence across pairs, the software allowed participants to view only the current bid and asking price for their particular negotiation pair each round.

Participants in the face-to-face condition convened in a seminar classroom configured in the same layout as the laboratory used in the computerized version (i.e., Parts seated on one side of the room and Assembly on the other). After they read preliminary instructions and completed the case-based questionnaire for the first round, we escorted participants to private M.B.A. interview rooms in the college's Career Placement Center, with a separate room for each negotiating pair. Participants negotiated transfer prices in these rooms, with no restrictions on communication. We closed the door of each room during negotiation (with the experimenters outside) to ensure the privacy and freedom of communication of each negotiating pair. After the first negotiation period, we collected written outcomes and distributed the case and questionnaire for the next round. Participants individually wrote their expectations for the next case without talking, after which we moved one of each pair to a new room for a new round of face-to-face negotiation. This process continued through four rounds in total (just as in the computerized version).

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[5] Before the first round, the experimenter pointed out the clockwise sequential computer identification number assigned to each participant, which all could observe. The ability to identify other participants even in the computerized setting maintains realism, because computerized transfer price negotiations in practice are unlikely to be anonymous.

[6] The computer software required an additional training phase to familiarize participants with its use. In this training phase, participants practiced offering and accepting transfer prices of S5 and $10, amounts unrelated to the (as yet undisclosed) actual costs and profits used in the experiment.

IV. PRIMARY RESULTS

Expected Transfer Prices (H1 and H2)

Table 1 shows descriptive statistics for both expected and actual transfer prices. Table 2 reports an ANOVA with the expected transfer price from the pre-negotiation questionnaire as the dependent variable and the OUTSIDE PRICE ($50 or $70), ROLE (buyer or seller), and subsequent negotiation MEDIUM (computerized or face-to-face) as the independent factors.[7] For comparability to Luft and Libby (1997), this analysis includes expectations in Rounds 1 and 2 only (i.e., the participants' first experiences under the $50 and $70 outside market prices). In Section V, we report a supplemental analysis of decisions in Rounds 3 and 4.

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[7] In two instances, a participant indicated an expected transfer price lower than his or her estimate of the minimum seller reservation price. This is logically inconsistent, so we deleted these two observations.

TABLE 1
Mean Expected and Actual Negotiated Transfer Prices (Standard Deviations in Parentheses)

	Computerized Negotiation				*Face-to-Face Negotiation*			
	$50 Outside Price		*$70 Outside Price*		*$50 Outside Price*		*$70 Outside Price*	
	Rounds 1 and 2	*Rounds 3 and 4*	*Rounds 1 and 2*	*Rounds 3 and 4*	*Rounds 1 and 2*	*Rounds 3 and 4*	*Rounds 1 and 2*	*Rounds 3 and 4*
Price expectations:								
Buyers (Assembly)	$49.38	$49.62	$64.63	$70.14	$49.38	$49.75	$63.83	$66.13
	(1.77)	(1.06)	(7.52)	(1.46)	(1.19)	(0.71)	(6.34)	(3.87)
Sellers (Parts)	52.25	50.06	69.00	70.25	50.31	51.63	67.50	67.13
	(3.65)	(0.18)	(1.41)	(1.83)	(1.83)	(2.83)	(7.07)	(5.08)
Overall mean	50.81	49.84	66.81	70.20	49.84	50.69	65.93	66.62
	(3.15)	(0.77)	(5.69)	(1.61)	(1.57)	(2.21)	(6.78)	(4.88)
Mean difference from outside market price	0.81	−0.16	−3.19	0.20	−0.16	0.69	−4.07	−3.38
Negotiation outcomes:								
Cases in which agreement was reached	50.25	49.46	69.30	69.42	49.56	49.97	65.33	66.75
	(2.06)	(0.95)	(1.72)	(1.13)	(1.05)	(0.08)	(3.08)	(1.26)
All cases, using average of final bid and ask in cases of impasse	49.32	49.94	69.16	69.38	49.56	49.97	64.94	66.94
	(2.51)	(1.34)	(1.33)	(0.99)	(1.05)	(0.08)	(3.36)	(1.12)
Number of impasses	4	2	3	2	0	0	2	2
Mean difference from outside market price	−0.68	−0.06	−0.84	−0.62	−0.44	−0.03	−5.06	−3.06

TABLE 2
Analysis of Variance for Expected Transfer Prices in Rounds 1 and 2[a]

Factor	*df*	*Sum of Squares*	*F*	*p-value (two-tailed)*
OUTSIDE PRICE	1	240.64	10.47	<0.01
ROLE	1	134.31	6.84	0.02
MEDIUM	1	18.18	0.93	0.35
OUTSIDE PRICE × ROLE	1	14.92	0.65	0.43
OUTSIDE PRICE × MEDIUM	1	0.01	0.00	>0.50
ROLE × MEDIUM	1	5.48	0.28	>0.50
OUTSIDE PRICE × ROLE × MEDIUM	1	0.96	0.04	>0.50
Error	26			

[a] Definition of variables:
Dependent variable = expected transfer price − outside market price;
OUTSIDE PRICE = within-subjects binary factor for the two outside market price conditions ($50 or $70);
ROLE = between-subjects binary factor to differentiate buyer expectations (Assembly) from seller expectations (Parts); and
MEDIUM = between-subjects binary factor to differentiate computer network negotiation from face-to-face negotiation.

For transfer price *expectations*, there are no significant main or interaction effects involving the MEDIUM factor, so Figure 1 plots the main effects of OUTSIDE PRICE and ROLE after collapsing across the computerized and face-to-face negotiation conditions. As predicted in H1, Figure 1 shows that participants' expected prices fall farther below the outside market price in the $70 condition than in the $50 condition, which the ANOVA in Table 2 confirms to be statistically significant (F = 10.47; one-tailed $p < 0.01$).[8] As predicted in H2, Figure 1 shows and Table 2 confirms that sellers (Parts) expected systematically higher prices than did buyers (Assembly) (F = 6.84; one-tailed p = 0.01).

The support for H1 and H2 replicates Luft and Libby's (1997) primary findings.[9] Figure 1 also shows that although sellers expected higher transfer prices than buyers, both sellers and buyers exhibit a fairness effect in their expected transfer prices. This is evidenced by the downward-sloping lines between the $50 and $70 outside market price conditions for both sellers and buyers in Figure 1 and the lack of an interaction between OUTSIDE PRICE and ROLE in Table 2 (F = 0.65; p = 0.43). Sellers expected a transfer price slightly

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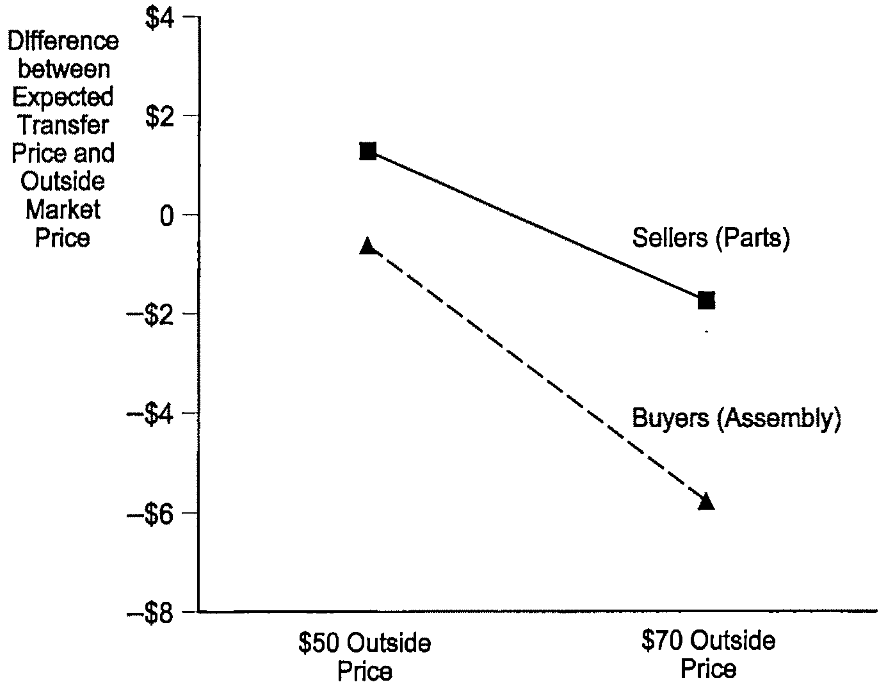
[8] The ANOVA source tables report two-tailed p-values for all main and interaction effects among the experimental factors. However, the narrative reports one-tailed p-values for the subset of these effects for which H1, H2, and H3 state directional predictions.

[9] For H1, our treatment difference of $3.96 between the $50 and $70 outside price conditions compares to $6.35 reported by Luft and Libby (1997). For H2, the seller − buyer difference is $2.96 in our study and $4.41 in Luft and Libby (1997). Notwithstanding the reasonably consistent treatment effects, there is a *systematic* difference in transfer prices between the two studies, with Luft and Libby (1997) reporting lower transfer prices in both the $50 and the $70 outside price conditions. This systematic difference could be partially attributable to our specification of numerical transaction costs in addition to any subject pool difference between our M.B.A. participants and the executive M.B.A.s that Luft and Libby (1997) used. The more important point, however, is that both studies detect a fairness effect between expected transfer prices in the two outside price conditions.

FIGURE 1
Difference between Expected Transfer Price and Outside Market Price
(Rounds 1 and 2)



below the outside market price in the $70 condition, suggesting that fairness pressures in this condition offset the natural egocentrism that led sellers to expect a price higher than market in the $50 control condition. Buyers expected a much larger price concession in the $70 condition, suggesting that their expectations of a fairer price compounded the natural egocentrism that led them to expect a price slightly below market even in the $50 condition. Thus, as reasoned in Section II, the largest difference between the expected transfer price and the outside market price occurred for buyers in the $70 condition.

Table 2 shows no significant interactions with the negotiation medium for expected transfer prices in Rounds 1 and 2, even though participants knew *ex ante* whether they would be negotiating by computer or face-to-face. This result is consistent with Drolet and Morris (2000), who also found that alternative negotiation media did not affect *anticipated* negotiation outcomes, albeit in a different setting (conflict resolution). Apparently, negotiators have difficulty anticipating their sensitivity to different negotiation processes, even though their actual negotiations sharply reflect this sensitivity, as we report presently.

Actual Transfer Prices (H3)

Table 3 reports an ANOVA on actual negotiated transfer prices, conducted similarly to the Table 2 ANOVA on expected transfer prices, with two exceptions. The first exception is that ROLE (buyer or seller) does not enter the model for actual transfer prices, because negotiated transfer prices are the product of joint buyer-seller agreement. The second exception involves cases in which the negotiating parties were unable to reach agreement

TABLE 3
Analysis of Variance for Actual Transfer Prices Negotiated in Rounds 1 and 2[a]

Panel A: Main Effects

Factor	*df*	*Sum of Squares*	*F*	*p-value (two-tailed)*
OUTSIDE PRICE	1	36.17	8.63	<0.01
MEDIUM	1	29.21	6.97	0.02
OUTSIDE PRICE × MEDIUM	1	14.50	3.46	0.08
Error	19			

Panel B: Main Effects for Each Negotiation Medium

Factor	*df*	*Sum of Squares*	*F*	*p-value (two-tailed)*
Effect of OUTSIDE PRICE when MEDIUM is *computerized*	1	2.01	0.48	0.50
Effect of OUTSIDE PRICE when MEDIUM is *face-to-face*	1	61.32	14.64	<0.01

[a] Definition of variables:
Dependent variable = actual transfer price − outside market price;
OUTSIDE PRICE = between-pairs binary factor for the two outside market price conditions ($50 or $70) (this factor is between-pairs because pairings change after each round); and
MEDIUM = between-pairs binary factor to differentiate computer network negotiation from face-to-face negotiation.

within the allotted time. Occasional impasses occur with approximately equal frequency in both the $50 and $70 outside price conditions (see Table 1), so these occurrences do not appear attributable to fairness pressures.[10] We adopt the conservative approach of deleting all impasse observations in the Table 3 analysis. We obtain the same conclusions at similar or stronger statistical significance levels when we repeat the analysis using the final buyer bid or the average of the final buyer bid and the final seller ask as substitute measures of the transfer price in cases of impasse.[11]

Hypothesis 3 predicts that fairness pressures will exert a greater influence on negotiation outcomes when participants engage in face-to-face negotiation than when negotiations are computerized. Accordingly, H3 posits an interaction between the outside market price and the negotiation medium. Figure 2 plots this interaction, which is in the hypothesized

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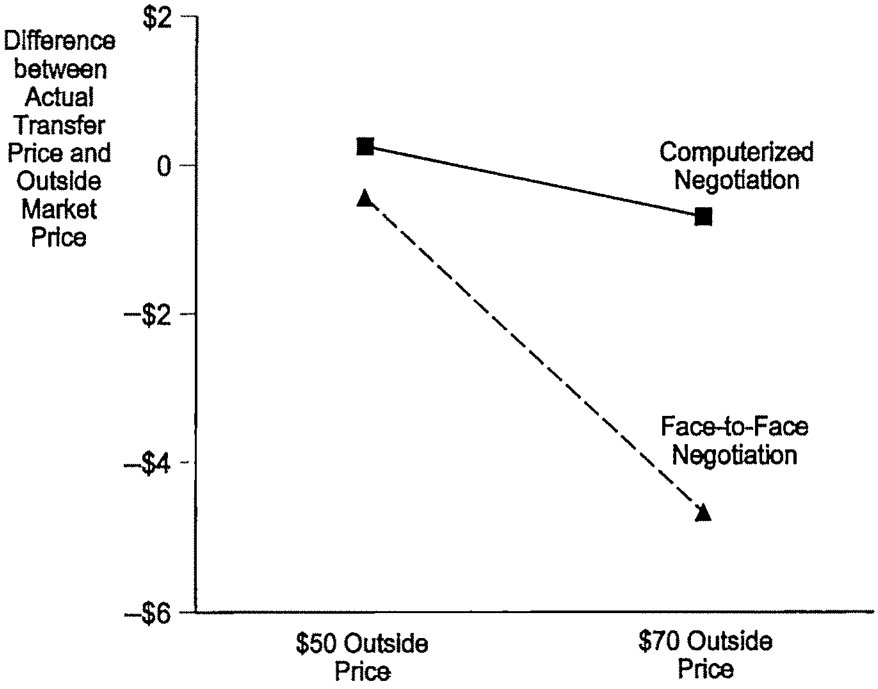
[10] Transfer pricing experiments by DeJong et al. (1989) and Mahenthiran et al. (1993) found moderate rates of impasse even in designs with perfect symmetry between buyer and seller profit functions. Thus, the occasional impasses in our study are neither unusual nor necessarily indicative of fairness-based pressures.

[11] The final buyer bid (i.e., the highest price the buyer was willing to pay) is a reasonable alternative metric in cases of impasse because fairness pressures in this setting arise primarily from buyers.

FIGURE 2
Difference between Actual Negotiated Transfer Price and Outside Market Price (Rounds 1 and 2)



direction. Panel A of Table 3 confirms that the OUTSIDE PRICE × MEDIUM interaction is statistically significant ($F = 3.46$; one-tailed $p = 0.039$).[12]

Given the significant interaction, we cannot interpret the main effect of the outside price manipulation without testing its simple effects within each negotiation medium. Panel B of Table 3 provides this analysis. The outside price manipulation does not affect the (actual negotiated transfer price − outside market price) difference in the computerized negotiation condition ($F = 0.48$; $p = 0.50$). A follow-up t-test indicates that when participants negotiate via the computer, actual transfer prices in the $70 outside price condition do not significantly differ from $70 ($t = -0.91$; $p = 0.41$). These findings indicate that although participants *expected* fairer transfer prices in the $70 outside price condition, they did not realize these expectations when negotiations were computerized.

A quite different result emerges under face-to-face negotiation, in which the outside price manipulation significantly affects actual negotiated prices ($F = 14.64$; $p < 0.01$). A follow-up t-test indicates that when participants negotiate face-to-face, actual transfer prices in the S70 outside price condition are significantly lower than $70 ($t = -3.72$; $p < 0.01$).

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[12] McNeil et al. (1996, 137–139) advise extending the usual logic of a one-sided hypothesis test to directional tests of single-degree-of-freedom interactions between two factors, as in H3. The OUTSIDE PRICE × MEDIUM interaction is also statistically significant at the 0.05 level when, instead of deleting the impasse observations, we substitute the final bid ($F = 5.21$; one-tailed $p = 0.02$) or the average of the final bid and ask ($F = 7.76$; one-tailed $p < 0.01$).

Thus, for face-to-face negotiations, both expected and actual transfer prices reflect a fairness effect.

These tests support H3, indicating that different methods of negotiation condition the extent to which the preferences for fairness observable in questionnaire-based transfer price expectations also materialize in actual transfer prices from cash-compensated negotiations.

V. SUPPLEMENTAL ANALYSES

Social Presence, Humanization, and Communication

To corroborate that our results reflect differing degrees of what Bazerman et al. (2000) termed "social presence," a post-experimental questionnaire asked participants to report the perceived degree of negotiation conflict on a seven-point Likert scale. There were separate questions for the $50 and $70 outside price conditions. Using this conflict measure as a dependent variable, we find a significant interaction between the outside market price and the negotiation medium ($F = 14.16$; $p < 0.01$).[13] Follow-up analysis of simple effects reveals that the $70 outside price significantly increased the degree of perceived conflict only when participants negotiated face-to-face. To the extent that social presence heightens the perceived conflict arising from the trade-off between fairness concerns and economic self-interest, this result corroborates the motivation underlying H3.

In developing H3, we cited studies indicating that face-to-face negotiation enhances social presence through both humanization and expanded communication. We intentionally combined both the humanization and communication aspects of face-to-face negotiation in order to detect any effect that might exist. The cost of this strategy is limited ability to disentangle the influence of humanization from that of communication. We can, however, provide one supplemental analysis, motivated from Bohnet and Frey (1999),[14] who found that a construct similar to what we term humanization ("personal identification" in their study) tends to lower the *dispersion* of outcomes, because it focuses attention on shared social norms. By contrast, enhanced communication *increases* dispersion, due to the broader message space.

A Levene test comparing face-to-face and computerized negotiation outcomes in the $70 outside market price setting reveals significantly greater dispersion in the face-to-face condition ($F = 6.66$; $p = 0.02$).[15] Following Bohnet and Frey (1999), this finding suggests that the enhanced communication in face-to-face negotiations contributed to our results. Nevertheless, both communication and humanization effects could occur in our setting, and may even interact (i.e., the communication effect could be enhanced by humanization and *vice versa*).[16] We leave more definitive investigation of these possibilities to future research.

Tying Expectations to Outcomes

Our primary tests analyzed transfer price expectations (Table 2) and outcomes (Table 3) separately. We now test price *movements*, defined as differences between initial expectations and either final negotiated transfer prices (cases with agreement) or the final bid and ask positions of each party (in cases of impasse). This approach retains the entire data set, including the occasional cases of impasse. We construct separate movement measures for buyers (Assembly) and sellers (Parts), coding these measures so that positive values indicate conciliatory price movements (i.e., increasing values for buyers and decreasing values for sellers).

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[13] We report only nondirectional (two-tailed) p-values in this section, reflecting its more exploratory nature.

[14] We are grateful to Iris Bohnet for suggesting this supplemental analysis.

[15] The Levene test is an analysis of variance applied to absolute deviations between observations and corresponding cell means. We favor it here because it is robust to violations of normality (Levene 1960).

[16] Researchers have found effects of alternative negotiation media even in studies that maintained equivalent communication opportunities (e.g., Mahenthiran et al. 1993; Drolet and Morris 2000), suggesting that there is more to the behavioral effects of different negotiation processes than simply the different communication opportunities they provide.

In a supplemental ANOVA with price movement as the dependent variable and OUTSIDE PRICE, negotiation MEDIUM, and negotiator ROLE (buyer or seller) as independent factors, the only significant effect is the three-way interaction among all three factors ($F = 5.46$; $p = 0.03$). Follow-up analyses of interaction contrasts and simple effects reveal that the two-way OUTSIDE PRICE × ROLE interaction is significant only in the computerized negotiation setting ($F = 7.80$; $p < 0.01$), not in the face-to-face setting ($F = 0.36$; $p = 0.55$). In turn, in the computerized setting, the simple effect of ROLE is significant only in the $70 (unfair) outside price condition ($F = 5.83$; $p = 0.02$), not in the $50 control condition ($F = 1.47$; $p = 0.24$).

The interpretation is as follows. Earlier we reported that for computerized negotiations, the outside market price manipulation influenced price expectations but not actual negotiated transfer prices. The analysis of price movements identifies the reason: buyers moved farther than sellers in the $70 market price condition. Buyers' final positions in the computerized negotiation setting were an average of $3.88 above their initial expectations when the outside market price was $70, indicating conciliatory movement toward sellers. By contrast, sellers' price movements in this setting were actually *anti-conciliatory*, increasing $0.81 above sellers' initial expectations instead of downward toward buyers' expectations. Thus, the dissipation of fairness from expectations to outcomes in the computerized negotiation setting reflects buyers' capitulation when sellers had a $70 outside price option. Such differences do not occur in the face-to-face setting, indicating that buyers both expected *and achieved* price concessions when the outside market price favored sellers and the parties negotiated face-to-face. These results are consistent with the reasoning underlying H3.

Repeated Negotiation

To this point our analyses have used the initial transfer price expectations and the negotiated price outcomes of Rounds 1 and 2, for comparability with Luft and Libby (1997). However, Franciosi et al.'s (1995, 938) argument that fairness reflects "expectations that are not sustainable" reveals the importance of examining expectations and outcomes over time.[17]

Rounds 3 and 4 were exact replicates of Rounds 1 and 2, although participants did not know this beforehand. We repeated the analyses of transfer price expectations and outcomes for Rounds 3 and 4, as discussed below. Figure 3 plots buyer expectations and final price outcomes across rounds for the $70 outside market price condition in both the computerized and face-to-face negotiation settings.[18]

For transfer price *expectations*, the analysis of Rounds 3 and 4 detects a significant OUTSIDE PRICE × MEDIUM interaction ($F = 6.29$; $p = 0.02$), indicating that expected transfer prices are no longer similar across the two negotiation settings. This finding contrasts with the similar expectations we observed across the two negotiation environments in Rounds 1 and 2 (Table 2). The likely reason for the interaction in Rounds 3 and 4 is

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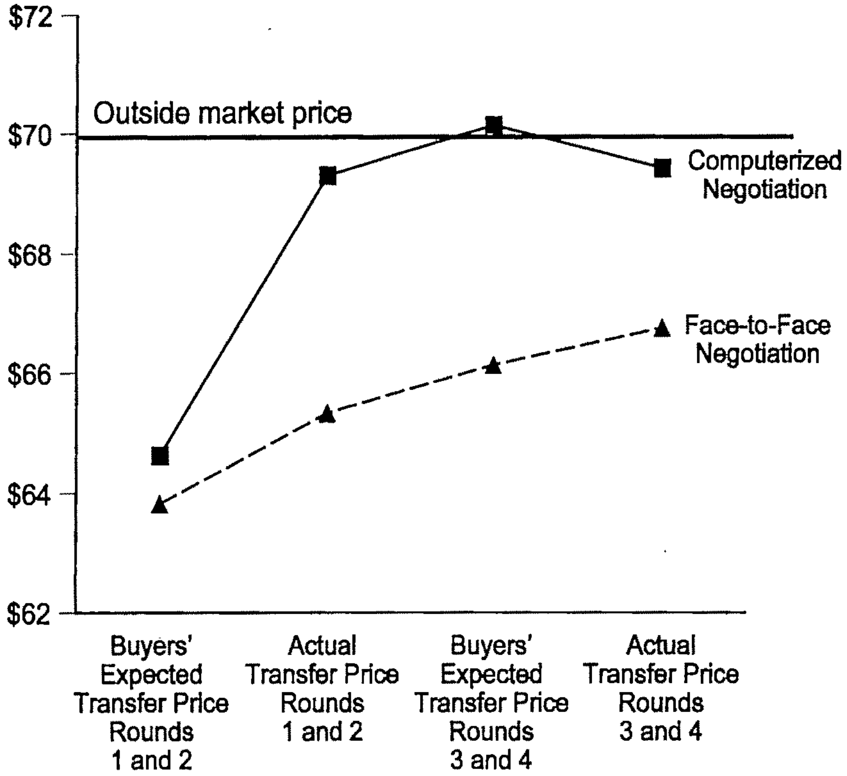
[17] Franciosi et al. (1995) supported their premise by extending the Kachelmeier et al. (1991) study of fairness to additional periods, showing that the fairness effect dampens over time.

[18] Figure 3 plots buyer expectations to minimize clutter and to focus on the expectations of the disadvantaged party. Table 1 shows both buyer and seller expectations.

FIGURE 3

Expected and Actual Transfer Prices across Rounds ($70 Outside Market Price Condition)



that participants (primarily buyers) in the computerized negotiation setting quickly learn not to expect concessions when the outside price is inequitable, given the outcome feedback from Rounds 1 and 2 that such concessions did not materialize. The revision in expectations is not nearly as dramatic in the face-to-face condition, likely because outcome feedback in this setting reinforces previous expectations of price concessions below the $70 outside price. Thus, results for expectations support the Franciosi et al. (1995) assertion that expectations of fairness are short-lived, ***but only when the negotiation setting suppresses rather than reinforces such expectations.***

For negotiated transfer price *outcomes*, Rounds 3 and 4 yield results similar to those for Rounds 1 and 2. There continues to be an OUTSIDE PRICE × MEDIUM interaction ($F = 18.88$; $p < 0.01$), indicating that the difference between the transfer price and the outside price is influenced by the outside price ($50 vs. $70) only when the parties negotiate face-to-face ($F = 41.55$; $p < 0.01$), but not when negotiations are computerized ($F = 0.01$; $p > 0.50$). In the face-to-face condition, Figure 3 suggests some movement toward the $70 outside market price from the first experience with this price (mean = $65.33) to the second experience (mean = $66.75), but the statistical significance of this movement over time is marginal ($t = 1.60$; two-tailed $p = 0.14$). The $66.75 average negotiated price under face-to-face negotiation in Rounds 3 and 4 is nearly identical to the $67.00 that Parts could obtain from the outside market *after* subtracting its $3.00 transaction cost penalty. Thus, in the final rounds, fairness manifests in face-to-face negotiation as the willingness of Parts to concede the entire transaction cost surplus (i.e., the firm's \$6.00 cost savings from transacting internally) to Assembly. In Rounds 1 and 2, Parts is willing to concede even more than this. Across all rounds, the fairness effect in both expectations and outcomes remains significant in the face-to-face condition.

Relative Profit Information

Our discussion has implicitly assumed that fairness-based pressures arise from disclosures that enable the buyer and seller to make relative profit comparisons. Luft (1997) stresses this point in explaining why fairness is important to accounting. However, neither Luft and Libby (1997) nor the analyses to this point explicitly vary relative profit information, leaving the door open for alternative explanations. It is possible, for instance, that participants are simply reacting to the absolute magnitude of their cash profits, irrespective of relative profit comparisons with others.

To provide direct evidence of the incremental role of accounting disclosures, we withheld relative profit information in a supplemental experimental condition with 16 additional M.B.A. participants. Case information in this supplemental condition enabled participants to calculate their own profits, but not the profits for the other negotiating party. We conducted this supplemental condition in the computerized negotiation setting only, because the unrestricted communication allowed in the face-to-face setting would have made it impossible to ensure against disclosure of relative profits.[19]

We analyze the effect of relative profit information on both price expectations and outcomes. For expectations, an ANOVA detects an interaction between relative profit disclosure and the outside market price manipulation ($F = 5.28$; $p = 0.03$).[20] Follow-up analysis of simple effects reveals that the outside market price manipulation affects the (expected − outside) price difference only when relative profit information is provided ($F = 9.07$; $p < 0.01$), not when it is withheld ($F = 0.08$; $p > 0.50$). This finding corroborates that the fairness effect in expectations does indeed result from the explicit disclosure of relative profit information. For actual transfer prices, by contrast, there is no similar interaction between the outside price manipulation and the disclosure of relative profits ($F = 0.22$; $p > 0.50$). The absence of an interaction for actual price outcomes is not surprising, given that we conducted this test in the computerized negotiation setting, in which a fairness effect did not emerge in actual prices even when relative profit information was provided (Table 3). Of course, it would be interesting to contemplate the effect of relative profit information in the face-to-face setting, where we observed a fairness effect in both expectations and outcomes. As noted above, however, even if we withheld relative profit information from the case in this setting, participants might well volunteer this information as part of their negotiation strategies.[21]

We also examine the effect of relative profit information on the *dispersion* of price expectations. Returning to the notions of egocentrism and motivated reasoning, it is likely that relative profit "information increases the number of plausible definitions of fair exchange that can be rationalized" (Luft 1997, 210). This reasoning suggests greater dispersion in expected transfer prices when relative profit information is available *and* the outside market price is inequitable.[22] Consistent with this reasoning, a Levene test for dispersion indicates that the disclosure of relative profit information significantly increases the variability of buyers' price expectations in the \$70 outside price condition ($p = 0.03$). Thus, we support Luft's (1997) assertion that relative profit information can fuel disagreement when it reveals inequity.

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[19] This is a case in which the control of a computerized setting with no other communication allowed is the only way to rigorously manipulate a construct of theoretical interest. Nevertheless, for initial transfer price expectations, the restriction of this supplemental test to the computerized setting should not be a limiting factor, because initial expectations did not vary between the computerized and face-to-face environments (see Table 2).

[20] The ANOVA also supports the egocentrism effect of different expectations between buyers and sellers, as predicted in H2 ($F = 7.53$; $p = 0.01$). There is no interaction between role and relative profit information ($F = 0.49$; $p = 0.49$), indicating that sellers expect systematically higher transfer prices than do buyers across both information conditions.

[21] In this case, relative profit disclosures could serve as independent verification of assertions made in negotiation, a potentially interesting aside that we leave for future research.

VI. CONCLUSIONS AND FUTURE DIRECTIONS

Parties to transfer price negotiation may say they expect fairness-based price concessions that moderate an inequitable outside market price (Luft and Libby 1997), but are they willing to act on these expectations? Our experimental study finds that the answer to this question depends on *how* parties negotiate transfer prices. When participants negotiate over a computer network restricted to bids, asks, and acceptances, the fairness effect in questionnaire-elicited expected transfer prices does not materialize in actual price negotiations. Conversely, when participants engage in face-to-face negotiations with unrestricted communication, both expected and actual transfer prices reflect fairness-based price concessions when the outside market price favors one of the parties. The likely reason why fairness effects differ across negotiation media is that face-to-face contact affords a more humanized negotiation and provides opportunities for persuasive communication, making it more difficult for the advantaged party to exercise the full economic bargaining power of an outside market price option. By contrast, computerized negotiation with no communication other than price proposals focuses participants' attention on their monetary profits, dampening any countervailing preferences for fairness.

Our computerized negotiation setting is extreme, because in practice, even computerized negotiations likely involve some face-to-face contact and do not restrict communication to bids, asks, and acceptances. Nevertheless, many field settings lie between the extremes of highly restricted computer negotiation and completely unrestricted face-to-face negotiation. For example, although email allows richer communication than merely the price proposals and acceptances of our computerized setting, it also avoids the humanization of face-to-face contact, thereby weakening inhibitions against socially undesirable behavior (Maruca 2000; Bazerman et al. 2000). Future research could attempt to differentiate the humanization and communication effects of alternative negotiation processes in accounting settings.

One implication of our findings is that firms can influence the extent to which an outside market price determines a negotiated internal transfer price by changing the means of negotiation. If the firm wants to align its internal transactions with the outside market, then impersonal negotiation mechanisms closer to our computerized setting can reduce the influence of nonmarket organizational frictions such as preferences for interdivisional fairness. Of course, at the extreme, the firm could simply *impose* the outside market price as the internal transfer price. Presumably, one reason many firms use negotiated instead of imposed transfer prices is that the firm benefits from self-determined internal operations, influenced but not constrained by outside market forces (Williamson 1975; Eccles 1985).

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[22] Relative profit information can increase bargaining costs if it exposes inequity (Camerer and Loewenstein 1993; Smith 1994). Conversely, in settings without such inequity, common information can *reduce* bargaining costs. See Haka et al. (2000) for a more complete discussion.

Moreover, in a multiperiod world, changing outside forces can lead to dysfunctional retribution behavior unless divisions learn to cooperate (Ravenscroft et al. 1993; Luft 1997). An important extension beyond both this study and the related research by Luft and Libby (1997) would be to examine explicitly the influence of multiperiod retribution opportunities in accounting-based negotiations.

We close by noting the broader implications of our study for experimental research in accounting. Researchers from the experimental economics tradition often advise abstract, tightly controlled experiments similar to our computerized negotiations (e.g., Friedman and Sunder 1994). Conversely, researchers from the cognitive-behavioral tradition often advise context-rich experiments that capture the natural environment, similar to our face-to-face negotiations (e.g., Gibbins and Jamal 1993). This study reinforces comments recently advanced by Camerer (1997) and Haynes and Kachelmeier (1998) that there are testable empirical questions underlying these contrasting calls for control vs. realism, with implications for both theory and practice. Varying the means of negotiation enabled us to more subtly probe the influence of fairness pressures on transfer prices than if we had simply followed the experimental economics or social psychology paradigms. We encourage similar explorations in future research, exploiting the comparative advantage of the accounting literature in integrating both economic and psychological insights.

APPENDIX
CASE INFORMATION

INFORMATION FOR ASSEMBLY MANAGER OF XYZ COMPANY

[This case version is for participants assigned to ASSEMBLY. We modified the role wording appropriately for participants assigned to PARTS.]

Assume you are the manager of the ASSEMBLY division of XYZ Company. XYZ has two divisions:

(1) PARTS, which manufactures parts for use by ASSEMBLY and also for sale to outside customers, and
(2) ASSEMBLY, which buys parts from the PARTS division and from outside suppliers, and assembles them into final products for sale to outside customers.

You can buy parts either from XYZ's PARTS division or from outside suppliers. However, it is more convenient for you both if you trade with each other rather than with outsiders, and your relationship has been reasonably good in the past.

The transfer price that you pay when you buy from PARTS is negotiated by the two divisions, who can choose whatever transfer price is satisfactory to both of them. Both of you are free to consider whatever factors seem appropriate to you in setting the price.

Suppose that you and the manager of the PARTS division are negotiating the transfer price of a part for the next year. If you and PARTS can't agree on a price, then you can always buy these parts from outside suppliers and PARTS can sell them to outside customers. You will each lose a little when you trade with outsiders, however. PARTS has additional marketing costs (collection expenses, advertising, etc.) when it deals with outsiders that is doesn't have when it sells to ASSEMBLY, and ASSEMBLY has additional purchasing costs (quality testing, purchasing department costs, etc.) when it deals with outsiders that it doesn't have when it buys from PARTS.

[In the supplemental condition with relative profit information withheld, we disclosed only the amounts relevant to the participant's assigned role.]

You already know that the PARTS division's production cost per unit is **$20.00** and that ASSEMBLY's final profit is **$80.00** less the price paid for the part. Thus, once you and the manager of PARTS decide on the transfer price, you both know how much you will make on this trade, as follows:

PARTS: TRANSFER PRICE − $20.00 cost = PARTS DIVISION PROFIT

ASSEMBLY (YOU): $80.00 − TRANSFER PRICE = YOUR FINAL PROFIT

The table below shows your total profits per unit from the internal trade at selected transfer prices, along with the PARTS division's profits.

Transfer Price	$20	$30	$35	$40	$45	$50	$55	$60	$65	$70	$80
PARTS profit	$ 0	$10	$15	$20	$25	$30	$35	$40	$45	$50	$60
ASSEMBLY profit	$60	$50	$45	$40	$35	$30	$25	$20	$15	$10	$ 0

Outside Market Price

Suppose the current market price for these parts—what you will pay if you buy them from outsiders, and what PARTS will get if they sell to outsiders—is **$70.00**. However, as noted above, PARTS would have additional marketing costs and ASSEMBLY would have additional purchasing costs if an internal transfer price agreement cannot be reached. Suppose these additional costs are **$3.00** for each division. In this case, if you can't agree to trade with each other, and you both trade with outsiders instead, then your profits will be as follows:

PARTS: $70.00 market price − $3.00 marketing costs − $20.00 cost
= $47.00 PARTS DIVISION PROFIT

ASSEMBLY (YOU): $80.00 − $70.00 market price − $3.00 purchasing costs
= $7.00 PROFIT FOR YOU

[The other market price condition substituted $50 as the current market price, with corresponding changes in the profit calculations.]

Your Compensation

You can think of this negotiation as involving the transfer price between divisions for thousands of parts. However, your compensation for this experiment is based on your **PROFIT PER UNIT.**

When we conduct the actual computerized *[we deleted the word "computerized" in the face-to-face condition]* negotiation for this case, you will be paid your profit per unit *in cash* if this case is randomly selected from the four we will conduct. To sum, if this case is selected for payment at the end of the session (a 25 percent chance), your payment will be determined as follows:

> **If you agree upon a transfer price before the negotiation period expires, then you get paid $80.00 − the transfer price, and the PARTS manager with whom you are paired would be paid the transfer price − $20.00.**

If you do not agree upon a transfer price before the negotiation period expires, then you get paid \$80.00 − \$70.00 default market price − \$3.00 additional cost = \$7.00, and the PARTS manager with whom you are paired would be paid \$70.00 default market price − \$3.00 additional costs − \$20.00 parts cost = \$47.00.

SUMMARY

The exact transfer price is important to both of you, because it has a significant effect on your divisional profit. Your division (ASSEMBLY) and PARTS are about the same size in terms of invested capital and divisional profit, and the price of the parts is equally important to both of you.

Think about the level of profit you would like to make and the level of profit you can get the PARTS manager to accept, given the outside market opportunities and your relationship within the firm. This should give you some idea of what price to aim at in negotiating with the PARTS manager.

Please answer these two questions, after which we will be ready to conduct the computerized negotiation *[we deleted the word "computerized" in the face-to-face condition]*:

What would you expect the final negotiated transfer price to be? \$________

What is the lowest transfer price you think PARTS would accept in this case? \$________

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Disclosure-Disciplining Mechanisms: Capital Markets, Product Markets, and Shareholder Litigation

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ABSTRACT: This paper demonstrates that a firm's trade-offs between reporting good news to reduce the cost of capital and bad news to minimize proprietary costs can induce the firm's manager to provide truthful disclosures when the opposing effects balance each other. We also show that greater proprietary costs can make a firm's disclosures more credible, increase the frequency of voluntary adverse disclosures, and improve the disclosing firm's welfare.

Further, we find that potential shareholder litigation can interact with capital and product markets' influences to make voluntary disclosures more credible, but only under certain circumstances. For example, although product market competition can complement the capital market effects in inducing the manager to provide truthful disclosures, shareholder litigation cannot complement the capital market in the same way. Nevertheless, while shareholder litigation can never induce misreporting, a very strong product market influence can prompt a firm to underreport its true economic condition.

Keywords: *voluntary disclosures; cost of capital; proprietary costs; shareholder litigation; disciplining mechanism.*

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I. INTRODUCTION

This paper examines firms' unaudited voluntary disclosures such as management earnings forecasts. Firms may disclose good news to reduce their cost of capital and bad news to reduce both competition in their product market and exposure to litigation damages. By analyzing these opposing informational motives for disclosures, we provide greater insight into firms' disclosure decisions. The AICPA Special Committee on Financial Reporting (AICPA 1994, Chapters 4 and 5) has also emphasized the need for further research on the determinants and consequences of voluntary disclosures.

While the impact of a firm's voluntary disclosures depends critically on their credibility, there is little analytical accounting literature on the determinants of the credibility of unaudited disclosures, with the exception of Newman and Sansing (1993) and Gigler (1994). Disclosure models typically assume that voluntary disclosures must be truthful (Grossman 1981; Milgrom 1981; Verrecchia 1983; Dye 1985; Wagenhofer 1990; Feltham and Xie 1992), and hence, these models do not address the mechanisms that make voluntary disclosures credible.

In contrast to most analytical disclosure literature, empirical research on shareholder litigation concludes that firms sometimes fail to make complete and honest disclosures (Skinner 1994; Francis et al. 1994; Baginski et al. 2002). These studies also suggest that the threat of shareholder litigation affects the nature and extent of firms' voluntary disclosures. Despite this growing empirical interest in the effects of shareholder litigation on the quality of firms' voluntary disclosures, only Trueman (1997) models the relation between shareholder litigation and voluntary disclosures, and his model assumes that any disclosures must be truthful.[1] To address this gap in the literature, we examine how shareholder litigation affects the credibility of disclosures that need not be truthful. We also compare the ability of product market competition and shareholder litigation to complement the capital market in disciplining firms' voluntary disclosures.

Neither the capital markets nor the product markets, acting in isolation, can induce managers to provide truthful disclosures in a single-period environment. A firm would make the most favorable disclosure if only the capital market used the disclosure, and would make the least favorable disclosure if only the product market used the disclosure. However, we show that when both capital and product markets use a firm's disclosures, these offsetting informational demands can induce the firm's manager to issue truthful disclosures under some circumstances.

We begin with a benchmark setting in which shareholders cannot sue the firm. An incumbent firm makes a voluntary disclosure about its future prospects to both the capital and product markets. If the disclosure is credible and favorable, then the capital market provides the firm with equity capital at a lower cost. However, a favorable disclosure also increases the probability that a rival firm will enter the incumbent's product market, imposing a proprietary cost on the incumbent. In contrast, issuing a credible adverse disclosure increases the firm's cost of capital, but decreases the firm's expected proprietary loss by discouraging the rival firm's entry. We demonstrate that if a favorable disclosure would increase the incumbent's proprietary cost sufficiently, the mutual discipline created by the trade-off between reducing the cost of capital versus discouraging the rival from entering can induce the firm to report truthfully. On the other hand, when the capital market consequences of disclosures outweigh the product market effects, favorable disclosures are more common and less credible.

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[1] In further contrast to Trueman (1997), market prices and litigation damages are endogenous in our model.

We extend in several ways Gigler's (1994) "mutual discipline" result, whereby opposing informational demands of product and capital markets lead the firm to disclose truthfully. First, we show more precisely how equilibrium disclosures vary as a function of the firm's economic environment. That is, we describe how the firm's disclosures vary with the magnitude of the proprietary costs of disclosure, the amount of capital the incumbent firm must raise, the rival's cost to enter the incumbent's market, and the magnitude of the incumbent's outcomes. Second, our more stylized model permits us to predict when the firm will overreport versus underreport the realized outcome. Finally, we examine the effectiveness of shareholder litigation as an alternative to product market competition in complementing capital markets in disciplining firms' disclosures.

Our analysis of firms' disclosure behavior in the no-litigation regime yields two interesting insights. First, when the firm's favorable prospects are large enough, the firm makes more informative disclosures as its proprietary costs increase, in contrast to earlier studies suggesting that proprietary costs discourage disclosures (e.g., Verrecchia 1983). In contrast, when the firm's favorable prospects are not as large, the firm makes false adverse disclosures to avoid sufficiently large proprietary costs. In this way, we highlight how the interaction of the magnitudes of proprietary costs and the firm's outcomes can produce both truthful and false disclosures.

The second insight is that a greater potential proprietary cost of disclosure can actually benefit an incumbent firm. Increasing the proprietary cost can produce both costs and benefits for the firm. By reducing the firm's net outcome, a greater proprietary cost increases the firm's expected cost of capital, as measured by the expected fraction of the firm that investors demand in return for their investment in the firm. However, a greater proprietary cost can also benefit the firm by reducing its *expected* proprietary cost. With a greater proprietary cost, the rival may enter the firm's product market only when the firm makes a credible favorable disclosure versus always entering after observing the incumbent's uninformative disclosure that results from a smaller proprietary cost. The shareholders' welfare will increase because the reduced likelihood of the rival's entry decreases the *expected* proprietary cost by an amount that exceeds the expected increase in capital market costs.

The second part of our paper identifies two ways in which shareholder litigation enhances the credibility of firms' voluntary disclosures by complementing other disclosure-disciplining mechanisms. First, litigation expands the circumstances (i.e., the range of proprietary costs) under which the firm voluntarily makes truthful adverse disclosures. Second, when the firm discloses truthfully with some probability less than 1, the threat of litigation increases that probability.

However, shareholder litigation does not always effectively complement other disclosure-disciplining mechanisms. When proprietary costs are very large, shareholder litigation will be redundant because the firm already discloses truthfully, and when proprietary costs are very small, shareholder litigation will be insufficient to induce truthful disclosures. Likewise, when the rival firm's entry cost is very small the rival will always enter, and when the entry cost is very large the rival will never enter, regardless of the incumbent's disclosure. In these circumstances, shareholder litigation may have no incremental effect on the credibility of the firm's disclosures.

Further, shareholder litigation is less effective in certain ways than product market competition in complementing capital market effects to generate truthful disclosures. Specifically, even without shareholder litigation, the product market effects of disclosures can complement the capital market and induce the manager to issue only truthful disclosures under certain circumstances. This result follows because the incumbent's truthful disclosure never reduces the rival firm's incentive to enter, and hence, never reduces the ability of product market competition to complement capital market effects. In contrast, without product market competition, shareholder litigation cannot complement capital market effects to produce truthful reporting. If the firm always disclosed truthfully, then shareholders would have no incentive to sue. Anticipating this response, the firm would have incentive to overreport to reduce its cost of capital, preventing a truthful disclosure equilibrium.

Although product market competition can sometimes be more effective than shareholder litigation as a complement to capital market influences in disciplining the firm's disclosures, in other circumstances, product market competition can actually inhibit truthful disclosures. For example, when proprietary costs are sufficiently great relative to the capital market benefits of favorable disclosures, the firm may voluntarily understate its actual outcome to discourage the rival from entering. Further, when operating in isolation, either the capital market or the product market would inhibit truthful disclosures. In contrast, the prospect of shareholder litigation never prompts the firm to misreport.

Our analysis relates to several streams of recent empirical research. For example, we establish when mutual discipline makes disclosures credible, consistent with Botosan (1997) and Sengupta (1998) who demonstrate that greater levels or quality of disclosures can reduce the firm's cost of equity and debt, respectively. We also show that mutual discipline requires that the proprietary costs of a favorable disclosure be sufficiently large to offset the capital market costs of an adverse disclosure. Consistent with our results, Clarkson et al. (1994) find that firms are more likely to make adverse disclosures when they face greater proprietary costs from competitive entry. Similarly, Shin (2001) provides empirical evidence that product market competition can discourage firms' favorable disclosures.

Skinner (1994) and Kasznik and Lev (1995) investigate how shareholder litigation influences voluntary disclosures and conclude that the threat of such litigation explains why some firms voluntarily disclose bad news. However, it remains unclear why other firms with bad news choose not to make voluntary adverse disclosures. For instance, Francis et al. (1994) find that only 15 percent of their sample of "at risk" firms voluntarily disclose bad news, suggesting that factors besides litigation influence firms' adverse disclosure decisions. Our analysis offers an explanation for these mixed findings because we show how capital and product market considerations, together with shareholder litigation, can induce some firms to make voluntary adverse disclosures while other firms, faced with higher capital costs and lower proprietary costs, do not.

Our results also have empirical implications for the relation between alternative legal liability regimes and firms' disclosure decisions. An implication of our analysis is that more stringent legal liability in the form of greater litigation damages leads firms to issue more adverse disclosures. Consistent with this result, Baginski et al. (2002) find that more stringent legal liability for disclosures leads U.S. firms to provide a higher incidence of unfavorable earnings forecasts than do Canadian firms, which face a less stringent liability regime.

Further, our analysis suggests that more stringent legal liability can enhance shareholders' welfare by making their firms' disclosures more credible under specific circumstances. Ali and Kallapur (2001) examine the security price consequences of the Private Securities Litigation Reform Act of 1995, which reduced firms' legal liability by restricting private litigants' ability to sue for investment losses from securities fraud. Consistent with our finding that legal liability and shareholder wealth can be positively associated, they conclude that the reduction in legal liability stemming from the Act decreased shareholder wealth. The empirical evidence is mixed, however, because Johnson et al. (2000) examine the same legislation and conclude that the Act (which reduced legal liability) increased shareholder wealth.

This paper also predicts that, under certain conditions, firms raising more capital will issue more false favorable disclosures. This prediction is consistent with Korajczyk et al.'s (1991) empirical findings that disclosures preceding an equity issue are more likely to contain favorable information, but is seemingly inconsistent with Frankel et al.'s (1995) evidence that management forecasts are unbiased when issued in conjunction with the firm raising capital. A potential explanation for the discrepancy between our prediction and Frankel et al.'s (1995) results is that our prediction is contingent on the magnitude of proprietary costs, a factor that their study does not control. Our analysis also predicts that as the firm's good news becomes even more favorable, the firm will issue more false favorable disclosures. While Korajczyk et al. (1991) do not test this prediction directly, they do find that information releases preceding an equity issue contain "unusually good news."

The notion that the simultaneous presence of multiple parties with opposing informational demands can provide a balancing influence on a firm's disclosure decisions has been studied in other contexts. For instance, Gertner et al. (1988) examine a firm's choice of its capital structure to signal its private information to a competing firm. Farrell and Gibbons (1989) also study the relative credibility of disclosures with multiple audiences when one disclosure is potentially private. However, neither of these models examines the effectiveness of shareholder litigation as an alternative disclosure-disciplining mechanism.

As a benchmark setting, Section II analyzes the basic model consisting of capital and product market uses of a firm's disclosures in the absence of shareholder litigation. Section III examines the effect of shareholder litigation on the firm's disclosure behavior. Section IV concludes the paper. Appendix A explains how we derive the pricing equation and litigation damages in the environment with litigation. All proofs appear in Appendix B.

II. DISCLOSURE WITHOUT LITIGATION

Model

Consider a single-period setting in which a firm's first-generation shareholders must raise capital, k, by selling a fraction of their shares to second-generation shareholders. All players are risk neutral. The firm's manager privately observes a perfect signal about the outcome (terminal dividend), $\tilde{x} \in \{x_1, x_2\}$, where $0 < x_1 < x_2$.[2] For expositional convenience, we assume that the two outcomes are equally likely, and that $k < x_1$. We refer to x_1 as an *adverse outcome* and to x_2 as a *favorable outcome*. Likewise, the firm is a *low type* when x_1 occurs and a ***high type*** when x_2 occurs.

After realizing the outcome, the manager makes a disclosure $\hat{x}_i$, indicating that x_i occurred, for i = 1,2. Consistent with the previous literature on disclosures (e.g., Verecchia 1983; Dye 1985; Gigler 1994), the manager chooses a disclosure to maximize the expected welfare of the first-generation shareholders. We assume that shareholders' responsibility for losses is limited to their investment in the firm (limited liability). Finally, in this section we assume that shareholders cannot sue for false disclosures.

Both the capital market and the product market (a rival firm) observe the firm's disclosure. Based on the firm's disclosure, investors price the firm at $P_i = P(\hat{x}_i)$, updating their priors about the firm's terminal dividend. The rival firm likewise uses the incumbent's disclosure to update its beliefs before deciding whether to enter the incumbent's product market. If the rival enters the incumbent's product market, then the rival incurs a cost, k^R, and receives the same outcome as the incumbent. Therefore, the rival will enter if and only if $E[\tilde{x}|\hat{x}_i] > k^R$. The increased competition from the rival's entry produces a proprietary loss of L for the incumbent firm.[3] To avoid uninteresting complications from prospective bankruptcy of the incumbent, we assume that $L < x_1$.

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[2] Although we describe the manager as directly observing a perfect signal about the outcome, the manager's private information could also reflect any event that affects her estimate of the value of the firm. Examples of such events include an ongoing merger negotiation, development of a new product or process, obtaining a new order, geological determination of underground resources, and a private contract of strategic significance such as a joint venture.

After the firm discloses $\hat{x}_i$, investors supply capital of k to the first-generation shareholders in return for a fractional ownership $\alpha_i \equiv \alpha(\hat{x}_i)$ in the firm. Capital market competition ensures that $k = \alpha_i P_i$, indicating that investors' expected returns equal their investment. The first-generation shareholders commit to hold their remaining shares until the end of the period. Figure 1 illustrates the timeline of events.

Analysis without Litigation

In a pooling equilibrium, the market prices all firms identically, independent of their disclosures, i.e., the market "pools" all firms together. In contrast, in a separating equilibrium, the market prices firms differentially based on their disclosures, i.e., the disclosures "separate" the different types of firms. Although a pooling equilibrium could involve any disclosure or nondisclosure made by both high- and low-type firms, we assume for ease of exposition that both firm types report $\hat{x}_2$ in a pooling equilibrium.[4]

Let the firm's disclosure strategy be a probability density function, ρ_{ji}, indicating that after realizing outcome x_j the firm discloses $\hat{x}_i$ with probability ρ_{ji}. Similarly, let $\mu_i \equiv \mu(\hat{x}_i)$ be the probability that the rival enters after the incumbent discloses $\hat{x}_i$. In equilibrium, the firm's disclosure strategy (ρ_{ji}) maximizes the expected payoff to the first-generation shareholders, $k + (1 - \alpha_i)[x_j - \mu_i L]$. In the firm's expected payoff, k is the capital new investors provide, and $(1 - \alpha_i)[x_j - \mu_i L]$ indicates that first-generation shareholders retain the fraction $(1 - \alpha_i)$ of the expected net payoff, which equals the outcome x_j reduced by the expected

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[3] For simplicity we describe L as a fixed parameter, but none of our analysis would change if the proprietary loss, L, were stochastic and drawn from a support of $(0,x_1)$, provided the realized value of the proprietary loss, L, were publicly observable before the firm made its disclosure decision.

[4] Because all parties know that the manager always observes the firm's outcome ($\tilde{x}$), nondisclosure is another possible report that the firm could make in a pooling equilibrium without litigation. In such an environment, nondisclosure and a pooling equilibriium produce the same posterior beliefs about the firm's outcome because investors have no basis for updating their beliefs in either case.

FIGURE 1
Timeline 1: Voluntary Disclosure without Shareholder Litigation

t = 0	t = 1	t = 2
↑	↑	↑
Original shareholders are endowed with shares in a firm.	The manager privately observes the firm's terminal dividend, x_j. The manager discloses $\hat{x}_i$. The manager must raise k by selling fraction α_i of the firm at price P_i.	The rival firm's entry decision is publicly observed. Shareholders cannot sue the firm for any misreporting. Shareholders consume the net payoff.

proprietary cost, $\mu_i L$. All players (the incumbent firm, investors, and the rival) anticipate that other players will act to maximize their own expected net payoffs, given their information sets. That is, each player anticipates sequentially rational strategies of other players, as determined by their consistent, Bayesian-updated beliefs.

The form of the disclosure equilibrium will depend on the firm's trade-off between the capital and product market effects of its disclosure. In a separating equilibrium, a favorable disclosure ($\hat{x}_2$) reduces the firm's cost of capital (smaller α_i), but also increases the expected proprietary cost from the rival's entry. In contrast, an adverse disclosure ($\hat{x}_1$) discourages entry by the rival but increases the cost of capital (larger α_i). In a separating equilibrium, these two countervailing effects create a mutual discipline that makes the disclosures credible even without the threat of an audit, SEC investigation, or shareholder litigation. As shown below, in such a separating equilibrium, if the incumbent issues an unfavorable disclosure, then the rival does not enter and investors price the firm at $P_1 = x_1$. Conversely, if the incumbent issues a favorable disclosure, then the rival does enter and investors price the firm at $P_2 = x_2 - L$. Investors demand a fraction of the firm that equates their return to the amount of capital, k, that they provide, which yields $k = \alpha_i P_i$, and in a separating equilibrium, $k = \alpha_1 x_1 = \alpha_2(x_2 - L)$. The mutual discipline necessary for a separating equilibrium also requires that the rival's entry decision depend on the firm's disclosure. That is, the rival's cost to enter, k^R, must be neither so small that the rival always enters nor so large that the rival never enters.

The following analysis considers mixed-strategy equilibria only when no pure-strategy equilibrium exists. Proposition 1 predicts how the firm's disclosure behavior will vary with the economic environment as characterized by the parameters L, k^R, and x_2 for circumstances most conducive to credible disclosures. We focus initially on these combinations of parameter values because they reflect the richest interaction of capital market and product market influences on the firm's disclosures. Corollary 1 then completes the characterization for the remaining parameter values.

Proposition 1 (No Shareholder Litigation): When the rival's cost to enter falls in an intermediate range ($k^R \in [x_1, (x_1 + x_2)/2)$) and the incumbent's favorable outcome is sufficiently large ($x_2 \geq x_2^*$):[5]

(a) There exists a threshold, L^*, such that for all proprietary costs, $L \in [L^*, x_1)$, the firm will always report truthfully (i.e., disclose $\hat{x}_1$ after realizing x_1 and $\hat{x}_2$ after realizing x_2);[6]

(b) For proprietary costs less than or equal to a second threshold, L^{**}, where $L^{**} < L^*$, only a pooling equilibrium will occur in which the firm will always disclose $\hat{x}_2$; and

(c) For all intermediate proprietary costs, $L \in (L^{**}, L^*)$, no pure-strategy disclosure equilibrium exists. However, a partial separating equilibrium will exist in which the firm will report truthfully after realizing x_2, but will mix reports of $\hat{x}_1$ and $\hat{x}_2$ after realizing x_1.

In all three cases, the rival will enter only when the incumbent firm discloses $\hat{x}_2$.

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[5] The threshold is $x_2^* = (x_1^2/k) + x_1$; e.g., for $k = 0.5x_1$, $x_2^* = 3x_1$.

[6] For certain off-equilibrium beliefs, a pooling equilibrium could also exist in the setting specified in Proposition 1(a). However, common reasonableness checks on off-equilibrium beliefs such as the Cho and Kreps' (1987) intuitive criterion eliminate such a pooling equilibrium. Likewise, using a criterion that focuses on the more informative equilibrium would also eliminate the pooling equilibrium as being less informative than the truthful reporting equilibrium.

Although previous research suggested that potential proprietary costs discourage complete and truthful disclosures (e.g., Verrecchia 1983), Proposition 1 demonstrates that disclosures can become more complete and truthful as the proprietary cost (L) increases.[7] With $x_2 \geq x_2^*$, when the proprietary cost is sufficiently small (i.e., for $L \leq L^{**}$), the incumbent's disclosures have no credibility in the resulting pooling equilibrium (see Region A of Figure 2). As the proprietary cost increases (i.e., for $L \in (L^{**}, L^*)$), firms make partially credible disclosures (Region B of Figure 2). Over this interval, adverse disclosures ($\hat{x}_1$) are fully credible, but favorable disclosures ($\hat{x}_2$) are only partially credible. Finally, for large proprietary costs (i.e., for $L \in [L^*, x_1)$), all disclosures are fully credible (Region C of Figure 2). As the proprietary cost increases, the incumbent's incentives to report adverse information to discourage the rival's entry (i.e., the product market effects of disclosure) increase relative to the incumbent's incentives to report favorable information to reduce the cost of

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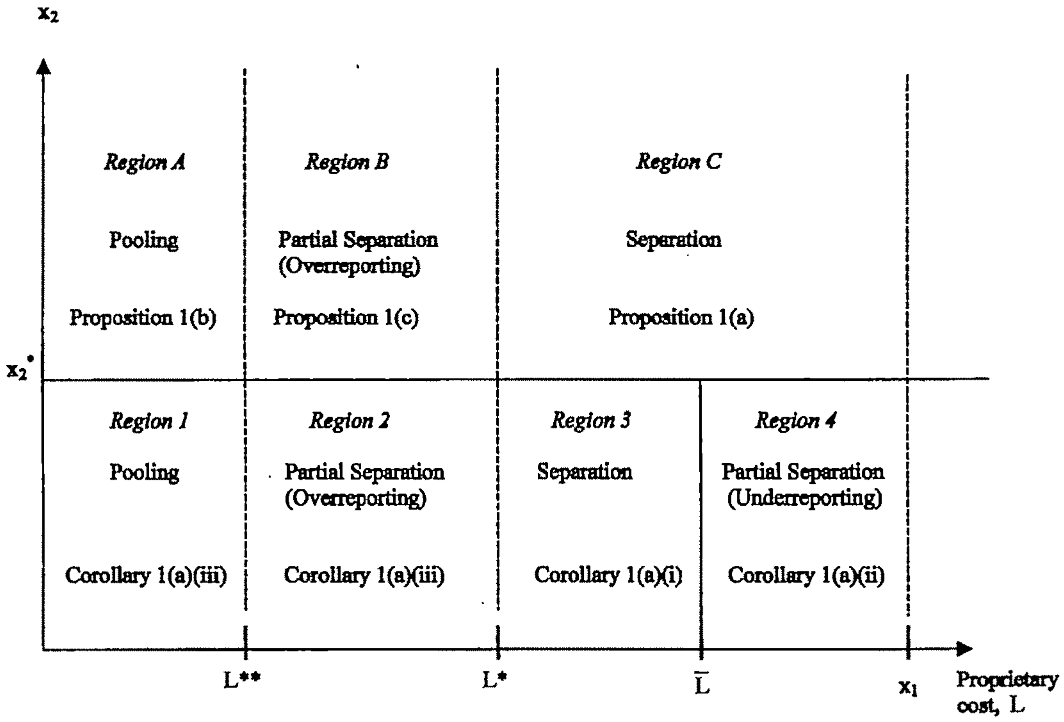
[7] Although the underlying intuition for voluntary disclosures in our analysis is similar to the rationale in the signaling literature, where a firm uses a costly signal to communicate its type, important distinctions also arise. First, in the signaling literature, the informed party makes a decision imposing the cost of the signal on itself, whereas in our model the uninformed party (the rival firm) makes a decision that imposes a cost on the informed party (the incumbent firm). Second, multiple users responding to the firm's disclosure in our setting can render the costs and benefits imposed on the firm stochastic, whereas the self-imposed cost of signaling is deterministic in most signaling models.

FIGURE 2
Disclosure Equilibria without Shareholder Litigation[a]



[a] Figure 2 assumes that $x_1 \leq k^R < (x1 + x2)/2$. For other values of k^R, a pooling equilibrium exists.

raising new capital (i.e., the capital market effects). When proprietary costs become sufficiently large, the mutual discipline created by the combination of product market and capital market effects gives the firm sufficient incentive to disclose completely and truthfully. In contrast, when proprietary costs are smaller, mutual discipline is limited or absent because the capital market incentives to report favorable information dominate the product market incentives to report adverse information, and as a result, the firm's disclosures are not fully credible.

The assumption in Proposition 1 that x_2 is sufficiently large ($x_2 \geq x_2^*$) ensures that for a high-type firm, the capital market benefit from disclosing a favorable outcome exceeds the product market proprietary cost from the rival's entry. Corollary 1, below, shows that when x_2 is not sufficiently large, product market disclosure consequences could dominate so that a firm with a favorable outcome (x_2) would underreport, i.e., disclose an unfavorable outcome, $\hat{x}_1$, to avoid the proprietary cost.

Finally, the equilibria in Proposition 1 require bounds on the rival's cost of entry, k^R (i.e., $k^R \in [x_1, (x_1 + x_2)/2)$) to ensure that the rival's entry decision is sensitive to the firm's report. Specifically, for costs of entry within the specified interval, the rival always enters *except* following a credible adverse disclosure ($\hat{x}_1$). A separating equilibrium requires that the incumbent's adverse disclosure be credible, thereby discouraging the rival from entering the market.

Proposition 1 integrates the strategic interests of first-generation shareholders, potential investors (second-generation shareholders), and the rival firm in equilibrium. In doing so, it demonstrates how the informational conflict between the capital and product markets can both (1) lend credibility to a firm's voluntary disclosures via the mutual discipline of opposing capital and product market incentives, and (2) provide incentives for the firm to make voluntary adverse disclosures.

The results in Proposition 1 help us provide insights into how a firm's proprietary costs interact with its cost of capital to determine the welfare of its first-generation shareholders. First, Remark 1 characterizes the effect of product market competition on the firm's cost of capital.

Remark 1: Assume that the rival's cost to enter, k^R, falls in the intermediate range (i.e., $k^R \in [x_1, (x_1 + x_2)/2)$). Then the incumbent firm's expected cost of capital, as measured by the expected fraction of the firm that must be sold to raise capital of k, is strictly greater with product market competition than without.

Remark 1 follows because the potential loss to the firm from product market competition reduces the firm's expected terminal value, thereby increasing the fraction of the firm that new investors require in return for their investment. Next, Remark 2 establishes that notwithstanding the result in Remark 1, greater proprietary cost can actually increase the welfare of the first-generation shareholders.

Remark 2: Let the rival's cost to enter, k^R, fall in the intermediate range (i.e., $k^R \in [x_1, (x_1 + x_2)/2)$), the incumbent's favorable outcome be sufficiently large ($x_2 \geq x_2^*$), and the initial proprietary cost be $L \leq L^{**}$. Then increasing the proprietary cost from L to L′, where $L' \geq L^* > L^{**} \geq L$, strictly increases the first-generation shareholders' welfare if and only if $L > 0.5L'$.

The counterintuitive result in Remark 2 follows because increasing the proprietary cost produces both costs and benefits for the firm. On the cost side, the increased proprietary cost means that the firm must give investors a larger fraction of the firm in order to raise a given amount of capital. The corresponding benefit is that with a larger proprietary cost, L', the rival will enter only when the firm reports $\hat{x}_2$ (which occurs with probability 0.5) in the resulting separating equilibrium as opposed to always entering (probability 1) in a pooling equilibrium produced by the smaller proprietary cost, L. That is, the larger proprietary cost, L', reduces the likelihood the rival will enter, thereby reducing the probability that the incumbent firm will incur the proprietary cost L'. The impact of the reduction in the probability of incurring L' outweighs that of the increase from L to L', such that the *expected value* of the proprietary cost declines. Remark 2 identifies a necessary and sufficient condition under which the reduction in the incumbent firm's expected proprietary cost exceeds the increase in expected capital market costs, so that the greater proprietary cost actually improves the shareholders' overall expected welfare.

Next, Corollary 1 analyzes equilibria for the remaining parameter values not covered by Proposition 1.

Corollary 1 (No Shareholder Litigation; other parameter values)

(a) For intermediate values of the rival's cost to enter (i.e., $k^R \in [x_1, (x_1 + x_2)/2)$), if the favorable outcome is not large (i.e., if $x_2 < x_2^{\ddagger}$), then in addition to the thresholds L^* and L^{**} identified in Proposition 1, there also exists a third threshold $\bar{L}$, where $L^* < \bar{L} < x_1$. Furthermore:

- (i) For all $L \in [L^*, \bar{L}]$, the firm will always report truthfully;
- (ii) For all $L \in (\bar{L}, x_1)$, a partial separating equilibrium will occur in which the low-type firm always reports truthfully while the high-type firm mixes truthful (favorable) and false (adverse) disclosures;
- (iii) for all $L \in (L^{**}, L^*)$, a partial separating equilibrium will exist in which the high-type firm reports truthfully, but the low-type firm mixes truthful (adverse) and false (favorable) disclosures, and for all $L \leq L^{**}$ only a pooling equilibrium will exist.

(b) For all other values of k^R (i.e., for $k^R < x_1$ and $k^R \geq (x_1 + x_2)/2$), a pooling equilibrium will exist.[8]

Given conditions identical to those in Proposition 1 except that the favorable outcome is not large ($x_2 < x_2^{\ddagger}$), part (a) of Corollary 1 shows that, as in Proposition 1, disclosures initially become more credible as the proprietary cost of disclosure increases. That is, Figure 2 shows that as L increases, disclosures become more truthful over Regions 1–3 under Corollary 1 just as they do over Regions A–C under Proposition 1. However, Figure 2 also shows that a new threshold, $\bar{L}$, now divides Region C from Proposition 1 into Regions 3 and 4 under Corollary 1. Significantly, in Region 4 the firm underreports; i.e., after realizing a favorable outcome the firm makes an adverse disclosure with a strictly positive probability. In Region 4 the combination of large proprietary costs and a smaller favorable outcome (x_2) means that product market influences dominate capital market influences, leading the incumbent firm to make a false adverse disclosure to deter the rival's entry. Hence, from Region 3 to Region 4 disclosures become less credible.

Part (b) of Corollary 1 addresses environments with no tension between the informational demands of capital and product markets. In these environments, the rival's cost of entry is either so low that the rival always enters or so high that the rival never enters.

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[8] Furthermore, this pooling equilibrium is the unique pure-strategy disclosure equilibrium for all $k^R \notin [x_1, (x_1 + x_2)/2)$, except when $(x_1 + x_2)/2 \leq k^R < x_2$. When $(x_1 + x_2)/2 \leq k^R < x_2$ and $L \in [L^*, \mathrm{Max}\{\bar{L}, x_1\})$, a separating equilibrium also exists, where $x_2 \geq x_2^{\ddagger} \Leftrightarrow \bar{L} \geq x_1$.

Because the rival firm does not react to the firm's disclosure, the disclosures are not credible, producing a pooling equilibrium.

Next, Corollary 2 provides empirical predictions concerning how changes in the economic environment alter the firm's disclosure behavior.

Corollary 2: When the rival's cost to enter falls in an intermediate range ($k^R \in [x_1, (x_1 + x_2)/2)$):

(a) If the firm's favorable outcome is sufficiently large ($x_2 \geq x_2^*$), then thresholds L^* and L^{**} and ρ_{12} (the probability of overreporting for $L \in (L^{**}, L^*)$) all increase in k and x_2, and decrease in x_1.

(b) If the firm's favorable outcome is not large ($x_2 < x_2^*$), then the threshold $\underline{L}$ increases in k and x_2, and decreases in x_1.

Corollary 2(a) demonstrates how the economic environment determines the extent of mutual discipline created by product and capital markets influences, yielding alternative disclosure equilibria. Increasing k means the firm raises more capital and hence has more to gain by overreporting and reducing the cost of capital. Increasing x_2 increases the spread between the favorable and unfavorable outcomes, making the firm's cost of capital more sensitive to its disclosures. Therefore, increases in k and x_2 enhance the importance of the capital market and increase the two thresholds, L^* and L^{**}, producing less credible disclosures. An increase in L^* means less credible disclosures because proprietary costs that originally produced fully informative disclosures now yield only partially informative disclosures. Similarly, as L^{**} increases, pooling equilibria with uninformative disclosures occur for a wider interval of proprietary costs, and hence become more likely. Conversely, increasing x_1 reduces the capital cost of an adverse disclosure, and therefore the firm overreports less frequently (i.e., voluntary adverse disclosures become more frequent). The intuition is similar for the remaining cases.

III. LITIGATION

Model

Extending the model from Section II to incorporate shareholder litigation is important for several reasons. First, by imposing significant costs on firms (including the cost of frequent out-of-court settlements; Alexander 1991), shareholder litigation affects firms' disclosure decisions (AICPA 1994). Second, only disclosure models with shareholder litigation relate directly to the growing empirical literature on the relations among firms' financial disclosures, shareholder litigation and various antecedent and consequent conditions (Skinner 1994; Francis et al. 1994; Bamber and Cheon 1998; Johnson et al. 2000, 2001; Ali and Kallapur 2001; Baginski et al. 2002). However, most prior disclosure models do not address the determinants of truthful disclosures, and those that do (Newman and Sansing 1993; Gigler 1994) have not examined shareholder litigation. We address this gap by developing theoretical predictions concerning how shareholder litigation affects a firm's disclosure behavior. Third, shareholder litigation is itself an interesting phenomenon because the shareholders' decision to sue is noncontractual, discretionary, and sequentially rational. These features distinguish shareholder litigation from an alternative disciplining mechanism, contractual monitoring, in which a firm *ex ante* commits to a monitoring policy that is suboptimal once the agent chooses his effort (e.g., Baiman and Demski 1980; Kanodia 1985).

As shown in the timeline in Figure 3, we generalize the model from Section II by assuming that following the firm's disclosure and subsequent trading, first-generation or second-generation shareholders can sue the firm for distorting the stock price through false

FIGURE 3
Timeline 2: Voluntary Disclosure with Shareholder Litigation

$t = 0$	$t = 1$	$t = 2$
↑	↑	↑
Original shareholders are endowed with shares in a firm.	The manager privately observes the firm's terminal dividend, x_j. The manager discloses $\hat{x}_i$. The manager must raise k by selling fraction α_i of the firm at price P_i.	The rival firm's entry decision is publicly observed. Shareholders may sue. The shareholders consume the net payoff after the firm pays any litgation damages.

disclosures ($\hat{x} \neq x$). Shareholders sue with probability $\sigma_i = \sigma(\hat{x}_i, \alpha_i, e/ne)$, reflecting the fact that when they sue, shareholders know the firm's disclosure ($\hat{x}_i$), the fraction of the firm sold (α_i), and whether the rival entered (e) or did not (ne). We assume that shareholders incur a cost, $k^S > 0$, to sue and that the litigation-prompted investigation perfectly reveals the manager's private information (x_j).[9] When litigation reveals a false disclosure, the firm must pay the litigants damages of $D_{ji} \equiv D(x_j, \hat{x}_i, \alpha_i)$, depending on the actual outcome (x_j), the disclosed outcome ($\hat{x}_i$), and the fraction of the firm sold (α_i). We use the widely recognized "market model" (Hurd and Wagner 1990; Posner 1992) to determine damages. According to this model, damages equal the difference between the realized market price of the shares sold based on the firm's actual disclosure and what would have been the "true" price of the shares based on a full and honest disclosure. In a rational expectations equilibrium, the market price will incorporate expected future litigation damages. Appendix A explains the joint determination of market prices and damages.[10]

With litigation, a firm with an unfavorable outcome chooses a disclosure by trading off the potentially higher stock price (lower cost of capital) associated with a favorable disclosure against the corresponding proprietary and litigation costs. The manager's disclosure strategy, ρ_{ji}, maximizes the expected payoff to first-generation shareholders, $k + (1 - \alpha_i)[x_j - \mu_i L - \sigma_i D_{ji}]$, based on the sequentially rational strategies of all other players. This objective function parallels that in Section II but now includes expected litigation damages.

Analysis

Shareholder litigation produces four related changes in the analysis of equilibrium disclosures. First, shareholders choose suing strategies based on the firm's disclosure. Second, the firm incorporates expected litigation damages in its disclosure decision. Third, the rival considers how potential litigation damages could affect the firm's disclosure. Fourth, investors incorporate expected litigation damages in pricing the firm's shares. Lemma 1 characterizes disclosure equilibria with litigation.

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[9] To ensure a positive probability of suing in the event of misreporting, we assume that the cost of suing is bounded from above., i.e., that $k^s < ((x_2 + x_1 - 2L - 2k)/(x_2 + x_1 - 2L))\ (k(x_2 - x_1))/((x_1 + x_2) - 2L)$.

[10] Lemma 2 in Appendix B establishes equilibrium prices with litigation.

Lemma 1 (Shareholder Litigation):

1. When the rival's cost to enter falls in an intermediate range ($k^R \in [x_1, (x_1 + x_2)/2)$) and the incumbent's favorable outcome is sufficiently large ($x_2 \geq x_2^*$), then:
   (a) For proprietary cost $L \in [L^*, x_1)$, the firm will always report truthfully;
   (b) For proprietary costs less than or equal to a second threshold, $\underline{L}$, where $\underline{L} < L^*$, only a pooling equilibrium will occur; and
   (c) For all intermediate proprietary costs, $L \in (\underline{L}, L^*)$, only a mixed-strategy equilibrium will occur, in which the firm will report truthfully after realizing x_2 but will mix reports of $\hat{x}_1$ and $\hat{x}_2$ after realizing x_1.
2. When the rival's cost to enter falls in an intermediate range ($k^R \in [x_1, (x_1 + x_2)/2)$) and the incumbent's favorable outcome is not large ($x_2 < x_2^*$), then:
   (a) For all proprietary costs in the interval $L \in [L^*, \bar{L}]$, the firm will always report truthfully;
   (b) For all proprietary costs, $L \in (\bar{L}, x_1)$, a partial separating equilibrium will occur in which the firm will report truthfully after realizing x_1 but will mix reports of $\hat{x}_1$ and $\hat{x}_2$ after realizing x_2;
   (c) For all $L \in (\underline{L}, L^*)$, a partial separating equilibrium will exist in which the firm will report truthfully after realizing x_2 but will mix reports of $\hat{x}_1$ and $\hat{x}_2$ after realizing x_1, and for all $L \leq \underline{L}$ only a pooling equilibrium will exist.
3. For all other values of k^R, a pooling equilibrium will exist.

In parts 1 and 3 of Lemma 1 the firm trades off the lower capital costs from overreporting against both increased proprietary costs and expected shareholder litigation costs. When capital market incentives dominate product market concerns, the incumbent firm overreports in equilibrium. In contrast, in part 2 of Lemma 1, when $L \in (\bar{L}, x_1)$, the firm trades off avoiding proprietary costs against both higher capital costs and expected shareholder litigation costs. In this case, product market concerns dominate capital market influences, and the firm underreports.[11] In this way, Lemma 1 distinguishes circumstances under which the dominant factor in disclosure decisions is the cost of capital (parts 1 and 3) versus product market effects (part 2).

The litigation equilibria in Lemma 1 and the no-litigation equilibria in Proposition 1 and Corollary 1 share similar general structures. Proposition 2 identifies the differences created by incorporating potential shareholder litigation. Let P_i^{SL} and P_i denote prices after the firm discloses $\hat{x}_i$ with and without shareholder litigation (SL), respectively.

Proposition 2:

When the rival's cost to enter falls in an intermediate range ($k^R \in [x_1, (x_1 + x_2)/2)$), shareholder litigation:

1. (a) Reduces the likelihood of uninformative disclosures (a pooling equilibrium) by reducing the threshold proprietary cost below which pooling occurs ($\underline{L} < L^{**}$), and
   (b) Reduces the frequency of overreporting for all intermediate proprietary costs, $L \in (\underline{L}, L^*)$;

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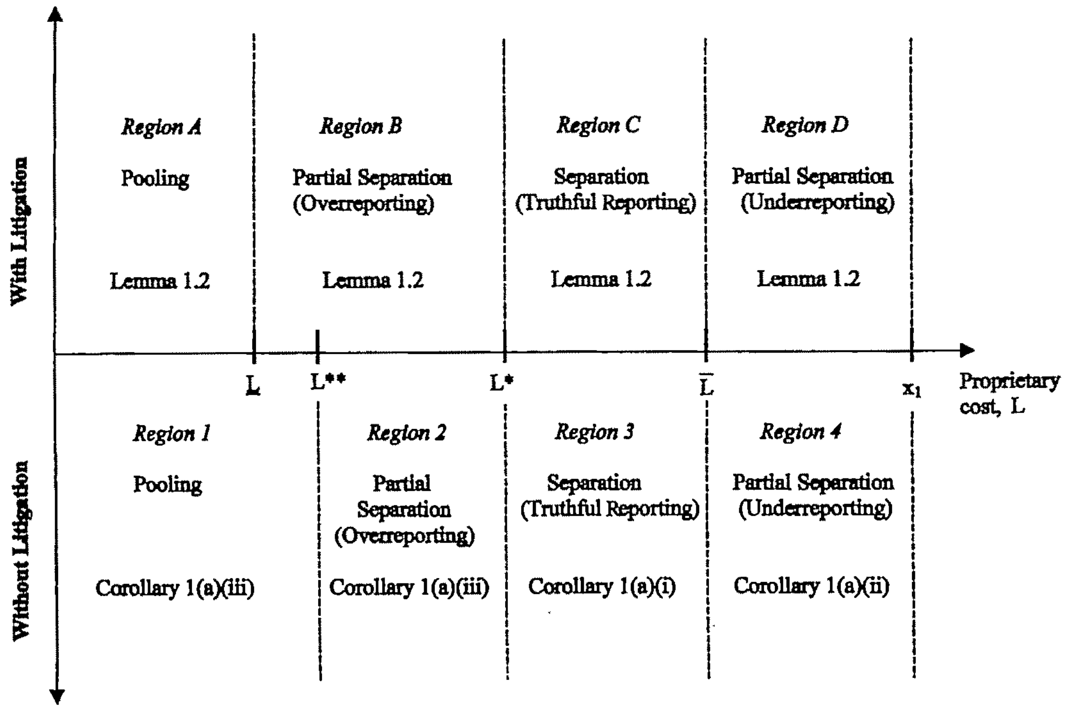
[11] While most shareholder lawsuits involve alleged overreporting, some lawsuits have also alleged that the firm has understated future economic prospects. For example, the widely cited *Basic Incorporated v. Levinson*, 108 S.Tr. 978 (1988) case involved Basic's denial that certain value-enhancing transactions had taken place (Davidson et al. 1993).

2. Increases the price for a favorable disclosure, $P_2^{SL} > P_2$ for all $L \in (\underline{L}, L^*)$, when the incumbent's favorable outcome is large ($x_2 \geq x_2^*$); and
3. (a) Reduces the frequency of underreporting for proprietary costs $L \in (\bar{L}, x_1)$ when the incumbent's favorable outcome is not large ($x_2 < x_2^*$), and
   (b) Reduces prices for unfavorable disclosures ($P_1^{SL} < P_1$) for all $L \in (\bar{L}, x_1)$ and increases prices for favorable disclosures ($P_2^{SL} > P_2$) for all $L \in (\underline{L}, L^*)$ when $x_2 < x_2^*$.

Proposition 2 establishes that litigation can boost credibility by making false disclosures more costly. Parts 1(a), 1(b), and 3(a) of Proposition 2 describe how greater credibility takes the form of either a reduced range of proprietary costs for which disclosures have no credibility (i.e., pooling occurs) or an increased frequency of truthful disclosures in mixed-strategy equilibria. The greater credibility imparted by litigation results in higher prices for favorable disclosures (parts 2 and 3b) and lower prices for unfavorable disclosures (part 3b). Figure 4 illustrates how shareholder litigation increases the informativeness of disclosures by reducing the region over which a pooling equilibrium occurs (Region A with shareholder litigation is less than Region 1 without shareholder litigation.) Consequently, for $L \in (\underline{L}, L^{**})$ the firm sometimes makes voluntary adverse disclosures because of the incremental effect of shareholder litigation.

FIGURE 4

A Comparison of Disclosure Equilibria with and without Shareholder Litigation[a]



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[a] Figure 4 assumes that $x_1 \leq k^R < (x_1 + x_2)/2$ and that $x_2 < x_2^*$. Recollect that $\bar{L} < x_1$ if and only if $x_2 < x_2^*$.

At the same time, the effect of litigation on the firm's disclosure behavior is limited in several ways. First, with $x_2 \geq x_2^*$ and intermediate values of the rival's entry cost ($k^R \in [x_1, (x_1 + x_2)/2)$), one might expect litigation to yield more truthful disclosures by reducing the threshold proprietary cost (L^*) above which a truthful-reporting equilibrium obtains. However, we find that the threshold L^* remains unchanged with litigation because when $L \geq L^*$, sufficient product market incentives to induce truthful disclosure already exist without litigation. Further, if shareholder litigation were to reduce the threshold for a truthful-reporting equilibrium to, say, $L^{*\prime} < L^*$, then for $L \in [L^{*\prime}, L^*)$ the firm would report truthfully. But in any truthful-reporting equilibrium, shareholders would not sue because suing is costly and yields no benefit given that the firm discloses honestly. Therefore, the cost of overreporting for $L \in [L^{*\prime}, L^*)$ would consist only of the proprietary cost. In turn, this would contradict the derivation of the original threshold L^* in a no-litigation regime. Likewise, when $x_2 < x_2^*$, litigation does not alter the threshold $\bar{L}$ and the associated range of proprietary losses that sustains a truthful-reporting equilibrium, $[L^*, \bar{L}]$.

Second, if there were no rival firm or if proprietary cost were zero, then shareholder litigation alone could never produce a truthful-reporting equilibrium. If the firm always disclosed honestly in equilibrium, then rational shareholders would never sue in equilibrium. In turn, facing neither proprietary costs nor litigation damages, the incumbent would deviate and always disclose $\hat{x}_2$ to reduce its cost of capital.[12] In contrast, Proposition 1(a) shows that, even without shareholder litigation, the threat of the rival's entry alone is sometimes sufficient to produce truthful reporting in equilibrium. In this way, the product market alone (without shareholder litigation) can support a truthful-reporting equilibrium, but shareholder litigation alone (with no product market) cannot.

The greater disclosure discipline imposed by product market forces *vis-à-vis* shareholder litigation stems from fundamental differences between the two processes. Product market competition can impose proprietary costs on the firm even when the rival believes the incumbent has truthfully disclosed its outcome. In contrast, shareholder litigation can be effective only when second-generation shareholders (correctly) believe that the firm's report differs from the actual outcome with some positive probability. When second-generation shareholders believe that the firm has disclosed truthfully, they will never sue in equilibrium and, as a consequence, shareholder litigation will be ineffective as a disclosure-disciplining mechanism because it imposes no costs on the firm.

Finally, when the rival's entry cost is sufficiently high ($x_2 \leq k^R$) or sufficiently low ($k^R < x_1$), the rival's entry decision is insensitive to the incumbent's disclosure and shareholder litigation alone is again insufficient to support a truthful-reporting equilibrium for similar reasons.

While the product market is sometimes more effective in complementing capital markets to discipline a firm's disclosure behavior than shareholder litigation, product market effects can also induce the firm to misreport in some circumstances. For instance, under the conditions in Corollary 1(a)(ii), where the high outcome is not sufficiently large and k^R falls in the intermediate range, the firm will sometimes underreport to discourage the rival from entering. If there were no rival firm, then the incumbent would report truthfully after realizing x_2 under those circumstances. However, the absence of the rival firm would also prompt the low-type incumbent to overreport, and thus a pooling equilibrium would obtain, rendering all the disclosures uninformative. Although potential proprietary costs lead the firm to report truthfully after realizing the low outcome under the conditions specified in Corollary 1(a)(ii), the same product market considerations can also lead the firm to underreport after realizing the high outcome (Corollary 1(a)(ii)). In contrast, the prospect of shareholder litigation never prompts the firm to misreport.

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[12] Without a rival firm, a mixed-strategy equilibrium could still exist in which the firm would randomize over reports of $\hat{x}_1$ and $\hat{x}_2$ after realizing x_1 and the shareholders would sue the firm with a strictly positive probability after observing $\hat{x}_2$. Such a mixed-strategy equilibrium would produce more frequent misreporting than the truthful-reporting equilibrium produced by product market competition alone under conditions described in Proposition 1(a).

Our model assumes that the first-generation shareholders retain a residual interest in the firm. However, in practice some first-generation shareholders may sell all of their shares, while other first-generation shareholders retain an interest in the firm. The existence of first-generation shareholders who sell all of their shares would further reduce the effectiveness of shareholder litigation as a disclosure-disciplining mechanism if these first-generation shareholders can influence the design of the manager's compensation contract. Such shareholders would design the manager's contract to produce a disclosure that would maximize the firm's price at time t = 1 because they would enjoy the benefits of a potentially higher price while bearing no subsequent litigation damages. This would further reduce the effectiveness of shareholder litigation relative to product market competition as a disclosure-disciplining mechanism. On the other hand, if the manager were held personally liable for misreporting or if he were to suffer reputation loss following a shareholder lawsuit, then one would expect shareholder litigation to become a more effective disclosure-disciplining mechanism. The effectiveness of shareholder litigation as a disclosure-disciplining mechanism therefore depends on which shareholders retain the rights to design the manager's contract, and whether the manager is personally liable for misreporting or bears costs such as the loss of reputation from misreporting.

IV. CONCLUSION

To decrease its cost of capital, a firm would prefer to overreport its outcome, whereas the proprietary costs associated with product market competition (i.e., a rival's potential entry into the incumbent firm's product market) gives the incumbent firm incentive to underreport. When shareholder litigation is not feasible, disclosures become more informative as the product market's influence becomes stronger relative to the capital market's influence. The resulting disclosure equilibrium moves from being completely uninformative (Proposition 1(b), Corollary 1(a)(iii), and Regions A and 1 in Figure 2) when the product market's effect is weakest to being fully informative (Proposition 1(a), Corollary 1(a)(i), and Regions C and 3 in Figure 2) when the product market's effect is strongest.[13] For intermediate levels of product market influence, disclosures are partially informative.

Further, we establish the counterintuitive result that under certain circumstances, a greater proprietary cost increases both the credibility of voluntary disclosures and the welfare of first-generation shareholders. When a greater proprietary cost increases the credibility of the incumbent firm's disclosure, a more credible adverse disclosure can reduce the probability that a rival firm will enter the firm's market, so that the expected value of the proprietary cost declines. This benefit of a smaller expected proprietary cost can exceed the increased capital market costs, and thus benefit first-generation shareholders.

Introducing shareholder litigation generally reduces the likelihood of false disclosures, and thereby leads to higher prices for favorable disclosures and lower prices for adverse disclosures. However, product market competition and shareholder litigation in some cases interact to render shareholder litigation less effective in promoting truthful disclosure. In particular, shareholder litigation has no incremental effect on voluntary disclosures when: (1) the rival's cost to enter is either sufficiently small or sufficiently large, or (2) the proprietary cost from the rival's entry is sufficiently small or sufficiently large. These results suggest circumstances under which shareholder litigation has a limited effect in disciplining a firm's disclosure behavior. However, we demonstrate how the product market can sometimes prompt the firm to underreport, an effect that shareholder litigation never produces.

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[13] Corollary 1(a)(iii) identifies an exception to this general rule. As shown in Figure 2, when the favorable outcome is not large (i.e., if $x_2 < x_2^*$) and the proprietary cost (L) increases from Region 3 to Region 4, the firm's disclosure shifts from truthful reporting in Region 3 to underreporting with a strictly positive probability in Region 4.

Our analysis is subject to several limitations associated with specific features of our model. First, relaxing our assumption that litigation perfectly reveals the firm's actual outcome would produce an environment with imperfect litigation. Determining litigation damages will be more complex, and it is not clear how the interaction between shareholder litigation and product market competition will be affected. Second, a more general analysis would allow our fixed proprietary cost, L, to depend on the firm's actual outcome. We would also expect this change to affect the interaction of shareholder litigation and product market competition as disclosure-disciplining mechanisms, although the precise effect is likely to depend on the assumed form of the proprietary cost function.

The additional credibility provided by shareholder litigation is robust to expanding the outcome space to include any finite number of outcomes. In this more general environment, there will exist parameter values L and k^R such that the market price is strictly increasing in the firm's disclosure and the rival firm will enter only after observing a report greater than a given threshold level. Unfortunately, the resulting analytical complexity in an environment with three strategic players without commitment, endogenous determination of litigation damages and prices, etc., prevents us from fully characterizing disclosure equilibria for a range of parameter values.

A multiperiod extension would allow managers and firms to build a reputation for prompt and complete disclosures, potentially increasing the likelihood of adverse disclosures. Likewise, examining the influence of vague disclosures, ambiguous legal provisions (Trueman 1997), and uncertain costs of suing are other potential avenues for future research. Finally, the analysis could be generalized to examine how contracting to mitigate managerial moral hazard would affect the firm's disclosure behavior.

APPENDIX A

Determination of Prices and Damages with Multiple Markets and Shareholder Litigation

Second-generation shareholders (investors) in a competitive market pay k in return for a fraction α_i of the firm at time t = 1 following a disclosure of $\hat{x}_i$. These investors pay the expected value of their future net cash flow, yielding:

$$k = \alpha_i \left\{E_1[\tilde{x} - \mu_i L - D(\tilde{x},\hat{x}_i,\alpha_i)|\hat{x}_i,\alpha_i]\right\} + E_1[D(\tilde{x},\hat{x}_i,\alpha_i) - k^s|\hat{x}_i,\alpha_i], \tag{1}$$

where $E_t[\cdot]$ refers to expectations taken at time t. On the right-hand side (RHS), the first expectation represents investors' share of the firm's expected net residual value. The second expectation indicates that second-generation shareholders receive all expected litigation damages less the cost of suing in the event of overreporting in equilibrium.[14] Dividing

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[14] If the firm underreports in equilibrium, then the sellers (from the first-generation shareholders) potentially receive the damages and the investors (or buyers of these shares) receive no damages in equilibrium, because it is only the sellers and not the investors who suffer from underreporting in equilibrium. In this case, the expressions in the pricing equation in (1) can be modified to be consistent with equilibrium underreporting.

Equation (1) by α_i, using the relation $k = \alpha_i P_i$, and collecting common terms yields the following pricing equation for the firm:

$$P_i = E_1[\tilde{x} - \mu_i L|\hat{x}_i,\alpha_i] + \frac{(1 - \alpha_i)E_1[D(\tilde{x},\hat{x}_i,\alpha_i)|\hat{x}_i,\alpha_i] - E_1[k^*|\hat{x}_i,\alpha_i]}{\alpha_i}. \tag{2}$$

Litigation damages, $D(x_j,\hat{x}_i,\alpha_i)$, are determined by the "market model" as the difference between the realized price, P_i, and the "true" or "intrinsic" price of the firm, $P_T(x_j)$.[15] Thus, litigation damages are:

$$D(x_j,\hat{x}_i,\alpha_i) = \begin{cases} 0 \text{ if } \hat{x}_i = x_j; \\ \alpha_i\,|P_i - P_T(x_j)| \text{ if } \hat{x}_i \neq x_j, \end{cases} \tag{3}$$

where the true price $P_T(x_j) = \begin{cases} x_j \text{ if } x_j \leq k^R \\ x_j - L \text{ if } x_j > k^R. \end{cases}$

APPENDIX B

Proof for Proposition 1

(a) Separating Equilibrium: Given the assumptions that (1) the rival's payoffs are identical to that of the firm, and (2) the condition in Proposition 1 that $k^R \in [x_1, (x_1 + x_2)/2)$, the rival will enter only following a report of $\hat{x}_2$ in a separating equilibrium. A separating equilibrium requires first, that the firm (i.e., the first-generation shareholders) prefers to disclose an unfavorable outcome (x_1) honestly to overreporting, which means that:

$$k + (1 - \alpha_1) \times x_1 \geq k + (1 - \alpha_2) \times (x_1 - L), \tag{4}$$

where k is the capital the first generation of shareholders receives from investors in return for the fraction α_i of the firm, i = 1,2, based on the disclosure $\hat{x}_i$ by the firm. The terms $(1 - \alpha_1)x_1$ on the left-hand side (LHS) of Equation (4) and $(1 - \alpha_2) \times (x_1 - L)$ on the RHS of (4) represent first-generation shareholders' net expected residual interests in the firm from disclosures of $\hat{x}_1$ and $\hat{x}_2$, respectively. The term $(x_1 - L)$ reflects the rival's sequentially rational strategy of entering the incumbent's product market after observing a report of $\hat{x}_2$ in a separating equilibrium. Then, with equilibrium prices $P_1 = x_1$ and $P_2 = x_2 - L$ reflecting investors' consistent beliefs in a separating equilibrium, $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2 = k/(x_2 - L)$. Substituting these values for α_1 and α_2 in (4) yields:

$$k + \left(1 - \frac{k}{x_1}\right) \times x_1 \geq k + \left(1 - \frac{k}{x_2 - L}\right) \times (x_1 - L). \tag{5}$$

Subtracting k from both sides of (5) and some simplifying yields:

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[15] The true price is based on the legal premise under Rule 10b-5 that all disclosures must be complete and accurate. Hence, the true price reflects the result that in a first-best setting the rival firm would act only when the firm's terminal dividend exceeded its cost to enter, k^R. Hurd and Wagner (1990, 563) define *true value* as the "value that would be observed but for the fraud."

$$(x_1 - k) \geq \left(\frac{x_2 - L - k}{x_2 - L}\right) \times (x_1 - L). \tag{6}$$

Define L* as the proprietary cost that leaves the firm indifferent between disclosing honestly ($\hat{x}_1$) vs. disclosing $\hat{x}_2$, given x_1:

$$(x_1 - k) = \left(\frac{x_2 - L^* - k}{x_2 - L^*}\right) \times (x_1 - L^*). \tag{7}$$

Claim 1 below demonstrates that there is always a solution to (7) such that $0 < L^* < k$. (Claims 1–6 appear at the end of the proof for Proposition 1.) The LHS of (6) is independent of L, and Claim 2 demonstrates that the RHS of (6) is decreasing in L. Therefore, for any $L \geq L^*$, (6) is satisfied.

A separating equilibrium also requires that following a favorable outcome (x_2), the firm prefers to disclose honestly rather than disclosing $\hat{x}_1$. Given the assumed parameter values, the rival would, in a separating equilibrium, enter following a disclosure of $\hat{x}_2$ but not following a disclosure of $\hat{x}_1$. Thus, truthful disclosure of $\hat{x}_2$ requires:

$$k + (1 - \alpha_2) \times (x_2 - L) \geq k + (1 - \alpha_1) \times x_2, \tag{8}$$

where, as before, $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2 = k/(x_2 - L)$. Substituting these values for α_1 and α_2 in (8), subtracting k from both sides, and rearranging yields:

$$(x_2 - L - k) \geq \left(\frac{x_1 - k}{x_1}\right) \times x_2. \tag{9}$$

Define $\bar{L}$ as the proprietary cost that leaves the firm indifferent between disclosing $\hat{x}_2$ honestly versus disclosing $\hat{x}_1$, given x_2:

$$(x_2 - \bar{L} - k) = \left(\frac{x_1 - k}{x_1}\right) \times x_2 \Rightarrow \bar{L} = k\left(\frac{x_2 - x_1}{x_1}\right). \tag{10}$$

The RHS of (9) is independent of L, whereas the LHS decreases in L, implying that (9) is satisfied for:

$$L \leq \bar{L} = k\left(\frac{x_2 - x_1}{x_1}\right). \tag{11}$$

The preceding analysis indicates that (6) and (9) constitute necessary and sufficient conditions for a separating equilibrium. Claim 3 establishes that $\bar{L} = k([x_2 - x_1]/x_1) \geq x_1$ when $x_2 \geq x_2^*$, as assumed in Proposition 1. In combination with the assumption that $k < x_1$, these relations together with the Claim 1 result that $L^* \in (0,k)$ imply that $0 < L^* < k < x_1 \leq \bar{L}$. Then, because (6) is satisfied for any $L \geq L^*$ and condition (9) is satisfied for any $L \leq \bar{L}$, it follows that both (6) and (9) will be met for any $L \in [L^*, x_1)$, thereby ensuring that a separating equilibrium will exist.

(b) Pooling Equilibrium: We assume that in a pooling equilibrium, firms with both favorable and unfavorable outcomes disclose $\hat{x}_2$, and that investors' off-equilibrium beliefs are such that an off-equilibrium disclosure of $\hat{x}_1$ implies that an unfavorable outcome (x_1) occurred. Therefore, given the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$, the rival will enter following the equilibrium disclosure of $\hat{x}_2$, but not following the off-equilibrium disclosure of $\hat{x}_1$. A pooling equilibrium then requires that following the realization of x_1, the firm prefers to report $\hat{x}_2$:

$$k + (1 - \alpha_2) \times (x_1 - L) \geq k + (1 - \alpha_1) \times (x_1), \tag{12}$$

where $\alpha_2 = k/P_2 = k/(0.5(x_1 + x_2) - L)$ and $\alpha_1 = k/P_1 = k/x_1$ represent investors' and the rival's consistent equilibrium and off-equilibrium beliefs, respectively, in a pooling equilibrium. The price $P_1 = x_1$ reflects investors' off-equilibrium belief that the low report comes from a low-type firm. Substituting for α_1 and α_2 in (12) yields:

$$\left(\frac{0.5(x_1 + x_2) - L - k}{0.5(x_1 + x_2) - L}\right) \times (x_1 - L) \geq (x_1 - k). \tag{13}$$

Define L** as the proprietary cost that leaves the firm with an unfavorable outcome (x_1) just indifferent between disclosing $\hat{x}_2$ versus $\hat{x}_1$:

$$\left(\frac{0.5(x_1 + x_2) - L^{**} - k}{0.5(x_1 + x_2) - L^{**}}\right) \times (x_1 - L^{**}) = (x_1 - k). \tag{14}$$

Claims 4 and 5 establish that there exists a solution to (14) such that $0 < L^{**} < L^*$. Claim 6 establishes that the LHS of (13) is decreasing in L, whereas the RHS of (13) is independent of L, which implies that (13) will be satisfied for all $L < L^{**}$. Hence, given $L < L^{**}$, a firm with an unfavorable outcome will disclose $\hat{x}_2$.

Similarly, after realizing x_2, the firm will prefer the pooling disclosure $\hat{x}_2$ if:

$$k + (1 - \alpha_2) \times (x_2 - L) \geq k + (1 - \alpha_1) \times x_2, \tag{15}$$

where, as before, $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2 = k/(0.5(x_1 + x_2) - L)$. Substituting for α_1 and α_2 in (15), multiplying by (x_1/x_2), and rearranging yields:

$$\left(\frac{0.5(x_1 + x_2) - L - k}{0.5(x_1 + x_2) - L}\right) \times \left(x_1 - L\left(\frac{x_1}{x_2}\right)\right) \geq (x_1 - k). \tag{16}$$

The RHS's of (13) and (16) are identical and the LHS of (16) > LHS of (13) because $(x_1/x_2) < 1$ and $L > 0$. Thus, (13) implies (16), and hence a pooling equilibrium will exist for all $L < L^{**}$. ■

(c) Mixed-Strategy Equilibrium: Parts (a) and (b) have established that no pure-strategy separating equilibrium can exist for $L < L^*$ and that no pure-strategy pooling equilibrium can exist for $L > L^{**}$. Therefore, for $L \in (L^{**}, L^*)$, we next establish that at least one mixed-strategy equilibrium will exist. Conjecture a mixed-strategy equilibrium in which the firm discloses honestly after realizing x_2, but follows a mixed disclosure strategy after realizing x_1. After realizing x_j, the firm's expected payoff from reporting $\hat{x}_1$ and $\hat{x}_2$ with probabilities ρ_{j1} and ρ_{j2}, respectively, will be $\pi_j \equiv \rho_{j1}\pi_{j1} + \rho_{j2}\pi_{j2}$, where π_{ji} represents the firm's expected payoff (excluding k) from reporting $\hat{x}_i$ after realizing outcome x_j. Define r_{ji}

$= \Pr(x_j|\hat{x}_i)$ to be the posterior probability that x_j occurred given that the firm disclosed $\hat{x}_i$. Consistent beliefs of the market and the rival firm imply that:

$$\pi_{j1} = \left(1 - \frac{k}{P_1}\right)(x_j - \mu_1 L); \text{ and} \tag{17}$$

$$\pi_{j2} = \left(1 - \frac{k}{P_2}\right)(x_j - \mu_2 L), \tag{18}$$

where $\alpha_i = k/P_i$ and the market price $P_i = r_{1i}x_1 + r_{2i}x_2 - \mu_i L$ for $i = 1,2$, reflects the capital and product markets' consistent beliefs in the conjectured mixed-strategy equilibrium. After realizing x_1, the firm will mix reports $\hat{x}_1$ and $\hat{x}_2$ only if:

$$\pi_{11} = \pi_{12}. \tag{19}$$

From the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$, we have $\mu_1 = 0$ (no entry following $\hat{x}_1$) and $\mu_2 = 1$ (entry following $\hat{x}_2$) for all values of ρ_{11}, given that $\rho_{22} = 1$. Applying these values of μ in (17) and (18) yields:

$$\pi_{11} = \left(1 - \frac{k}{P_1}\right)(x_1 - \mu_1 L) = \left(1 - \frac{k}{x_1}\right)x_1 = x_1 - k; \text{ and} \tag{20}$$

$$\pi_{12} = \left(\frac{r_{12}x_1 + (1 - r_{12})x_2 - L - k}{r_{12}x_1 + (1 - r_{12})x_2 - L}\right)(x_1 - L). \tag{21}$$

Because $\pi_{11} = \pi_{12}$ in equilibrium, equating the RHS's of (20) and (21) yields:

$$x_1 - k = \left(\frac{r_{12}x_1 + (1 - r_{12})x_2 - L - k}{r_{12}x_1 + (1 - r_{12})x_2 - L}\right)(x_1 - L). \tag{22}$$

Bayesian updating yields $r_{12} = 0.5\rho_{12}/(0.5\rho_{12} + 0.5\rho_{22}) = \rho_{12}/(\rho_{12} + 1)$ because in the conjectured equilibrium $\rho_{22} = 1$, and (22) becomes:

$$x_1 - k = \left(\frac{x_2 - \dfrac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L - k}{x_2 - \dfrac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L}\right)(x_2 - L). \tag{23}$$

It can be shown that there always exists a value of $\rho_{12} \in (0, 1)$ that solves (23). Such a value of ρ_{12} defines the mixed disclosure strategy for the firm after realizing x_1.[16]

After realizing x_2, the firm will report $\hat{x}_2$ with probability 1 ($\rho_{22} = 1$) only if:

$$k + (1 - \alpha_2) \times (x_2 - L) \geq k + (1 - \alpha_1) \times x_2. \tag{24}$$

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[16] This mixed-strategy equilibrium exists for all values of k^R such that $x_1 \leq k^R < r_{12}x_1 + r_{22}x_2$. Because $\rho_{12} < 1$, $(x_1 + x_2)/2 < r_{12}x_1 + r_{22}x_2$, and hence this mixed-strategy equilibrium exists for all $x_1 \leq k^R < (x_1 + x_2)/2$, as in Proposition 1.

As before, based on the markets' consistent beliefs and the rival's entry strategy in the conjectured mixed-strategy equilibrium, we have $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2 = k/(r_{12}x_1 + (1 - r_{12})x_2 - L)$. Substitute these values in (24) and multiply by (x_1/x_2) to yield:

$$\left(\frac{r_{12}x_1 + (1 - r_{12})x_2 - L - k}{r_{12}x_1 + (1 - r_{12})\,x_2 - L}\right)\left(x_1 - L\frac{x_1}{x_2}\right) \geq (x_1 - k). \tag{25}$$

Given that $(x_1 - L\,(x_1/x_2)) > (x_1 - L)$, LHS (25) > RHS (23) because $x_1 < x_2$ and $L > 0$. This implies that (25) is met, and the firm will always report $\hat{x}_2$ after realizing x_2, as conjectured. This establishes that at least one mixed-strategy equilibrium will exist for $L \in (L^{**}, L^{*})$. Finally, we provide the following claims without proofs.[17]

Claim 1
A solution to (7) exists such that $0 < L^* < k$.

Claim 2

$$\frac{d\left(\frac{x_2 - L - k}{x_2 - L}\right) \times (x_1 - L)}{dL} \leq 0.$$

Claim 3

$$x_2 \geq x_2^* \equiv \frac{x_1^2}{k} + x_1 \Leftrightarrow \bar{L} \geq x_1.$$

Claim 4
A solution to (14) exists such that $0 < L^{**}$.

Claim 5
$L^{**} < L^*$.

Claim 6

$$\frac{d\left(\frac{0.5(x_1 + x_2) - L - k}{0.5(x_1 + x_2) - L}\right) \times (x_1 - L)}{dL} < 0.$$

This concludes the proof for Proposition 1. ■

Proof for Remark 2

Compare the expected welfare of first-generation shareholders given a lower proprietary cost of L (which yields a pooling equilibrium) to their welfare given a higher proprietary cost of L′ (which yields a separating equilibrium). With a proprietary cost of L, expected welfare is:

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[17] The proofs are straightforward and are available from the authors.

$$(1 - \alpha_P)(0.5(x_1 + x_2) - L) = (0.5(x_1 + x_2) - L - k). \tag{26}$$

because in a pooling equilibrium, $\alpha_P = k/(0.5(x_1 + x_2) - L)$. With a higher proprietary cost of L', the expected welfare in the resulting separating equilibrium is:

$$0.5 \times [(1 - \alpha_1)x_1 + (1 - \alpha_2)(x_2 - L')] = 0.5 \times [(x_1 - k) + (x_2 - L' - k)]. \tag{27}$$

(27) is greater than (26) if and only if:

$$(0.5(x_1 + x_2) - L - k) < 0.5 \times [(x_1 - k) + (x_2 - L' - k)] \Leftrightarrow L > 0.5L'. \tag{28}$$

So, if and only if $L > 0.5L'$, increasing L to L' benefits the first-generation shareholders. ∎

Proof for Corollary 1

Consider the mixed-strategy equilibrium conjectured in Corollary 1(a)(ii). The firm's expected payoff π_{ji} from observing x_j and disclosing $\hat{x}_i$ will be $\pi_{ji} = (1 - \alpha_i)(x_j - \mu_i L) = (1 - (k/P_i))(x_j - \mu_i L)$ for $i, j = 1,2$, where the market price $P_i = r_{1i}x_1 + r_{2i}x_2 - \mu_i L$ for $i = 1,2$, reflects the markets' consistent beliefs in the conjectured mixed-strategy equilibrium. After realizing x_2, the firm will follow a mixed-disclosure strategy only if:

$$\pi_{21} = \pi_{22}. \tag{29}$$

Given the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$, the rival's equilibrium strategy will be to enter with probability $\mu_1 \geq 0$ and $\mu_2 = 1$ for all values of ρ_{21}, given that $\rho_{11} = 1$. Applying these strategies for the rival in the conjectured equilibrium yields:

$$\begin{aligned}\pi_{21} &= (1 - \alpha_1)(x_2 - \mu_1 L) = \left(1 - \frac{k}{P_1}\right)(x_2 - \mu_2 L)\\ &= \left(1 - \frac{k}{r_{11}x_1 + r_{21}x_2 - \mu_1 L}\right)(x_2 - \mu_2 L); \text{ and}\end{aligned} \tag{30}$$

$$\begin{aligned}\pi_{22} &= (1 - \alpha_2)(x_2 - \mu_2 L) = \left(1 - \frac{k}{P_2}\right)(x_2 - L)\\ &= \left(1 - \frac{k}{x_2 - L}\right)(x_2 - L) = x_2 - L - k.\end{aligned} \tag{31}$$

Given that $\pi_{21} = \pi_{22}$ in equilibrium, equating the RHS's of (31) and (30) yields:

$$x_2 - L - k = \left(1 - \frac{k}{r_{11}x_1 + r_{21}x_2 - \mu_1 L}\right)(x_2 - \mu_1 L). \tag{32}$$

Using Bayesian updating, $r_{21} = 0.5\rho_{21}/(0.5\rho_{11} + 0.5\rho_{21}) = \rho_{21}/(\rho_{21} + 1)$ (because in the conjectured equilibrium $\rho_{11} = 1$); and using the result that $\mu_1 < 1$ in (32) yields:[18]

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[18] To demonstrate that $\mu_1 < 1$ in the conjectured equilibrium, we establish that assuming $\mu_1 = 1$ produces a contradiction. That is, substituting $\mu_1 = 1$ in (32) and subtracting $(x_2 - L)$ from both sides yields $-k = -((x_2 - L)/[x_1 + r_{21}(x_2 - x_1) - L])k$. This equation cannot hold because the fraction in parentheses is greater than 1 as our conjectured equilibrium reporting strategies imply that $r_{21} < 1$. The contradiction implies that $\mu_1 < 1$.

$$x_2 - L - k = \left(1 - \frac{k}{x_1 + \frac{\rho_{21}}{\rho_{21}+1}(x_2 - x_1) - \mu_1 L}\right)(x_2 - \mu_1 L). \tag{33}$$

Allowing for the rival also to adopt mixed strategies (when no pure strategies exist in equilibrium), the rival's equilibrium indifference between entering and not entering implies that $r_{21}x_2 + r_{11}x_1 = k^R$, which yields:

$$\frac{\rho_{21}}{\rho_{21}+1} = \frac{k^R - x_1}{x_2 - x_1}, \tag{34}$$

where ρ_{21} and μ_1 are determined as the solutions to (34) and (33), respectively. We next confirm the conjecture that $\rho_{11} = 1$. This requires that:

$$k + (1 - \alpha_1) \times (x_1 - \mu_1 L) > k + (1 - \alpha_2) \times (x_1 - L). \tag{35}$$

Subtracting k and dividing both sides of (35) by $(1 - \alpha_1)(x_1 - L)$ gives:

$$\frac{(x_1 - \mu_1 L)}{(x_1 - L)} > \frac{(1 - \alpha_2)}{(1 - \alpha_1)}. \tag{36}$$

From (29) we know that:

$$\frac{(1 - \alpha_2)}{(1 - \alpha_1)} = \frac{(x_2 - \mu_1 L)}{(x_2 - L)}. \tag{37}$$

Using the result $\mu_1 < 1$, we get $\frac{d\frac{(x_2 - \mu_1 L)}{(x_2 - L)}}{dx_2} < 0$, and therefore, $\frac{(x_1 - \mu_1 L)}{(x_1 - L)} > \frac{(x_2 - \mu_1 L)}{(x_2 - L)} = \frac{(1 - \alpha_2)}{(1 - \alpha_1)}$. Thus, (37) implies that (36) always holds, implying that the firm will always report $\hat{x}_1$ after realizing x_1, as conjectured. Equilibria in other cases can be characterized on the lines of the proof for Proposition 1. ■

Proof for Corollary 2

Part (a): First, we establish the results relating to the sensitivity of L*. Based on (7), define the function:

$$\phi(L^*,k,x_1,x_2) \equiv (x_1 - k) - \left(\frac{x_2 - L^* - k}{x_2 - L^*}\right)(x_1 - L^*).$$

Then by the implicit function theorem, $\frac{dL^*}{dk} = -\frac{\frac{d\phi(L^*,k,\cdot)}{dk}}{\frac{d\phi(L^*,k,\cdot)}{dL^*}}$. Differentiating $\phi(L^*,k,\cdot)$ with respect to k and L* yields, respectively:

$$\frac{d(x_1 - k) - \left(\frac{x_2 - L^* - k}{x_2 - L^*}\right)(x_1 - L^*)}{dk} = -1 + \frac{x_1 - L^*}{x_2 - L^*} = -\frac{(x_2 - x_1)}{(x_2 - L^*)}. \tag{38}$$

$$\frac{d\phi(L^*,\cdot)}{dL^*} = -\left(\frac{(-(x_1 - L^*) - (x_2 - L^* - k))(x_2 - L^*) + (x_2 - L^* - k)(x_1 - L^*)}{(x_2 - L^*)^2}\right)$$
$$= \frac{(x_2 - L^* - k)(x_2 - L^*) + k(x_1 - L^*)}{(x_2 - L^*)^2}. \tag{39}$$

Using (38) and (39) yields $\frac{dL^*}{dk} = -\frac{-\frac{(x_2 - x_1)}{(x_2 - L^*)}}{\frac{(x_2 - L^* - k)(x_2 - L^*) + k(x_1 - L^*)}{(x_2 - L^*)^2}} > 0.$

Along similar lines, the implicit function theorem yields $\frac{dL^*}{dx_1} = -\frac{\frac{d\phi(L^*,x_1,\cdot)}{dx_1}}{\frac{d\phi(L^*,x_1,\cdot)}{dL^*}} < 0$ and $\frac{dL^*}{dx_2} = -\frac{\frac{d\phi(L^*,x_2,\cdot)}{dx_2}}{\frac{d\phi(L^*,x_2,\cdot)}{dL^*}} > 0$. Next using (14), define the function:

$$\phi(L^{**},k,x_1,x_2) \equiv \left(\frac{0.5(x_1 + x_2) - L^{**} - k}{0.5(x_1 + x_2) - L^{**}}\right) \times (x_1 - L^{**}) - (x_1 - k). \tag{40}$$

Using the implicit function theorem in a similar manner yields $\frac{dL^{**}}{dk} = -\frac{\frac{d\phi(L^{**},k,\cdot)}{dk}}{\frac{d\phi(L^{**},k,\cdot)}{dL^{**}}} > 0$; $\frac{dL^{**}}{dx_1} = -\frac{\frac{d\phi(L^{**},x_1,\cdot)}{dx_1}}{\frac{d\phi(L^{**},x_1,\cdot)}{dL^{**}}} < 0$; and $\frac{dL^{**}}{dx_2} = \frac{\frac{d\phi(LL^*,x_2,\cdot)}{dx_2}}{\frac{d\phi(L^{**},x_2,\cdot)}{dL^{**}}} > 0.$

Similarly, using (23), define the function:

$$\phi(\rho_{12},k,x_1,x_2) \equiv \left(\frac{x_2 - \frac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L - k}{x_2 - \frac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L}\right)(x_1 - L) - (x_1 - k). \tag{41}$$

Using the implicit function theorem yields $\frac{d\rho_{12}}{dk} = -\frac{\frac{d\phi(\rho_{12},k,\cdot)}{dk}}{\frac{d\phi(\rho_{12},k,\cdot)}{d\rho_{12}}} > 0;$

$$\frac{d\rho_{12}}{dx_2} = -\frac{\dfrac{d\phi(\rho_{12},x_2,\cdot)}{dx_2}}{\dfrac{d\phi(\rho_{12},x_2,\cdot)}{d\rho_{12}}} > 0; \text{ and } \frac{d\rho_{12}}{dx_1} = -\frac{\dfrac{d\phi(\rho_{12},x_1,\cdot)}{dx_1}}{\dfrac{d\phi(\rho_{12},x_1,\cdot)}{d\rho_{12}}} < 0.$$

Part (b): Using (10), define the function of $\phi(\bar{L},k,x_1,x_2) \equiv \bar{L} - k\left(\frac{x_2 - x_1}{x_1}\right)$ and apply the implicit function theorem to obtain $\frac{d\bar{L}}{dk} = -\frac{\frac{d\phi(l,k,\cdot)}{dK}}{\frac{d\phi(\bar{L},k,\cdot)}{dL}} > 0$; $\frac{d\bar{L}}{dx_1} = -\frac{\frac{d\phi(\bar{L},x_1,\cdot)}{dx_1}}{\frac{d\phi(\bar{L},x_1,\cdot)}{dL}} < 0$;

and $\frac{d\bar{L}}{dx_2} = -\frac{\frac{d\phi(L,x_2,\cdot)}{dx_2}}{\frac{d\phi(\bar{L},x_2,\cdot)}{dL}} > 0.$ ■

Proof for Lemma 1 and Proposition 2 (sketch)

Now assume that shareholders can sue the firm for misreporting. First, we provide Lemma 2, which establishes pricing equations for the equilibria examined in subsequent proofs.

Lemma 2: Assume that shareholders can sue the firm for misreporting. Then:[19]

(a) The firm's market price in a separating equilibrium or in a mixed-strategy equilibrium where the firm overreports sometimes and where σ_i assumes an interior value is:

$$P_i = \sum_{j=1}^{2} r_{ji}x_j - \mu_i L \text{ for } j,i = 1,2. \tag{42}$$

(b) The firm's market price in a pooling equilibrium or in a mixed-strategy equilibrium with overreporting and where $\sigma_i = 1$ is:

$$P_2 = \left(\sum_{j=1}^{2} r_{j2}x_j - \mu_i L\right) + \sigma_2\left(\left(1 - \frac{k}{P_2}\right) r_{12}(P_2 - (x_1 - \mu_1 L)) - (k^s P_2/k)\right). \tag{43}$$

Proof for Lemma 2 (a) In any given equilibrium, using the result that $k = \alpha_i P_i$ in the pricing Equation (1) yields:

$$\alpha_i P_i = k = \alpha_i E_1[\tilde{x} - \mu_i L|\hat{x}_i,\alpha_i] + (1 - \alpha_i)E_1[D_{ji}|\hat{x}_i,\alpha_i] - E_1[k^s|\hat{x}_i,\alpha_i]. \tag{44}$$

For a given report, $\hat{x}_i$, the above equation implies:

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[19] The following analysis is based on the firm's overreporting in equilibrium, in which case the aggrieved party is the second-generation shareholders who buy the shares at an exaggerated value. When the firm underreports in equilibrium, the sellers would be the aggrieved party and the following analysis can be easily adapted to address such underreporting in equilibrium. To save space, we do not provide the details of determining the pricing equation when the firm underreports in equilibrium.

$$\alpha_i P_i = \alpha_i \left(\sum_{X(\hat{x}_i)} r_{ji} x_j - \mu_i L \right) + \sigma_i \left((1 - \alpha_i) \sum_{X(\hat{x}_i)} r_{ji} D_{ji} - k^s \right), \tag{45}$$

where $X(\hat{x}_i) = \{x | \rho(\hat{x}_i | x) > 0\}$. Next, the second-generation shareholders' expected payoff from suing with probability σ_i following a report of $\hat{x}_i$ is:

$$\begin{aligned} &\sigma_i \left(\sum_{X(\hat{x}_i)} \alpha_i (x_j - D_{ji} - \mu_i L)\, r_{ji} + \sum_{X(\hat{x}_i)} D_{ji} r_{ji} - k^S \right) \\ &+ (1 - \sigma_i)\, \alpha_i \sum_{X(\hat{x}_i)} (x_j - \mu_i L) r_{ji} \qquad (46) \\ &= \sigma_i \left((1 - \alpha_i) \sum_{X(\hat{x}_i)} D_{ji} r_{ji} - k^s \right) + \alpha_i \sum_{X(\hat{x}_i)} (x_j - \mu_i L) r_{ji}. \end{aligned}$$

In the first line above, the probability of suing, σ_i, is multiplied by the sum of three terms. The first term is the second-generation's share of the firm's outcome net of damages and proprietary costs. The second term is the damages to be paid to second-generation shareholders, and the third term is the cost of suing. If the shareholders' suing strategy is in the interior, then set the derivative of (46) with respect to σ_i equal to zero to determine the firm's optimal disclosure strategy in a mixed-strategy disclosure equilibrium:

$$(1 - \alpha_i) \sum_{X(\hat{x}_i)} D_{ji} r_{ji} = k^s. \tag{47}$$

Letting $r_{ji} = 0.5\rho_{ji}/(0.5\rho_{ji} + 0.5\rho_{j'i})$ for $j,j',i = 1,2$ and $j \neq j'$, the firm's equilibrium disclosure strategy, ρ_{ji}, is derived as the solution to (47).[20] Substituting from (47) in (45) results in the damages dropping out of the pricing equation, yielding:

$$k = \alpha_i \left(\sum_{X(\hat{x}_i)} x_j r_{ji} - \mu_i L \right) = \frac{k}{P_i} \left(\sum_{X(\hat{x}_i)} x_j r_{ji} - \mu_i L \right). \tag{48}$$

Multiplying both sides of (48) by P_i/k establishes Lemma 2(a). In a separating equilibrium, the firm is never sued because it always reports truthfully, and therefore, the term containing damages again drops out of the pricing equation leaving $P_i = \Sigma_j r_{ji} x_j - \mu_i L$.

(b) Next, consider a pooling equilibrium. In the pricing equation (45), the upper bound on k^s, together with the sequential rationality of the shareholders in deciding whether to sue, yields $((1 - \alpha_2) \sum_{X(\hat{x}_2)} r_{j2} D_{j2} - k^S) \geq 0$, so that second-generation shareholders will always sue ($\sigma_2 = 1$). Then, using the result that $k = \alpha_2 P_2$ in (45) and dividing both sides of (45) by α_2 yields:

$$P_2 = \left(\sum_{X(\hat{x}_2)} r_{j2} x_j - \mu_2 L \right) + \sigma_2 [(1 - \alpha_2) r_{12} D_{12} - k^s]/\alpha_2. \tag{49}$$

Next, substituting $D_{12} = \alpha_2 (P_2 - (x_1 - \mu_1 L))$ and $\alpha_2 = k/P_2$ in (49) gives:

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[20] The shareholders' optimal suing strategy in a given equilibrium is established below in a subsequent part of the proof for Lemma 1.

$$P_2 = \left(\sum_{j=1}^{2} r_{j2}x_j - \mu_2 L\right) + \sigma_2\left(\left(1 - \frac{k}{P_2}\right) r_{12}(P_2 - (x_1 - \mu_1 L)) - (k^s P_2/k)\right). \quad (50)$$

The pricing equation in the case of a mixed-strategy equilibrium with $\sigma_i = 1$ follows the lines of the pooling equilibrium above.[21] This completes the proof for Lemma 2. ■

Part 1 of Lemma 1

Assume that $k^R \in [x_1, (x_1 + x_2)/2)$ and $x_2 \geq x_2^* = (x_1^2/k) + x_1$.

Lemma 1.1(a) Separating Equilibrium: We establish the existence of a separating equilibrium for all $L \geq L^*$. Given that the rival's payoffs are identical to the firm's payoff and the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$, the rival will enter only following a report of $\hat{x}_2$ in a separating equilibrium. A separating equilibrium requires that after realizing x_1 and x_2, respectively, the following relations must hold:

$$k + (1 - \alpha_1)(x_1 - \sigma_1 D_{11}) \geq k + (1 - \alpha_2)(x_1 - \sigma_2 D_{12} - L); \text{ and} \quad (51)$$

$$k + (1 - \alpha_2)(x_2 - \sigma_2 D_{22} - L) \geq k + (1 - \alpha_1)(x_2 - \sigma_1 D_{21}). \quad (52)$$

In a separating equilibrium consistent beliefs require that shareholders do not sue, i.e., $\sigma_1 = \sigma_2 = 0$. Further, $D_{11} = D_{22} = 0$, so (51) and (52) reduce, respectively, to:

$$k + (1 - \alpha_1)x_1 \geq k + (1 - \alpha_2)(x_1 - L); \text{ and} \quad (53)$$

$$k + (1 - \alpha_2)(x_2 - L) \geq k + (1 - \alpha_1)x_2. \quad (54)$$

These expressions are identical to (4) and (8), the corresponding expressions in the proof of Proposition 1 [no litigation], respectively. Therefore, the threshold levels, L^* and $\bar{L}$, with litigation are identical to those without litigation, as determined by equations (7) and (10), respectively. As established in the proof of Proposition 1, $x_2 \geq (x_1^2/k) + x_1 \Rightarrow \bar{L} \geq x_1$, and thus for all $L \in [L^*, x_1)$, a separating equilibrium exists. ■

Lemma 1.1(b) Pooling Equilibrium: As before, we assume that, in a pooling equilibrium, both firm types report $\hat{x}_2$ and that investors interpret any off-equilibrium report ($\hat{x}_1$) as coming from a firm with an unfavorable outcome (x_1). Thus, a pooling equilibrium requires that when x_1 is realized:

$$k + (1 - \alpha_2) \times (x_1 - \sigma_2 D_{12} - L) \geq k + (1 - \alpha_1) \times (x_1), \quad (55)$$

where $\alpha_2 = k/P_2$, with P_2 as determined in Lemma 2(b). This price reflects investors' and the rivals' consistent beliefs in a pooling equilibrium given the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$. Similarly, $\alpha_1 = k/P_1 = k/x_1$ represents the investors' off-equilibrium belief that the adverse report comes from the low-type firm. Because $D_{11} = 0$, damages drop out of the RHS of (55). Substituting in (55) for α_1 and α_2 and subtracting k from both sides yields:

$$\left(\frac{P_2 - k}{P_2}\right) \times (x_1 - \sigma_2 D_{12} - L) \geq (x_1 - k). \quad (56)$$

Let $\underline{L}$ be the solution to:

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[21] The existence of well-specified pricing equations and damages can also be established from the previous analysis.

$$\left(\frac{P_2(\underline{L}) - k}{P_2(\underline{L})}\right) \times (x_1 - \sigma_2 D_{12} - \underline{L}) = (x_1 - k) \tag{57}$$

where $P_2(\underline{L}) = (0.5(x_1 + x_2) - \underline{L}) + (0.5 \times (1 - (k/P_2(\underline{L})))(P_2(\underline{L}) - x_1) - (k^s P_2(\underline{L})/k))$, using the results from Lemma 2(b) and the conditions in Lemma 1.1(b). Because the LHS of (56) can be shown to decrease in L, the inequality (56) will be met for all $L < \underline{L}$. Finally, comparing (14) and (57), Claim 9 (Claims 7–9 appear at the end of this proof) establishes that $\underline{L} < L^{**} < L^*$. This proves Proposition 2.1(a).

Similarly, after realizing x_2, a pooling equilibrium requires that:

$$k + (1 - \alpha_2) \times (x_2 - L) \geq k + (1 - \alpha_1) \times (x_2 - \sigma_1 D_{21}), \tag{58}$$

where (as before) $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2$, with P_2 as determined in Lemma 2(b). Given the off-equilibrium belief that the report $\hat{x}_1$ came from a firm with an unfavorable outcome, not suing ($\sigma_1 = 0$) is the optimal off-equilibrium response; and inequality (58), after some algebraic manipulation, reduces to:

$$\left(\frac{P_2 - k}{P_2}\right) \times \left(x_1 - L\left(\frac{x_1}{x_2}\right)\right) \geq (x_1 - k). \tag{59}$$

If (56) is satisfied, then (59) will be also be met because LHS (59) > LHS (56), while the RHS's are the same. Therefore, a pooling equilibrium will exist for all $L < \underline{L} < L^{**}$. Finally, from the proof of part (a), no pure-strategy separating equilibrium can exist for $L < \underline{L}$. ■

Lemma 1.1(c) Mixed-Strategy Equilibrium: Because we know from parts (a) and (b) that for $L \in (\underline{L}, L^*)$ no pure-strategy equilibrium exists, we now conjecture a mixed-strategy equilibrium in which the firm discloses honestly after realizing x_2, but follows a mixed-disclosure strategy after realizing x_1. From the proof of Lemma 2 (see (47)), assuming that the shareholders' suing strategy is in the interior, we can establish that:

$$(\alpha_1 - \alpha_2) D_{12} r_{12} = k^s. \tag{60}$$

With $\rho_{22} = 1$ in the conjectured equilibrium, Bayesian updating gives $r_{12} = 0.5\rho_{12}/(0.5\rho_{12} + 0.5\rho_{22}) = \rho_{12}/(\rho_{12} + 1)$. Substituting this value for r_{12} in (60) yields ρ_{12}, the firm's equilibrium reporting strategy as the solution to (60).

From the assumption that $k^R \in [x_1, (x_1 + x_2)/2)$, we have $\mu_1 = 0$ and $\mu_2 = 1$ for all values of ρ_{12}, given that $\rho_{22} = 1$. Based on the investors' consistent beliefs and the rival's entry strategy in the conjectured mixed-strategy equilibrium and using the prices as determined by Lemma 2, we have $\alpha_1 = k/P_1 = k/x_1$ and $\alpha_2 = k/P_2 = (k/[r_{12}x_1 + (1 - r_{12})x_2 - L])$. Thus, the first-generation shareholders' expected payoff from reporting $\hat{x}_1$ given x_1 (excluding k) is:

$$\pi_{11} = (1 - \alpha_1)x_1 = \left(1 - \frac{k}{x_1}\right) x_1 = x_1 - k. \tag{61}$$

Next, we have:

$$\pi_{12} = (1 - \alpha_2)(x_1 - \sigma_2 D_{12} - L) = \left(\frac{r_{12}x_1 + (1 - r_{12})x_2 - L - k}{r_{12}x_1 + (1 - r_{12})x_2 - L}\right)(x_1 - \sigma_1 D_{12} - L) \tag{62}$$

and because the firm will randomize over reports $\hat{x}_1$ and $\hat{x}_2$ only if the reports yield the same expected payoffs, it must be that $\pi_{11} = \pi_{12}$. Substituting the value of $r_{12} = \rho_{12}/(\rho_{12} + 1)$ in (62) and equating (62) and (61) yields:

$$\left(\frac{x_2 - \dfrac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L - k}{x_2 - \dfrac{\rho_{12}}{\rho_{12} + 1}(x_2 - x_1) - L}\right)(x_1 - \sigma_2 D_{12} - L) = x_1 - k. \tag{63}$$

Given ρ_{12} as defined by (60), the value of σ_2 that solves (63) defines the shareholders' suing strategy after observing $\hat{x}_2$.[22]

After realizing x_2, for the firm to report $\hat{x}_2$ with probability one ($\rho_{22} = 1$) it must be true that:

$$k + (1 - \alpha_2) \times (x_2 - L) \geq k + (1 - \alpha_1) \times x_2, \tag{64}$$

where $\sigma_1 D_{12}$ has been dropped on the RHS of (64) because $\sigma_1 = 0$, given the off-equilibrium belief that a report of $\hat{x}_1$ came from the low-type firm. Similarly, $\sigma_2 D_{22}$ is dropped on the LHS of (64) because $D_{22} = 0$. Rearranging (64) yields:

$$\left(\frac{r_{12}x_1 + (1 - r_{12})x_2 - L - k}{r_{12}x_1 + (1 - r_{12})x_2 - L}\right)\left(x_1 - L\left(\frac{x_2}{x_2}\right)\right) \geq (x_1 - k). \tag{65}$$

Because LHS (65) > LHS (63), while the RHSs are the same, the inequality in (65) is also met, implying that the firm will always report $\hat{x}_2$ after realizing x_2, as conjectured.

Finally, comparing the LHS of (63) to the RHS of (23), in order for both expressions to equal $(x_1 - k)$ with $\sigma_2 D_{12} > 0$ (see Claim 7), it must be that ρ_{12} in (63) is strictly less than ρ_{12} in (23). That is, the probability of false disclosure must be smaller when shareholder litigation is feasible. This establishes Proposition 2.1(b). ■

The existence of the equilibria in all other cases can be established in a similar fashion. We next provide Claims 7–9 without proofs. (However, proofs are available from the authors.)

Claim 7

$$k^s < \left(\frac{(x_2 + x_1 - 2L - 2k)}{x_2 + x_1 - 2L}\right)\left(\frac{k(x_2 - x_1)}{(x_1 + x_2) - 2L}\right) \Rightarrow \sigma_2 > 0 \text{ in a pooling equilibrium.}$$

---

[22] If k^s is such that the shareholders' suing strategy is not in the interior (i.e., if $\sigma_2 = 1$), the firm's equilibrium disclosure strategy is determined as the solution to (63) with $\sigma_2 = 1$ and P_2 determined as in Lemma 2(b). When $\sigma_2 = 1$, the argument for the firm's disclosing $\hat{x}_2$ after realizing x_2 follows the lines provided below, with the modification that P_2 is as determined in Lemma 2(b).

Claim 8

ρ_{12} in (63) is strictly less than ρ_{12} in (23); i.e., the firm overreports less frequently with litigation.

Claim 9

$\underline{L} < L^{**}$.

This concludes the proof for Lemma 1. The firm's equilibrium disclosure behavior, as characterized by Lemma 1, yields the results in Proposition 2.2 and Proposition 2.3. This concludes the proof for Proposition 2. ■

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The Economic Dilution of Employee Stock Options: Diluted EPS for Valuation and Financial Reporting

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ABSTRACT: In this paper, we derive a measure of diluted EPS that incorporates the economic implications of the dilutive effects of employee stock options. We show that the existing FASB treasury-stock method of accounting for the dilutive effects of outstanding options systematically understates the options' dilutive effect, and thus overstates reported EPS. Using firm-wide data on 731 employee stock option plans, our proposed measure suggests that economic dilution from options is, on average, 100 percent greater than dilution in reported diluted EPS using the FASB treasury-stock method.

We examine the implications of our analysis for stock price valuation, the price-earnings relation, and the return-earnings relation. We demonstrate analytically that when firms have options outstanding, empirical applications of equity valuation models that use reported per-share earnings as an input (e.g., Ohlson 1995) yield upwardly biased estimates of the market value of common stock. We predict that when the difference between our measure of economic dilution from options and the FASB treasury-stock method dilution from options is greater, the observed return-earnings and price-earnings coefficients will be smaller, and we provide some (albeit weak) empirical support for this prediction.

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I. INTRODUCTION

Firms use employee stock options more frequently and in larger quantities than they did two decades ago. The median number of shares reserved for stock options as a percentage of shares outstanding for all firms on Compustat increased monotonically from 4.6 percent in 1985 to 8.9 percent in 1995. In our sample, options outstanding (which is a subset of shares reserved for options) grows monotonically from 5.3 percent of shares outstanding at December 1994 to 6.1 percent at December 1997. This growth has drawn scrutiny from members of the investment community, including individual and institutional shareholders, analysts, standard setters, and regulators, many of whom are concerned that diluted earnings per share (EPS) understates the effect of stock options on corporate performance (e.g., see *The Economist* 1999).

As a summary assessment of a corporation's current performance, the EPS number and forecasts of expected EPS are central to fundamental analysis, equity valuation, and performance evaluation. Investors, analysts, and regulators have at least two concerns about the economic impact of employee stock options on EPS. First, what is the appropriate charge to earnings for compensation expense associated with newly granted employee stock options? That is, what is the appropriate *numerator* in the EPS calculation? Second, how much of a firm's earnings accrues to holders of outstanding options? That is, what is the appropriate *denominator* in the EPS calculation such that diluted EPS reflects current performance per unit of common equity?

In this paper, we focus on the proper measurement of the *denominator* in the EPS calculation and provide theoretical and empirical evidence that diluted EPS, when computed using the FASB treasury-stock method, underestimates the economic dilutive effects of outstanding options.[1] Intuitively, the expected future stream of economic earnings drives the total value of a firm's equity, which includes outstanding common stock and options-based equity. That is, both current optionholders and current stockholders have economic claims on the firm's earnings, and the two sets of claimholders share the capitalized value of these earnings. We argue that any diluted EPS measure that summarizes firm performance per unit of common stock must reflect the claim on firm earnings that accrues to outstanding optionholders, and we show that the FASB treasury-stock method fails to achieve this objective.

We begin our theoretical analysis by noting that total equity value is the sum of common stock value and the value of total outstanding options. We use this relation to allocate earnings between stockholders and optionholders, and then scale the earnings allocated to stockholders to obtain a measure of options-diluted EPS that reflects the per-share claim common stockholders have on the firm's earnings. We show that our theoretical measure of option dilution is substantially greater than option dilution computed using the FASB treasury-stock method. We then derive the relation between *changes* in earnings and stock *returns*, and show that the portion of equity value change captured by optionholders is a function of the sensitivity of outstanding options to a change in stock price (i.e., the option delta). We illustrate theoretically that estimated return-earnings and price-earnings coefficients are expected to be smaller, the greater the difference between our measure of dilution from options and FASB treasury-stock method dilution from options.

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[1] Under the treasury stock method, the dilutive shares due to (in-the-money) stock options equals the difference between the number of common shares that would be issued upon exercise of the options and the number of common shares that can be purchased with the proceeds from option exercise.

Our empirical analysis indicates that FASB-diluted EPS systematically underestimates the dilutive effect of employee stock options. In a sample of 731 large firms over the period 1994–1997, the average economic dilution due to stock options (estimated using our method) is equivalent to 2.96 percent of weighted average common shares outstanding. In contrast, the average dilution due to stock options computed using the FASB treasury-stock method is only 1.46 percent of weighted average common shares outstanding. For intensive users of stock options, such as high-growth firms, the difference between our estimate of economic dilution and FASB treasury-stock method dilution is as much as 8 percent of weighted average common shares outstanding. Our empirical evidence on the return-earnings and price-earnings relation provides weak support for our hypothesis that earnings response coefficients estimated using FASB-diluted EPS have greater downward bias as error in the FASB treasury-stock method increases.

Our theoretical analysis has implications for stock price valuation, the price-earnings relation, and the return-earnings relation. For example, our findings highlight that empirical applications of Ohlson's (1995) residual income valuation model using FASB-diluted earnings to estimate per-share stock valuation are misspecified, particularly for relatively intensive users of stock options. The misspecification arises because these models inadequately address the sharing of equity value with optionholders in calculating earnings per share. For some sample firms with large option plans, the value of options outstanding is approximately 10 percent of the market value of common stock. For these firms, ratios of observed share price to estimated fundamental value (calculated using the residual income model applied to FASB-diluted EPS) are understated by about 10 percent, and empirical estimates of the equity risk premium as in Gebhardt et al. (2001) are overstated. Although Ohlson (2000) is pessimistic about the validity of empirical applications of the residual income model for firms that issue stock options, we offer a means of correcting for this misspecification with a deflator that varies with the characteristics of firms' option plans.

Our paper also provides insights for policymakers and accountants concerned with developing a measure of reported diluted EPS that captures the economic dilution from options accurately. Because the FASB treasury-stock method systematically understates the dilutive effects of outstanding options, thereby biasing FASB-diluted EPS upward, this accounting treatment is aggressive, and is neither unbiased nor conservative. In contrast to the FASB-diluted EPS, our measure of options-diluted EPS is consistent with the stated SFAS No. 128 (FASB 1997) objective to produce an EPS number that reflects the effect of dilutive securities.

In related research, Huson et al. (2001) use a sample of firms from 1970 to 1995 and document that the stock return response to changes in accounting income is smaller for firms with more shares reserved for conversion.[2] They also show that the return-earnings response is smaller for firms with higher recent returns, where recent returns proxy for the extent to which options and convertible securities are in the money. Their empirical evidence is consistent with (1) optionholders and other convertible securityholders having a claim on firms' earnings; (2) this claim being proportionately greater when the options and convertibles are further in the money; and (3) the market reflecting the dilution of options and convertibles in stock prices. We extend and formalize Huson et al. (2001) by deriving a method that partitions earnings between stockholders and optionholders based on their relative economic claims. As explained above, we use this method to calculate a measure of options-diluted EPS that is appropriate for both valuation and (potentially) financial reporting. We also identify the nature and source of deviation between our measure of options-diluted EPS and FASB-diluted EPS. Using detailed, hand-collected data on stock option plans for a sample of firms from 1994 to 1997, we document the degree to which FASB-diluted EPS understates stock options' dilutive effect, and provide (weak) evidence that, compared to our theoretical measure of options-diluted EPS, FASB-diluted EPS yields downward-biased estimates of earnings-response coefficients.

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[2] Shares reserved for conversion is the sum of (1) the maximum number of shares issuable upon conversion of convertible debt and preferred stock, (2) the number of stock options outstanding, (3) the number of outstanding warrants plus any other miscellaneous shares reserved for conversion, and (4) the maximum number of shares authorized for future option and restricted stock grants. Shares reserved for conversion is likely to be a noisy proxy for option dilution because it does not capture firm-specific variation in option characteristics, and because it includes shares reserved for future stock and option grants, and these reserved shares are not dilutive. An option grant in the *future* does not affect the *current* division of value between existing stock and option holders, just as, for example, the division of value is unaffected by the firm's future choice to issue convertible debt rather than straight debt.

Section II explains both the FASB treasury-stock method of computing options' dilution in FASB-diluted EPS and our method of adjusting diluted EPS to account for the economic dilutive effect of options. Section III describes sample selection and presents descriptive statistics on the sample firms' option plans. In Section IV, we compare the reported and economic dilutive effects of stock options, and examine whether stock prices and returns reflect the economic dilutive effect of options. We summarize the paper and discuss its implications in Section V.

II. COMPUTING OPTIONS-DILUTED EPS

In this study, we focus on the dilutive effects of employee stock options, and abstract from diluted EPS reporting and valuation issues related to convertible debt, convertible preferred stock, and warrants. In our sample, stock options account for more than 80 percent of all dilutive shares, so we expect the dilutive effects of other convertible securities to be of secondary importance, at least on average. Ninety-four percent of our firms report dilution from stock options, but only 20 percent of our firms report dilution from convertible debt and preferred stock. Further, because the FASB requires the if-converted method to incorporate the dilutive effects of convertible debt and preferred stock into diluted EPS, an analysis of the dilutive effects of these securities is substantially more complex than for stock options.[3]

In the following subsection, we summarize SFAS No. 128 accounting rules for calculating FASB-diluted EPS using the FASB treasury-stock method. Second, we use a price-earnings model to develop a measure of EPS that reflects the economic dilutive effects of employee stock options. We go on, in the third subsection, to operationalize the measure of options-diluted EPS for explaining returns. The fourth subsection compares our measure of options-diluted EPS to FASB-diluted EPS, and discusses the implications of the differences between these measures.

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[3] The if-converted method sets dilutive shares equal to the number of shares that would be issued upon conversion of the security, and adds back the corresponding dividends or interest expense to income (if the shares are determined to be dilutive to EPS; otherwise dilution is zero). Unlike the FASB treasury-stock method for options, which systematically underestimates the economic dilution from options, the if-converted method can overstate *or* understate the economic dilution from convertibles. Whether dilution is overstated or understated is a complex function that depends on both the accounting (the relative magnitudes of the numerator and denominator effects and whether the securities are dilutive) and the economics (prevailing interest rates and embedded call and call-protection features in the convertible). In addition, it is unclear, theoretically, whether one should add back the entire amount of interest expense or dividends to the numerator of EPS, because the holders of the convertibles bear a portion of these costs.

FASB-Diluted EPS under the FASB Treasury-Stock Method

SFAS No. 128 states that the objective of diluted EPS is to measure entity performance over the reporting period while recognizing the dilutive effect of all securities convertible into common stock that were outstanding during the period. Both SFAS No. 128 and Accounting Principles Board Opinion No. 15 (AICPA 1969) require firms to use the FASB treasury-stock method. Under this method, the dilutive shares due to (in-the-money) stock options equals the difference between the number of common shares that would be issued upon exercise of the options and the number of common shares that can be purchased with the proceeds from option exercise. In other words, the FASB treasury-stock method calculates options' dilutive effect using the intrinsic value of an option, which is the difference between the share price and the exercise price of the option. If all of a firm's options have the same exercise price, then the dilutive shares due to options are:

$$\text{Treasury-stock method dilutive shares from options} = N_O \max[0,(P - X)/P]$$

where:

N_O = the number of options outstanding;
P = price per share of the firm's common stock; and
X = exercise price of each option.[4]

We refer to diluted EPS calculated using the treasury-stock method as FASB-diluted EPS.

When the firm's only outstanding dilutive securities are options, we define FASB-diluted EPS as follows:

$$\begin{aligned}\text{FASB diluted EPS} &= \frac{E}{(N_S + N_O[(P - X)/P])} \\ &= E/D_{FASB}\end{aligned} \qquad (1)$$

where:

E = total earnings, and
N_S = the weighted number of common shares outstanding.

We define D_{FASB} as the denominator $[N_S + N_O((P - X)/P)]$ that transforms total earnings E into FASB-diluted EPS.

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[4] If a firm has tranches of options outstanding, N_{Oi}, with differing exercise prices, X_i, then the dilutive shares are computed by summing $\max(0, P - X_i/P) \times N_{Oi}$. In addition, the FASB treasury-stock method requires that firms use the average stock price and number of options outstanding over the fiscal period in computing dilutive shares. Although we take these averaging issues into consideration in our empirical work, in our formal analysis, we assume a single exercise price, X, and a constant number of outstanding options, N_O, without loss of generality. From this point forward, we assume for notational simplicity that the options are in the money, and use $(P - X/P)$ in place of $\max(0, P - X/P)$.

The Effect of Options on the Relation between Stock Prices and Earnings

To derive an EPS measure that incorporates the economic dilutive effects of stock options (options-diluted EPS), we assume that equity value is a function of current economic earnings, E:

$$V_{equity} = V_{equity}(E). \tag{2}$$

This valuation form is quite general and is a transformation of the dividend-discount model of equity valuation (e.g., Williams 1938; Gordon 1962; Fama and Miller 1972; Collins and Kothari 1989; Ohlson 1995). When the firm has outstanding options that can be converted into common stock, the value of equity is the sum of common stock value and option value:

$$\begin{aligned} V_{equity}(E) &= V_{stock}(E) + V_{options}(E) \\ &= PN_S + ON_O \\ &= P[N_S + N_O(O/P)] \end{aligned} \tag{3}$$

where:

V_{stock} = value of the firm's common stock;
$V_{options}$ = value of the firm's outstanding options; and
O = value per outstanding option.

We re-express Equation (3) as the per-share value of the common shareholders' equity:

$$P = \frac{V_{equity}(E)}{N_S + N_O(O/P)}. \tag{4}$$

We operationalize Equation (4) to construct a diluted EPS measure that reflects common shareholders' per-share earnings using the following price-earnings-multiple valuation model:

$$V_{equity} = kE \tag{5}$$

where k is the price-earnings multiple. Substituting Equation (5) into Equation (4) yields:

$$P = \frac{kE}{[N_S + N_O(O/P)]}. \tag{6}$$

The common shareholders' per-share earnings (the options-diluted EPS) is:

$$\begin{aligned} \text{Options} - \text{diluted EPS} &= \frac{E}{[N_S + N_O(O/P)]} \\ &= E/D_{ECON} \end{aligned} \tag{7}$$

where $D_{ECON} = [N_S + N_O(O/P)]$ is the denominator that transforms earnings to obtain options-diluted EPS. Our use of the denominator, D_{ECON}, to obtain options-diluted EPS recognizes that, even though options do not appear as common stock, they imply economic dilution and therefore a diluted EPS measure should decrease in the number of options and the value of each option relative to share price.

An immediate implication of Equation (4) is that commonly used empirical models that express common stock value as a function of earnings or earnings and book value of equity are misspecified for firms with outstanding stock options. For example, consider the following empirical expression of the Ohlson (1995) model:

$$V_{equity}(E)_t = \pi_0 BV_t + \pi_1 RI_{t+1} + \cdots + \pi_k RI_{t+k} \tag{8}$$

where BV is book value of equity, and RI_{t+k} are forecasts of future residual income (forecast earnings minus forecast book value times the cost of capital). To make an accurate transition from Equation (8) to per-share values of common stock outstanding, it is necessary to recognize that both stockholders and optionholders have claims on future earnings. Therefore, to correctly infer the valuation per share of common stock outstanding, the book value, earnings, and forecasted earnings in Equation (8) must be deflated by $[N_S + N_O(O/P)]$ as in Equation (4). Failure to adjust for the economic dilution of outstanding options results in an upward-biased share price.[5] In addition, residual earnings and book value must be adjusted to reflect the economic expense of any wages paid in stock options (Hess and Lueders 2001).[6]

Relating Stock Returns to Options-Diluted EPS

In the previous subsection, we derive options-diluted EPS in a setting that models stock price as a function of the level of earnings. Researchers and investors often model stock *returns* as a function of earnings *changes*. In general, the relation between option price and stock price differs from the relation between changes in option price and changes in stock price. The intuition for this difference is that an option's value is often significantly less than the value of a share of stock (assuming the exercise price is not trivially small), whereas the sensitivity of an option's value to stock price is often similar to that of a share of stock (if the option is not substantially out of the money). To derive a relation between the *change* in equity value (i.e., stock return) and the *change* in earnings, we return to Equation (3) and assume that the change in equity value for a dollar of unexpected accounting earnings, or the earnings response coefficient, is k.[7] Differentiating Equation (3) by E, we obtain:

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[5] We recognize that the computation of per-share earnings is not a necessary intermediate step in estimating stock price. That is, an equivalent estimate of per-share firm value can be obtained by using the level of earnings and book value to compute the total value of equity, subtracting an estimate of the value of options, and scaling the resulting estimate of total common stock value by the number of shares outstanding. We take as given that the FASB wishes to provide users of financial statements with a summary measure of firm performance (diluted EPS) that incorporates the dilutive effects of options and other convertible securities. With respect to valuation, our point is that the analysis and computations necessary for computing options-diluted EPS provide insight into misspecification in common empirical applications of equity valuation models, and that the correction for this misspecification is embedded in our options-diluted EPS measure. We thank the referee for pointing out the linkage between these issues.

[6] Consistent with our analysis above, Hess and Lueders (2001) also show an adjustment is necessary both to the denominator and the numerator of EPS.

[7] Clearly, ERCs are not constant across firms, but vary with firm characteristics. Our theoretical analysis focuses on a single firm, and it is thus unaffected by cross-sectional variation in ERCs. To make the assumption of constant cross-sectional ERC realistic in our empirical analysis below, we create relatively homogeneous firm cross-sections in which the assumption of a constant earnings response coefficient is *a priori* reasonable.

$$\frac{dV_{equity}(E)}{dE} = k = \frac{dP}{dE} N_S + N_O \frac{dO}{dP}\frac{dP}{dE}$$

$$k = \frac{dP}{dE}\left[N_S + N_O \frac{dO}{dP}\right]. \tag{9}$$

Upon rearranging, we obtain:[8]

$$\begin{aligned} dP &= k\frac{dE}{[N_S + N_O(dO/dP)]} \\ &= k(dE/D_{RETURNS}) \end{aligned} \tag{10}$$

which says that the price change is k times the change in earnings divided by the denominator $D_{RETURNS}$, where $D_{RETURNS} = [N_S + N_O(dO/dP)]$.

Like D_{ECON}, $D_{RETURNS}$ is a denominator that adjusts for the dilutive effect of options. When a firm has no outstanding options, $D_{RETURNS}$ equals N_S in Equation (10), which means the sensitivity of stock price to earnings changes is proportional to the change in earnings. When the firm has options outstanding, however, Equation (10) shows that share prices are less sensitive to changes in earnings because a portion of the change accrues to optionholders.[9] The denominator, $D_{RETURNS}$, operationalizes this economic dilutive property by transforming a given dollar change in earnings to adjust for both the quantity of options outstanding and the sensitivity of an option's value to a change in stock price, dO/dP. In the Appendix, we provide a simple example that illustrates the intuition behind Equations (7) and (10).

Bias in Earnings-Based Valuation and Response Coefficients Due to Error in the FASB Treasury-Stock Method

When a firm has stock options outstanding and positive earnings, options-diluted EPS is less than FASB-diluted EPS calculated according to the FASB treasury-stock method. Therefore, valuation models that rely on FASB-diluted EPS yield upwardly biased estimates of per-share stock price, and regressions of stock price on FASB-diluted EPS yield downwardly biased regression coefficients. This follows directly from a comparison of D_{ECON} with D_{FASB}. To show this formally and to provide guidance to our empirical specifications in the next section, we restate the price-earnings relation using options-diluted EPS, i.e., Equation (6):

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[8] We derive the relation between stock returns and earnings changes in Equation (10) for a marginal change in earnings assuming that the option delta, dO/dP, does not change for a marginal change in earnings (i.e., $d(dO/dP)/dE = 0$). Because we empirically test this expression using discrete earnings changes, variation in dO/dP due to earnings changes can induce measurement error in our tests. The derivation of Equation (10) for a marginal change in earnings also requires that the numbers of shares outstanding (N_S) and options outstanding (N_O) are constant. Over a discrete period, these values will change if, for example, the firm issues or repurchases shares. When these changes occur, the amount of equity capital that generates earnings also changes. In empirical tests of Equation (10), we follow prior research and control for these changes using a weighted average of diluted shares outstanding.

[9] While Huson et al. (2001, 596) focus on how shares reserved for conversion affect the permanence of abnormal earnings in an Ohlson framework, their Appendix (Huson et al. 2001, 610) provides intuition similar to ours using an option-pricing framework to illustrate the direction of the effect of dilution from options.

$$P = k\frac{E}{[N_S + N_O(O/P)]} = k(E/D_{ECON}) = k\left[\frac{D_{FASB}}{D_{ECON}}\right](E/D_{FASB}) \tag{11}$$

The first equality in Equation (11) shows that price is k times options-diluted EPS (E/D_{ECON}). Of course, firms do not report options-diluted EPS, but instead report options' dilutive effect on EPS using the FASB treasury-stock method, or a dilution adjustment to earnings using D_{FASB}, as in Equation (1). The second equality in Equation (11) shows that the multiple on FASB-diluted EPS is biased downward relative to k when D_{ECON} is greater than D_{FASB}.

A comparison of D_{ECON} with D_{FASB} from Equations (7) and (1) indicates that D_{ECON} is greater than D_{FASB} as long as the option price O is greater than the option's intrinsic value ($P - X$). Because an option has time value, O always exceeds ($P - X$). Figure 1 plots O/P and $(P - X)/P$ for a typical employee stock option as a function of the price-to-strike ratio (i.e., P/X). We examine options with price-to-strike ratios between 0.5 and 3.0 (less than 4 percent of our sample firms' option plans have average price-to-strike ratios outside these price-to-strike bounds).

The largest discrepancy between O/P and $(P - X)/P$ occurs when the price-to-strike ratio is around 1, that is, when the option is "at the money." For the typical at-the-money, long-duration employee stock option illustrated in Figure 1, option value as a fraction of stock price, O/P, is roughly 0.40. Therefore, each option gives rise to 0.40 dilutive shares. In contrast, the FASB treasury-stock method assumes zero dilutive shares for at-the-money options because $(P - X)/P = 0$. Although the gap between O/P and $(P - X)/P$ shrinks as the price-to-strike ratio deviates from 1, the difference between economic and FASB dilution is substantial throughout the range in the figure. The illustration in Figure 1, in conjunction with Equation (11), motivates a primary hypothesis of this study: that we expect a regression of stock price on FASB-diluted EPS to yield a regression coefficient that is biased downward relative to k.

We scale Equation (10) by lagged stock price, and use the logic in Equation (11) to obtain an expression that links changes in equity value (i.e., stock returns) to scaled changes in earnings:

$$\text{Return} = k\left[\frac{D_{FASB}}{D_{RETURNS}}\right]\left[\frac{dE/D_{FASB}}{\text{Price}_{t-1}}\right]. \tag{12}$$

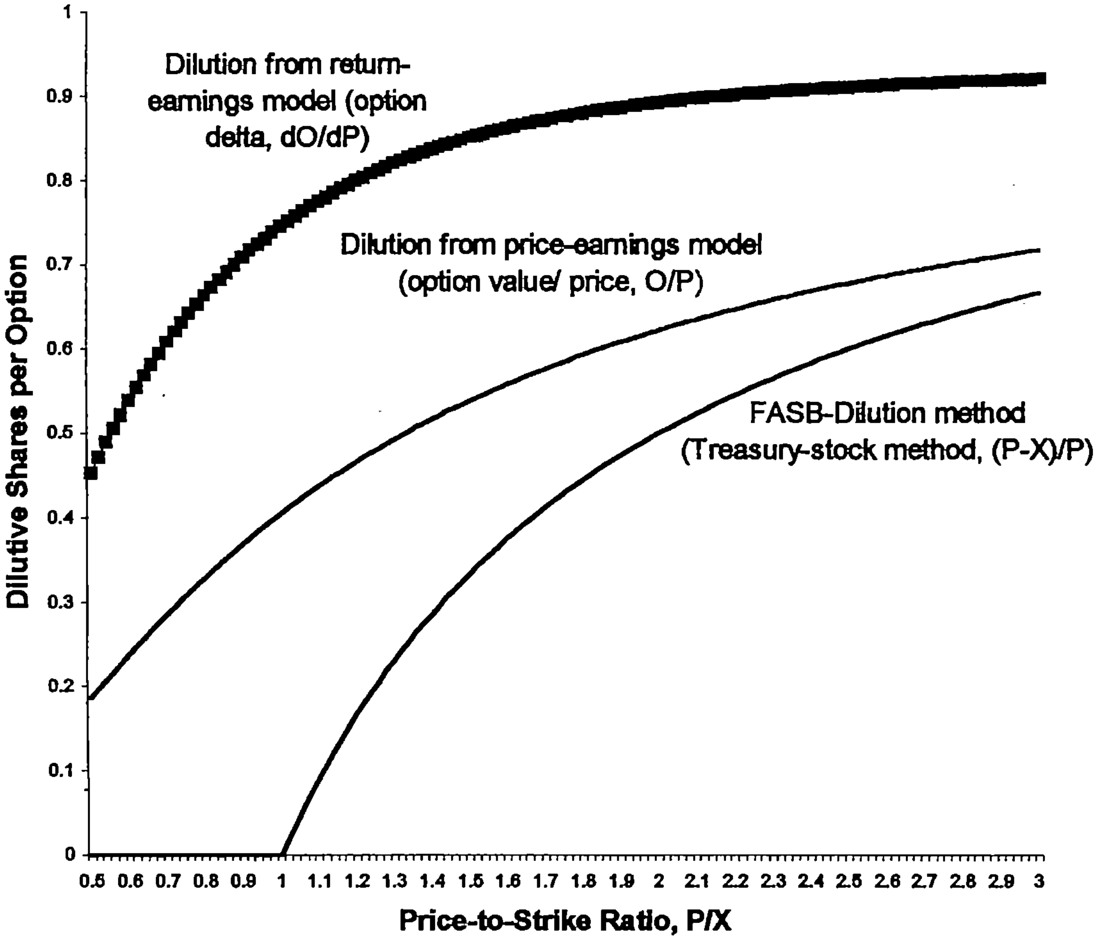
Analogous to the arguments above, the multiple on change in FASB-diluted EPS is biased downward relative to k when $D_{RETURNS}$ is greater than D_{FASB}.

A comparison of $D_{RETURNS}$ with D_{FASB} from Equations (10) and (1) indicates that $D_{RETURNS}$ exceeds D_{FASB} as long as dO/dP is greater than $(P - X)/P$. The dashed line in Figure 1 shows that dO/dP always exceeds $(P - X)/P$. This relation follows from the fact that dO/dP is always greater than O/P, and we have previously shown that O/P is greater than $(P - X)/P$. The intuition for the reason dO/dP is greater than O/P follows from the option valuation model, in which:

$$O = P \times (dO/dP) - x \tag{13}$$

and x is the expected value of the exercise price. Thus, O/P equals $dO/dP - x/P$, from which it follows that dO/dP always exceeds O/P.

FIGURE 1
Comparison of Dilutive Shares Arising from Options, Based on: (1) Option Delta (Return-Earnings Model), (2) Option Value to Price (Price-Earnings Model), and (3) FASB Treasury-Stock Method[a]



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[a] We base the plots on an underlying option with a maturity of 7 years on a stock with a price of $100. We determine the exercise price of the option by the price-to-strike ratio. The stock's dividend yield is 1 percent, the annualized standard deviation of stock returns is 0.30, and the risk-free rate is 6 percent. We value the option using the Black-Scholes formula for valuing European call options, as modified to account for dividend payouts by Merton (1973). Dilution based on option delta applies to the return-earnings model as derived in Equation (10); dilution based on the option value to price applies to the price-earnings model as derived in Equation (7). Dilution based on the FASB treasury-stock method equals $(P - X)/P$, where P is the stock price and X is the option's exercise price.

Figure 1, in conjunction with Equation (12), motivates another primary hypothesis of this study: that we expect a regression of stock returns on the change in FASB-diluted EPS to yield a regression coefficient that is biased downward relative to k. The divergence between $D_{RETURNS}$ and D_{FASB} also implies that common earnings capitalization valuation models that underlie the earnings response coefficient literature (e.g., Kormendi and Lipe 1987; Collins and Kothari 1989) are misspecified for firms that are intensive issuers of stock options, unless earnings are properly adjusted for the options' dilutive effects.

III. SAMPLE AND VARIABLE MEASUREMENT

This section describes our sample selection procedure, explains how we calculate economic dilution due to employee stock options, and provides descriptive statistics on the magnitudes and characteristics of the sample firms' stock option plans.

Sample Selection and Descriptive Statistics

Our data come from four sources. First, we obtain an initial sample of 1,059 firms with December fiscal year-ends from the 1998 Execucomp database. This database contains a broad sample of small, medium-sized, and large firms. Second, we obtain data on outstanding options for fiscal years 1994–1997 from firms' 1997 10-K reports. Third, we use CRSP data to calculate stock returns, stock-return volatility, and Treasury bond yields. Finally, we obtain firms' financial data and industry classifications from Compustat.

We remove firms from the initial sample of 1,059 firms if data are missing from the 10-K, CRSP, or Compustat. We exclude firm-years if an acquirer assumes the target company's options in an acquisition. We also remove any firm-year in which a company makes an acquisition using the pooling-of-interests method and all firm-years prior to this acquisition, because it is not possible to determine the preacquisition composition of the firm's option plan. We remove firm-years with losses because existing accounting rules treat the dilutive effects of options differently for firms with negative and positive earnings. Specifically, there is no adjustment for options for firms with losses (because these options are considered by the FASB to be antidilutive).[10] Furthermore, Hayn (1995) and others show that the relation between returns and earnings differs across firms with positive and negative earnings. Finally, we eliminate the most extreme 1 percent earnings change and return observations. The resulting sample consists of 731 firms and 1,787 firm-years of observations for fiscal years 1995 to 1997.[11]

Table 1 summarizes descriptive information about the sample firms' option plans. On average, the firms have 7 million employee stock options outstanding, or 5.8 percent of reported weighted-average shares outstanding. There is substantial variation in option plan size, with options ranging from 0 percent to more than 31 percent of weighted average common shares outstanding. The average option plan has a Black-Scholes value of $164 million, with the largest plan valued at more than $9.3 billion (the next section discusses our use of the Black-Scholes model in detail). The option plan values are a substantial fraction of firms' market capitalization. The value of the average option plan is 3.1 percent of common stock value, and more than 5 percent of the plans in our sample have a value that exceeds 10 percent of their firm's stock value. These large plan values are not surprising given the large number of options outstanding and the fact that, on average, the options in these plans are substantially in the money. The mean price-to-strike ratio of the options

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[10] The theory in Section II suggests that optionholders participate in both increases and *decreases* in firm value. Thus, options are dilutive even for firms that report losses. Specifically, the analysis in Section II predicts that outstanding options will dampen the negative impact of poor earnings on the value of equityholders' claims. However, the treasury stock method incorporates *no dilution* when earnings are negative, and this substantial difference between economic dilution and reported dilution provides a potential explanation for some of the decreased magnitude of ERCs on loss observations.

[11] We have no observations for 1994 because in order to compare our method with the FASB treasury-stock method, we require two years of data (e.g., we use the 1994 and 1995 year-end option data to compute average diluted shares outstanding in 1995).

TABLE 1
Descriptive Statistics on 1,787 December Fiscal Year-End Observations for a Sample of 731 Firms' Option Plans from 1995–1997[a]

	Mean	*Standard Deviation*	*Minimum*	*Q1*	*Median*	*Q3*	*Maximum*
Total number of options outstanding, millions	7.01	15.99	0.00	0.99	2.36	5.95	177.22
Total options scaled by reported weighted-average shares outstanding	5.79%	4.74%	0.00%	2.55%	4.74%	7.68%	31.31%
Total value of options outstanding, $millions	$163.95	$529.57	$0.00	$9.91	$33.50	$111.25	$9,323.50
Total value of options scaled by market value of common stock	3.08%	3.07%	0.00%	1.02%	2.17%	4.13%	20.41%
Price-to-strike ratio of options (excludes firms with no options)[b]	1.61	0.67	0.35	1.18	1.44	1.87	8.96

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[a] We obtain option plan details from the Execucomp database and firms' 1997 10-K reports; stock price data and Treasury bond yields from CRSP; and financial data from Compustat. We exclude from the sample firm-years with losses; firm years in which an acquirer assumes a target firm's options using the pooling-of-interests method of accounting; and the extreme 1 percent of the firm-years ranked according to earnings changes and stock returns. We base option values on the Black and Scholes (1973) model for valuing European call options, as modified to account for dividend payouts by Merton (1973).

[b] We calculate the price-to-strike ratio of options for each firm using the entire portfolio of outstanding options for each firm, and therefore it represents an average ratio across different options with different exercise prices outstanding for each firm.

outstanding is 1.61, indicating that the stock price exceeds the options' exercise price by an average of 61 percent.

Measuring the Dilutive Effect of Options

Our measure of the dilutive effect of stock options incorporates the optionholders' share of firm value. From Equation (7), the optionholders' share of total firm value in the price-earnings-multiple model depends on the number of shares under option, N_O, multiplied by O, the average value of options outstanding. From Equation (10), the optionholders' share of the change in firm value in the return model depends on the number of shares under option, N_O, multiplied by dO/dP, the average sensitivity of option value to stock price (i.e., the option delta).

We obtain the number of shares under option from 10-K disclosures. However, firms do not report the average value and delta of outstanding options. We use the method developed in Core and Guay (2002) to estimate the value and delta of the entire portfolio of the firm's outstanding options at year-end. The essence of their method is to use the Black and Scholes (1973) model, as modified by Merton (1973) to account for dividend payouts, to calculate the value and delta as if the option portfolio were a single grant. Core and Guay (2002) document that this method yields value and delta estimates that are approximately unbiased in broad samples and highly correlated (> 99 percent) with the measures that would be obtained if the parameters of the individual options in the portfolio were known.

Employee stock options have properties, such as a vesting period and nontransferability, that deviate from the assumptions underlying standard option-pricing models (Hemmer et al. 1994; Huddart 1994; Cuny and Jorion 1995). These features likely contribute to the findings of Hemmer et al. (1996) and Huddart and Lang (1996) that employees tend to exercise options prior to maturity. To address this potential valuation issue, we estimate Black-Scholes value using two alternative expected times-to-maturity: five years and seven years. Although we base our reported results on a seven-year maturity, the results are not sensitive to using a five-year maturity.[12]

Earnings generated over a fiscal period are a function of the average amount of capital outstanding over that period. Therefore, we use the average stock option capital outstanding over the year to estimate both the FASB treasury-stock method dilution and our measures of economic dilution in Equations (7) and (10). The reported data on FASB treasury-stock method dilution already reflect this averaging procedure, as required under SFAS No. 128, and reflect firms' knowledge of the timing of option grants and exercises during the year. However, firms report data on options outstanding only as of the fiscal year-end. Therefore, we estimate average option value and delta as the average of the beginning-of-year and the end-of-year estimates.[13]

We obtain the number of FASB treasury-stock method shares in FASB-diluted EPS directly from the 10-K disclosures. To isolate the dilutive effect of stock options, we record separately the shares due to stock options and other dilutive securities. In about 10 percent of the firm-years, the shares due to stock options are combined with other convertible securities. In these cases, we estimate the FASB treasury-stock method shares from options using data from the option-plan footnote disclosures. We compute the average of FASB treasury-stock method shares at the beginning and end of the fiscal year as a proxy for the actual shares from options included in FASB-diluted EPS.

IV. RESULTS

This section begins with descriptive statistics on the difference between FASB-diluted EPS and options-diluted EPS for the sample firms. We then test the implication derived in the previous section that the understatement of stock option dilution in FASB-diluted EPS downwardly biases estimates of both the stock return response to changes in FASB-diluted EPS, and the stock price response to FASB-diluted EPS.

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[12] To the extent that employees exercise options earlier (later) than we assume, our estimate of Black-Scholes value is upwardly (downwardly) biased. For example, the option value-to-price ratio of an at-the-money, seven-year (five-year) option is 0.40 (0.34) for a stock with volatility of 30 percent per year and annual dividend yield of 1 percent when the risk-free rate is 6 percent. Because these differences in maturity make little difference in the likelihood that the option will finish in-the-money and that the holder will capture a price change, the Black-Scholes option *delta* used in our empirical application of the return-earnings model is less sensitive to the length of the expected exercise period. For example, the delta of an at-the-money seven-year (five-year) option is 0.74 (0.72) for a stock with volatility of 30 percent per year and annual dividend yield of 1 percent when the risk-free rate is 6 percent.

[13] As a practical matter, the prices we use to compute both SFAS No. 128-based diluted EPS and option-diluted EPS reflect average prices for the period over which we compute earnings. The prices we use in computing options dilution are not, in general, equal to the price after earnings are reported, which incorporates the information in reported earnings. This difference between the actual price and the price we use to compute dilution is an additional source of error in both measures in our tests of the influence of option dilution on the relation between stock price and earnings. However, because FASB treasury-stock method dilution is always less than economic option dilution, this error does not change the direction of the coefficient bias in regressions of price on FASB-diluted EPS.

Summary Statistics on Options-Diluted EPS vs. FASB-Diluted EPS

Table 2 summarizes the dilutive effect of stock options on EPS in our sample. Panel A of Table 2 reports that the dilutive shares from stock options under the FASB treasury-stock method is, on average, 1.46 percent of common shares outstanding. Panel B of Table

TABLE 2
Descriptive Statistics on Employee Stock Option-Related Dilutive Shares[a]

	Mean	*Std. Dev.*	*Min.*	*Q1*	*Median*	*Q3*	*Max.*
Panel A: FASB Option Dilution Based on (P − X)/P							
FASB treasury-stock method dilutive shares scaled by weighted average shares outstanding	1.46%	1.66%	0.00%	0.33%	0.95%	1.96%	14.47%
Panel B: Economic Option Dilution in Options-Diluted EPS, Based on O/P							
Option dilution in options-diluted EPS based on O/P scaled by weighted average shares outstanding	2.96%	3.00%	0.00%	0.97%	2.07%	3.90%	22.06%
FASB dilution/Options dilution based on O/P	48.35%	24.41%	0.00%	32.42%	49.64%	65.96%	100.00%
Panel C: Economic Option Dilution for Explaining Returns, Based on dO/dP							
Option dilution for explaining returns based on dO/dP scaled by weighted average shares outstanding	4.54%	4.02%	0.00%	1.81%	3.51%	6.02%	24.90%
FASB dilution/Options dilution based on dO/dP	30.41%	18.03%	0.00%	17.87%	29.07%	41.93%	99.88%

---

[a] The sample contains 731 firms and 1,787 firm-year observations from 1995 to 1997. FASB dilution is the number of dilutive shares due to employee stock options according to SFAS No. 128 financial-reporting requirements. Firms use the FASB treasury-stock method to compute dilutive shares from options, and each option's dilutive shares equals (P − X)/P, where P is the average stock price during the year and X is the exercise price of the option. Economic option dilution in options-diluted EPS based on O/P is dilutive shares from options in the denominator of options-diluted EPS; we derive this measure within a price-earnings model in Equation (7). This dilution measure is equal to the average of beginning- and end-of-year average Black-Scholes value for the options in a firm's stock option plan, divided by stock price (O/P), and multiplied by the number of options outstanding. We base option-dilution for explaining returns on dO/dP; we derive this measure in a returns-earnings model in Equation (10). Dilutive shares in the return-earnings model equal the average of beginning- and end-of-year average Black-Scholes deltas (i.e., dO/dP, or sensitivity of option value to stock price) for the options in a firm's stock option plan, multiplied by the number of options outstanding. We scale all dilution measures by the weighted-average number of shares used in computing basic EPS.

2 shows that our measure of average dilutive shares in options-diluted EPS is 2.96 percent, nearly twice the dilution under the FASB treasury-stock method. The maximum dilution from options is 22.06 percent of common shares outstanding, compared to a maximum of 14.47 percent under the FASB treasury-stock method. The distribution of the data indicates that FASB treasury stock shares are less than half the magnitude of our options-diluted shares for nearly 50 percent of the firm-years.

Panel C of Table 2 reports the fraction of returns (i.e., the change in equity value) attributable to optionholders. The return-earnings model implies average dilutive shares of 4.54 percent, which is about three times as much as the average FASB dilutive shares (1.46 percent). Furthermore, the distribution of the data indicates that FASB dilution is less than half as large as our measure of option dilution in the return-earnings model for nearly 90 percent of the firm-years. Intuitively, an option's dilutive effect in explaining returns is larger than that in Panel B for the price-earnings model because the ratio of an option's value relative to the stock price (i.e., O/P in Equation [7]) is smaller than the dollar change in option value per dollar change in stock price (i.e., dO/dP in Equation [10]). The magnitude of these differences in dilution suggests that the effect of using the downward-biased FASB measure is likely to be material for many firms.

Understated Dilution and Bias in the Return-Earnings Relation

In this section, we present tests of our hypothesis that understated stock option dilution in FASB-diluted EPS downwardly biases estimates of the stock-return response to FASB-diluted EPS changes as Equation (12) predicts. In the following subsection, we examine the downward bias in estimates of the relation between stock price and FASB-diluted EPS as Equation (11) predicts. We focus on the return-earnings relation first because of the concern of a potential correlated-omitted variable bias in the price-earnings levels regressions (e.g., Kothari and Zimmerman 1995; Holthausen and Watts 2001). However, results employing both approaches are similar.

Bias in the Return-Earnings Relation

As expected from the analysis in Section II, the data reported in the previous subsection document that treasury-stock method dilutive shares from options in FASB-diluted EPS do not fully capture the economic dilutive effect of options on EPS. Therefore, we expect the relation between changes in common stock value (i.e., returns) and changes in FASB-diluted EPS to be sensitive to the economic dilutive effects of stock options as derived in Equation (12).

Regression Model

To implement a regression test of the predicted downward bias in the ERC shown in Equation (12), we re-write the equation to include an interaction between the scaled EPS change and $(1 - D_{FASB}/D_{RETURNS})$:

$$\text{Return} = k\left[\frac{\text{d(FASB-diluted EPS)}}{\text{Price}_{t-1}}\right] - k\left[\frac{\text{d(FASB-diluted EPS)}}{\text{Price}_{t-1}} \times \left(1 - \frac{D_{FASB}}{D_{RETURNS}}\right)\right]. \tag{14}$$

The second expression in Equation (14) shows that the return response to FASB-diluted earnings change is lower than k, and is negatively related to the degree to which FASB-diluted EPS understates economic dilution of options (i.e., negatively related to $(1 - D_{FASB}/D_{RETURNS})$.

To estimate Equation (14), we measure FASB-diluted earnings per share with Diluted EPS in sample year 1997 and Primary EPS for earlier years.[14] To ensure that returns fully reflect the information in earnings changes, we use annual returns beginning in the fourth month of the fiscal year through the third month after the end of the fiscal year (we use raw returns to be consistent with measurement of the earnings variable). Because all our data have December fiscal year-ends, our inclusion of year dummies in the regressions controls for market-wide effects in the annual returns.

To operationalize $(1 - D_{FASB}/D_{RETURNS})$ (hereafter "Dilution error_t"), we use the definitions of D_{FASB} and $D_{RETURNS}$ in Equations (1) and (10), respectively, to rewrite this expression as follows:

$$
\begin{aligned}
\text{Dilution error} &= 1 - \frac{D_{FASB}}{D_{RETURNS}} \\
&= \frac{D_{RETURNS} - D_{FASB}}{\text{RETURNS}} = \left[\frac{N_O(dO/dP) - N_O[(P - X)/P]}{N_S + N_O(dO/dP)}\right] \qquad (15)\\
&= \left[\frac{(dO/dP) - [(P - X)/P]}{(dO/dP)}\right] \times \left[\frac{N_O(dO/dP)}{N_S + N_O(dO/dP)}\right] \\
&= [\text{Per-Option Error}] \times [\text{Option Intensity}]
\end{aligned}
$$

where:

N_O = average options outstanding;
dO/dP = per-option delta calculated using Black-Scholes model;
$(P - X)/P$ = per-option FASB treasury-stock method dilution from 10-K disclosures; and
N_S = weighted average shares outstanding exclusive of the effects of stock options, measured as total shares in the denominator of FASB-diluted EPS less $[N_O(P - X)/P]$.

The final expression of Equation (15) illustrates that the dilution error in the return-earnings model is the product of (1) the per-option error, expressed as the percentage difference between the option delta, dO/dP, and the option's intrinsic value, (P − X)/P; and (2) the intensity with which the firm uses options, as measured by the change in the value of options outstanding as a fraction of the change in total equity value for a given price change. We refer to this decomposition of the dilution error below when we discuss appropriate controls for other economic determinants of the earnings response coefficient.

A direct test of our predictions suggests the following regression:

$$
\begin{aligned}
\text{Return}_t = {} & a + b\,[\Delta EPS_{t-1,t}/P_{t-1}] + c\,[\Delta EPS_{t-1,t}/P_{t-1} \times \text{Dilution error}_t] \\
& + \text{year dummies} + e_t \qquad (16)
\end{aligned}
$$

where P_{t-1} is beginning of period price. The year dummies control for market-wide effects in annual returns. Consistent with Equations (12) and (14), we predict that the earnings response coefficient is smaller when FASB treasury-stock method dilution understates economic dilution to a greater degree; thus we predict that the coefficient c in Equation (16) is negative.

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[14] Prior to December 15, 1997, firms reported "Primary EPS"; since that date, in accordance with SFAS No. 128, firms have reported "Diluted EPS." The main difference between the two measures is how they account for dilution from convertible securities. We do not expect that mixing the two EPS measures will affect our inference, because both measures use the FASB treasury-stock method for computing the option dilution. For the remainder of the paper, we refer to both Primary and Diluted EPS as "FASB-diluted EPS."

The modeling above in Section II assumes a cross-sectional constant earnings response coefficient (ERC). Therefore, an important empirical issue in estimating Equation (16) is how to control for other economic determinants of the ERC that are correlated with firms' use of employee stock options. Economic determinants of the ERC, such as size, growth, and risk (see e.g., Collins and Kothari 1989; Biddle and Seow 1991; Ahmed 1994), are correlated with the intensity of firms' use of options for incentive purposes (e.g., Guay 1999; Core and Guay 1999; Bryan et al. 2000).[15] One potential empirical approach to address this problem is to include variables such as growth, size, and volatility as interactive terms with the change in EPS variable. However, untabulated regressions using proxies for ERC determinants interacted with the EPS variable indicate high collinearity between the EPS variable and the interacted EPS variables. For example, the average pairwise correlation between the earnings variable and the interacted earnings variables is 0.81.[16] Although the coefficient on dilution error in this regression is negative, as predicted, the combination of the collinearity problem and the relatively small sample size reduces the statistical significance of all the included variables and yields a specification with low power, thereby rendering the results uninformative.[17]

As an alternative means of controlling for determinants of the ERC that are correlated with options use, we partition the sample firms into relatively homogeneous groups based on variables that we expect to be correlated with cross-sectional variation in ERCs, and allow the coefficient on ΔEPS_t to vary across these groups. We use the following regression specification to estimate distinct ERCs for groups of firms ranked by quintiles of variables that capture variation in ERCs:

$$\text{Return}_t = a + \Sigma[I_p \times (b_p\ \Delta EPS_{t-1,t}/P_{t-1})] + c\ [\Delta EPS_{t-1,t}/P_{t-1} \times \text{Dilution error}_t] + \text{year dummies} + e_t, \quad (17)$$

where I_p is an indicator variable equal to 1 for quintile p, and 0 otherwise.

We estimate this model by ordinary least squares (OLS). Because our data are aligned in calendar time, our standard errors could be understated because of cross-sectional dependence. Therefore, we report t-statistics based on standard errors using Froot's (1989) procedure, assuming that cross-sectional correlation occurs within two-digit SIC code industries, but not across industries. In addition, because our pooled regressions have up to three observations per firm, we modify the Froot (1989) standard errors using Rogers' (1993) method to obtain standard errors that are corrected for the effect of autocorrelated residuals. The resulting standard errors are robust to heteroskedasticity, cross-sectional dependence, and serial correlation in the residuals.

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[15] Leverage is also correlated with both ERCs and stock option use. Because leverage is endogenously determined as a function of a firm's growth opportunities (Barclay and Smith 1995), controlling for growth opportunities also controls for leverage.

[16] The fact that the highest condition index for the regression is 113, and that more than 50 percent of the variance of three coefficient estimates is associated with this coefficient index, suggests severe multicollinearity problems (Belsley et al. 1980).

[17] Following Biddle et al. (1995), we also estimate a model including current and lagged earnings separately in place of the earnings change variable. Including current and lagged earnings separately allows the coefficients on those variables to differ, whereas using the earnings change variable alone forces the same coefficient on both current and lagged earnings. We also estimate the regression model with the dilution error term included as a main effect in addition to the interaction variables. All the alternative specifications yield results similar to those reported.

We estimate Equation (17) by ranking firms on variables that theory suggests are correlated with ERCs: growth (we use lagged values of the book-to-market ratio as a proxy), firm size, and risk (we use the standard deviation of returns as a proxy). However, because all of these variables are likely to be correlated with firms' use of employee stock options, none of these variables in isolation is likely to control fully for the potentially confounding effects described above.

We therefore also estimate Equation (17) using an alternative ranking variable based on the intensity of firms' use of employee stock options. This procedure more fully controls for variation in ERCs that is correlated with options use. Equation (15) shows that the dilution error in the return-earnings model equals the product of a per-option error multiplied by the fraction of value change due to options (i.e., option intensity). We partition our sample into quintiles based on option intensity, as defined in Equation (15), and argue that this variable more fully controls for the multiple confounding correlations between ERC determinants and options use. Because option intensity and ERCs are positively correlated with growth and risk and negatively correlated with size, we expect the ERC will increase across the option intensity quintiles. If this measure is successful, then we expect that the quintiles of firms ranked on option intensity are reasonably homogeneous with respect to our constant-ERC assumption noted above. However, we do not reduce the cross-sectional variation in the per-option dilution error within these quintiles, thereby preserving the power of our tests to detect the valuation implications of the dilution error.

Results

Table 3 reports OLS regression results for Equation (17) using the pooled sample of 1,787 firm-years from 1995 to 1997. We estimate the coefficients on change in EPS separately for each option-intensity quintile (first column) and then for each lagged book-to-market ratio quintile (second column). The regression specification includes untabulated intercept and year indicators.

The by-quintile regression coefficients reported in Table 3 show the expected positive relation between the ERC and option intensity, and provides some evidence of the expected negative relation between the ERC and the book-to-market ratio. The ERC for the most intensive stock option user firms is 8.31, compared to 2.19 for the least intensive users. Similarly, the lowest book-to-market quintile firms' ERC is 3.24, compared to 2.80 for the highest book-to-market quintile firms. The monotonic increase in ERCs across the option-intensity portfolios is broadly consistent with the option-intensity rankings controlling for cross-sectional variation in the ERC, but the nonmonotonic pattern in ERCs across the book-to-market portfolios suggests that these rankings do not fully capture cross-sectional variation in the ERC. The generally small magnitude of our ERC estimates is consistent with the downward-biased estimates typically found in ERC studies.

The results in Table 3 provide mixed evidence in support of our hypothesis that FASB-diluted EPS understates economic dilution of stock options, which in turn dampens the return response to EPS changes. When we use option intensity to control for cross-sectional variation in the ERCs, the coefficient on earnings changes interacted with the dilution error is negative and significant. The coefficient of -55 means that a dilution error equal to 1 percent of weighted-average common shares outstanding is associated with a 0.55 reduction in the ERC, on average. Equation (14) predicts that this coefficient should be of opposite sign and of the same magnitude as the average ERC. Our tests reject the hypothesis that the coefficient of -55 is equal in magnitude, but of opposite sign, to the average ERC of 4.35.

TABLE 3
Variation in the Return-Earnings Relation Due to Error in FASB Treasury-Stock Method Option Dilution: ERC Varies by Option Intensity Quintile and by Lagged Book-To-Market Quintile[a]

$$Return_t = a + \Sigma[D_p \times (b_p\ \Delta EPS_{t-1,t}/P_{t-1})] + c\ [\Delta EPS_{t-1,t}/P_{t-1} \times Dilution\ error_t] + year\ dummies + e_t$$

	Ranked on Option Intensity	*Ranked on Lagged Book-to-Market*
$\Delta EPS_{t-1,t}/P_{t-1}$ − Portfolio 1	2.19 (3.30)	3.24 (3.06)
$\Delta EPS_{t-1,t}/P_{t-1}$ − Portfolio 2	2.59 (4.12)	3.14 (4.06)
$\Delta EPS_{t-1,t}/P_{t-1}$ − Portfolio 3	4.14 (5.37)	1.68 (1.80)
$\Delta EPS_{t-1,t}/P_{t-1}$ − Portfolio 4	4.51 (4.23)	1.87 (2.27)
$\Delta EPS_{t-1,t}/P_{t-1}$ − Portfolio 5	8.31 (4.39)	2.80 (4.48)
$\Delta EPS_{t-1,t}/P_{t-1}$ × Dilution $error_t$	−55.07 (−2.90)	5.41 (0.54)
Adjusted R^2	17.9%	17.0%

[a] The sample contains 731 firms and 1,787 firm-year observations from 1995 to 1997. Dilution $error_t$ equals the fiscal year t value of $[(N_O(dO/dP) - N_O[(P - X)/P])/(N_S + N_O(dO/dP))]$ where N_O = average of beginning- and end-of-year options outstanding; dO/dP = average of beginning- and end-of-year average per-option delta calculated using Black-Scholes model; (P − X)/P = per-option FASB treasury-stock method dilution from 10-K disclosures; and N_S = weighted average shares outstanding exclusive of the effects of stock options, measured as total shares in the denominator of FASB-diluted EPS less $[N_O(P - X)/P]$. $Return_t$ is the annual buy and hold stock return from the fourth month of fiscal year t through the third month after the end of fiscal year t. $\Delta EPS_{t-1,t}/P_{t-1}$ is the change in annual FASB-diluted EPS from fiscal year t − 1 to t scaled by $Price_t$, the stock price at the end of the third month of fiscal year t. D_p is an indicator variable that takes a value of 1 for the pth portfolio, where we form the portfolios by ranking sample firms on option intensity or on lagged book-to-market values. We base the t-statistics (in parentheses) on Froot-Rogers standard errors that are robust to autocorrelation, cross-sectional dependence, and heteroskedasticity.

This finding suggests that although investors do recognize the effects of dilution, either investors overadjust for dilution or our estimates are biased.[18] When we use lagged book-to-market to control for cross-sectional variation in the ERCs, the coefficient on earnings changes interacted with the dilution error is insignificant. When we rank firms on size or the standard deviation of stock returns, the results are similar to those obtained when we rank on book-to-market (not tabulated). Thus, the results in Table 3 provide mixed support for our prediction that the dilution error is associated with variation in ERCs.

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[18] We obtain the same inference if we estimate a regression that allows the interaction coefficient to vary with each quintile portfolio. Specifically, the estimates on the interactions are jointly significantly less than zero, but the coefficients on the individual interactions are significantly different from the negative of the associated ERC. For parsimony, we do not tabulate these results.

Understated Dilution and Bias in the Price-Earnings Relation

We now examine whether the dilution error biases a price-earnings regression model. Following Equation (11), we estimate the following regression model:

$$\begin{aligned} \text{Price}_t = {} & a + \Sigma[I_p \times (b_p\ \text{EPS}_t)] + c\ [\text{EPS}_t \times \text{Dilution error}_t] \\ & + \text{year dummies} + e_t. \end{aligned} \tag{18}$$

We calculate the dilution error$_t$ analogous to Equation (15) with O/P substituted for dO/dP:

$$\begin{aligned} \text{Dilution error} & = \frac{D_{ECON} - D_{FASB}}{D_{ECON}} = \left[\frac{N_O(O/P) - N_O[(P - X)/P]}{N_S + N_O(O/P)}\right] \\ & = \left[\frac{(O/P) - [(P - X)/P]}{(O/P)}\right] \times \left[\frac{N_O(O/P)}{N_S + N_O(O/P)}\right] \\ & = [\text{Per-Option Error}] \times [\text{Option Intensity}] \end{aligned} \tag{19}$$

where:

N_O = average options outstanding;
O/P = per-option price calculated using Black-Scholes model divided by stock price;
$(P - X)/P$ = per-option FASB treasury-stock method dilution from 10-K disclosures; and
N_S = weighted average shares outstanding exclusive of the effects of stock options, measured as total shares in denominator of FASB-diluted EPS less $[N_O(P - X)/P]$.

Table 4 presents the results from the regression specification in Equation (18) using quintiles formed by ranking the firms on their option intensity or on the book-to-market ratio. Equations (10) and (11) show that economic dilution in the price specification is the same as in the returns specification, except that the calculation uses O/P instead of dO/dP. Consequently, the option intensity shown in Equation (19) is the same as that shown in Equation (15), except that O/P replaces dO/dP. As with the return-earnings specification, the coefficient on (EPS × Dilution error) is significantly negative when the ranking procedure uses option intensity, and insignificant when the ranking procedure uses the book-to-market ratio. Consistent with the results in Table 3, the results in Table 4 provide mixed evidence on our hypothesis that understated option dilution in FASB-diluted EPS dampens the association between prices and earnings.

Sensitivity Analysis

As additional tests, we also rank stocks independently on both option intensity and one of the three other economic determinants of ERCs (lagged values of book-to-market, firm size, and return volatility), and we assign ranks of one, two, or three for each ranking variable. This three-by-three independent sort on option intensity and another economic variable creates nine portfolios of firms, and ensures that the observations within each portfolio are relatively homogeneous with respect to two determinants of the ERC. The untabulated results from these specifications are similar to those reported in the first column of Tables 3 and 4 in that the coefficient on the dilution error term is negative and significant.

TABLE 4
Variation in the Price-Earnings Relation Due to Error in FASB Treasury-Stock Method Option Dilution: ERC Varies by Option Intensity Quintile and by Lagged Book-to-Market Quintile[a]

$$Price_t = a + \Sigma[D_p \times (b_p\, EPS_t)] + c\,[EPS_t \times Dilution\ error_t] + year\ dummies + e_t$$

	Ranked on Option Intensity	*Ranked on Lagged Book-to-Market*
EPS_t – Portfolio 1	9.42	14.07
	(10.02)	(17.77)
EPS_t – Portfolio 2	9.78	11.81
	(16.20)	(21.24)
EPS_t – Portfolio 3	11.56	9.83
	(13.81)	(13.59)
EPS_t – Portfolio 4	12.27	10.23
	(14.45)	(8.36)
EPS_t – Portfolio 5	13.01	9.75
	(10.56)	(11.55)
EPS_t × Dilution $error_t$	−82.45	1.31
	(−2.75)	(0.06)
Adjusted R^2	60.2 %	61.4 %

[a] The sample contains 731 firms and 1,787 firm-year observations from 1995 to 1997. Dilution $error_t$ equals the fiscal year t value of $[(N_O(O/P) - N_O[(P - X)/P])/(N_s + N_O(O/P))]$ where N_O = average of beginning- and end-of-year options outstanding; O/P = average of beginning- and end-of-year average per-option price calculated using Black-Scholes model divided by stock price; (P − X)/P = per-option FASB treasury-stock method dilution from 10-K disclosures; and N_S = weighted average shares outstanding exclusive of the effects of stock options, measured as total shares in the denominator of FASB-diluted EPS less $[N_O(P - X)/P]$. $Price_t$ is the stock price per share at the end of the third month after fiscal year t. EPS_t is the FASB-diluted EPS in fiscal year t. D_p is an indicator variable that takes a value of 1 for the pth portfolio, where we form the portfolios by ranking sample firms on option intensity or on lagged book-to-market values. We base the t-statistics (in parentheses) on Froot-Rogers standard errors that are robust to autocorrelation, cross-sectional dependence, and heteroskedasticity.

Finally, we rank stocks independently on both lagged values of book-to-market and one of the two other economic determinants of ERCs: firm size and return volatility. The results from these specifications are similar to those reported in the second column of Tables 3 and 4 in that the coefficient on the dilution error term is insignificant. In summary, the coefficient on dilution error is significantly negative in all specifications that control for option intensity, but is insignificant in all specifications that do not control for option intensity.

As noted in Sections I and II, in addition to appropriately measuring the denominator of EPS, investors, analysts, and regulators are also concerned about the appropriate charge to earnings in the numerator of EPS for compensation expense associated with newly granted employee stock options. SFAS No. 123 (FASB 1995) recommends, but does not require, firms to expense a prorated amount of the value of new option grants. Although few, if any, of our sample firms expense new stock options, empirical evidence suggests that investors consider the cost of options when setting stock prices (e.g., Aboody 1996;

Aboody et al. 2001). The conceptual argument that diluted EPS should reflect both an adjustment to the numerator for compensation expense from options and an adjustment to the denominator to reflect dilution from outstanding options is supported by both the FASB (SFAS No. 123, para. 101) and by theoretical research (e.g., Hess and Lueders 2001).

To ensure the failure of reported earnings to reflect stock option expense does not influence our results, we re-estimate our regressions using earnings that is adjusted for an estimate of pretax option expense.[19] Our adjusted earnings number is equal to reported earnings less an estimate of Black-Scholes value of new option grants in that year.[20] Our inference with respect to the coefficient on the dilution error (not tabulated) is robust to using adjusted earnings. Specifically, the coefficient on the dilution error remains significantly negative for the return-earnings and price-earnings specifications in Tables 3 and 4 that control option intensity, and becomes significantly negative in the Table 3 specification that controls for only book-to-market.

To summarize, our overall results provide weak support for our hypothesis that dilution error is associated with variation in ERCs. We find support for our hypothesis when we control for option intensity, but little support when we control only for other proxies for determinants of the ERC (i.e., book-to-market ratio, firm size, and standard deviation of returns). A potential explanation for these mixed findings is that option intensity better captures the multivariate determinants of ERCs, but no one (or pair) of the other variables eliminates the confounding effects of variation in ERCs that is correlated with firms' use of options.

V. SUMMARY AND CONCLUSIONS

We show theoretically that the FASB treasury-stock method required by SFAS No. 128 systematically understates the dilutive effect of outstanding stock options, thereby upwardly biasing FASB-diluted EPS. We construct an alternative measure of EPS, called options-diluted EPS, that incorporates the value consequences of outstanding options. Empirical results indicate that FASB-diluted EPS incorporates, on average, only 50 percent of the dilutive shares implied by our economic measure options-diluted EPS. The difference in dilutive shares between the FASB treasury-stock method and our method averages 1.5 percent of common shares outstanding, and is as great as 8 percent of common shares for intensive users of options. Furthermore, our evidence suggests (albeit weakly) that the understatement of stock option dilution in FASB-diluted EPS downwardly biases the estimated relation between FASB-diluted EPS and prices and returns.

This study has implications for financial analysis, fundamental analysis, and security valuation. Whether one employs an earnings-based or cash-flow-based valuation model, valuation per share relies on an accurate apportionment of firm equity value among the claimholders, who include shareholders and optionholders (as well as other holders of contingent claims on common stock). Our analysis offers a rigorous and economically intuitive approach to including the dilutive effects of stock options in calculating EPS measures for valuation. We recognize that the computation of per-share earnings is not a necessary intermediate step in estimating the stock price. Our point is simply that the analysis and computations necessary for computing options-diluted EPS provide insight into misspecification in some commonly used empirical equity valuation models, and that the correction for this misspecification is embedded in our options-diluted EPS measure.

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[19] We use pre-tax option expense because some types of employee options provide no tax shield, and for those that do, the amount and timing of the shield does not correspond to the SFAS No. 123 expense. However, we also perform the analysis proxying for the compensation expense with the estimated after-tax Black-Scholes value of the option grants in that year. We compute this after-tax expense at an assumed marginal tax rate of 40 percent, because all sample firm-years have positive earnings. These results are similar to the pre-tax results.

[20] Our method of expensing new option grants differs from *pro forma* option expense under SFAS No. 123, which prorates the value of new option grants over a multiyear period. With respect to the relation between contemporaneous returns and earnings changes, we expect that the change in the value of newly granted options is a reasonable proxy for the change in economic compensation expense from options.

We show that options-diluted EPS theoretically improves upon FASB-diluted EPS, and we believe our measure offers standard setters a useful perspective for thinking about the dilutive effects of options. However, we recognize that there are practical considerations. One such issue is the measurement of employee stock option value. Inherent in any employee stock option valuation model are assumptions about the expected behavior of future stock prices, expected employee exercise behavior, the probability of vesting, and expected dividend policy. The uncertainty and the discretionary nature of these assumptions have led many to question the reliability of option-valuation techniques, such as a modified Black-Scholes model (this problem is similar to that addressed by the FASB in SFAS No. 123 with respect to accounting for the expense of newly granted stock options). Although the FASB treasury-stock method is less susceptible to manipulation, the evidence in this paper shows that the treasury-stock method is not conservative, not representationally faithful, and not likely to be the most relevant measure of option dilution. The choice among dilution measures ultimately depends on the FASB's relative weighting of the conservatism, relevance, and reliability characteristics of the alternative measures.

APPENDIX

EXAMPLE TO ILLUSTRATE OPTION-DILUTED EPS CALCULATIONS SHOWN IN EQUATIONS (7) AND (10)

In this appendix, we use a two-period example to illustrate how stock options affect EPS dilution calculations both in a price-earnings model and in a return-earnings model. We first illustrate a price-earnings model for Company A without options and Company B with options. We then show how Company B's stock price changes when earnings change, and link the return-earnings model to our measure of options-diluted EPS. Table A1 summarizes our assumptions and calculations.

Effect of Options in a Price-Earnings Model

Company A has no options and 20 shares of common stock. The price-earnings multiple and earnings response coefficient is $k = 10$. Therefore, the value of total equity is (10 × \$Earnings), and equity value increases by \$10 for every \$1 increase in earnings. Because there are no options, the firm's equity value and changes in equity value accrue entirely to common stockholders. Thus, options-diluted EPS is equal to earnings divided by 20 shares of common stock. For example, if Company A's earnings are \$100, then total equity value = k × \$100 = \$1,000, stock price = \$1,000/20 shares = \$50 per share, and:

$$\text{Options-diluted EPS} = \$100/20.0 \text{ dilutive shares} = \$5.00 \text{ per diluted share.}$$

Now consider Company B with 20 shares of common stock and 5 stock options. Company B's price-earnings multiple and earnings response coefficient is $k = 10$ and earnings are \$100, making total equity value = k × \$100 = \$1,000. Also assume that (1) the option price is \$20 per option, so the total value of options is 5 × \$20 = \$100; and that (2) total common stock value is \$900, so stock price is \$900/20 shares = \$45 per share. In this setting, from Equation (7):

TABLE A1

Summary of Assumptions and Calculations in a Two-Period Example to Illustrate the Effect of Stock Options on Options-Diluted EPS in Price-Earnings and Return-Earnings Models

	Firm A	*Firm* $B_{Time\,1}$	*Change* $B_{Time\,1}$ *to* $B_{Time\,2}$	*Firm* $B_{Time\,2}$
P/E and ERC	10	10	10	10
Earnings	$100	$100	$20	$120
Total equity value	$1,000	$1,000	$200	$1,200
Shares outstanding	20	20	0	20
Options outstanding	0	5	0	5
Stock price	$50	$45	$8.51	$53.51
Option price		$20	$5.96	$25.96
Option delta		$0.70		
Options-diluted EPS	$5.00	$4.50	$0.85	$5.35

$$\begin{aligned}\text{Options-diluted EPS} &= \$100/[N_S + N_O(O/P)]\\ &= \$100/[20 + 5(\$20/\$45)]\\ &= \$4.50 \text{ per diluted share.}\end{aligned}$$

Thus, options-diluted EPS achieves the objective of preserving the relation between stock price and the price-earnings multiple (i.e., $45 per common share = k × $4.50).

Effect of Options in a Return-Earnings Change Model

We now extend options-diluted EPS to the return-earnings model. Assume Company B's earnings increase by $20 in the next period to $120, with a resulting increase in total equity value of $200 (i.e., $20 earnings increase × k). To compute the new options-diluted EPS, we first determine how the $200 increase in total equity value is shared between the common stockholders and the optionholders. We assume that, for an increase in equity value of this magnitude, each option's value changes by $0.70 for a $1.00 change in the stock price.[21] From Equation (10), the change in per-share stock price equals k times the change in earnings, divided by the denominator $D_{RETURNS}$:

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[21] This example assumes that the firm knows the option delta when it computes diluted EPS. However, the firm will actually use an estimate of the option delta based on the current stock price, which does not incorporate the information in the earnings change that the firm is about to release. If the estimate of option delta differs from the actual delta (i.e., the actual change in option value divided by the actual change in stock price), then the observed ERC will not equal 10. However, if the estimate of option delta is unbiased, then the average ERC will be 10.

$$\begin{aligned}\text{Change in Stock Price Per Share} &= k \times \frac{\text{Change in Earnings}}{[N_S + N_O(dO/dP)]}\\ &= 10 \times \frac{\$20}{[20 + 5(0.7)]}\\ &= \$8.51 \text{ per share}\end{aligned}$$

Thus, when earnings (and total equity value) increase by 20 percent, the per-share stock price increases by $8.51 to $53.51, or 18.9 percent. The less-than-20-percent increase in stock price is due to the fact that the optionholders share a portion of the total increase in earnings and firm value. Total common stock value increases by $8.51 × 20 = $170.20, meaning that 85.1 percent of the $200 increase in equity value accrues to common shareholders. The optionholders receive the remaining $29.80 of the increase in total equity value. Equivalently, when stock price increases by $8.51 per share, each option increases in value by $5.96 ($8.51 × 0.7) to $25.96, and total option value increases by $29.80 ($5.96 × 5 options). As a result, the new options-diluted EPS that links prices to earnings is:

$$\begin{aligned}\text{Options-diluted EPS} &= \$120/[N_S + N_O(O/P)]\\ &= \$120/[20 + 5(\$25.96/\$53.51)]\\ &= \$5.351 \text{ per diluted share.}\end{aligned}$$

Note that after the earnings increase, options-diluted EPS again achieves the objective of preserving the relation between stock price and the price-earnings multiple (i.e., the new price of $53.51 equals k × $5.351).

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The Effect of Information Precision and Information Reliability on Manufacturer-Retailer Relationships

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ABSTRACT: This study investigates the extent to which a retailer's willingness to share internal (sales and inventory) information with a manufacturer and the reliability of the information transmission between the retailer and the manufacturer affect the total supply-chain profits resulting from two alternative inventory management systems. I present analytical models of a traditional system and a Vendor Managed Inventory (VMI) system. The VMI system is a supply-chain management technique in which the retailer delegates its inventory decisions to, and shares its internal accounting information with, the manufacturer. With VMI, the parties aim to reduce inventory-related costs and increase supply-chain profits. The theoretical analysis indicates that the system that produces higher supply-chain profits depends on the extent to which the retailer reveals its internal accounting information to the manufacturer and the ability of the manufacturer to accurately receive and use this information in its decisions. Survey data corroborate the model's prediction that manufacturers are more likely to select VMI when retailers provide more precise sales and inventory (i.e., demand) information and when the manufacturers' systems ensure reliable transmission and receipt of this information. The study's results suggest that managerial accountants should consider the retailer's willingness to share sales and inventory information with the manufacturer and the manufacturer's ability to reliably transmit this information before implementing VMI systems.

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I. INTRODUCTION

This paper examines how both the extent to which a retail firm is willing to share internal accounting information with a manufacturer and the reliability of the information transmission from the retailer to the manufacturer affect the total supply-chain profits resulting from two alternative inventory management systems: a traditional system and a Vendor Managed Inventory (VMI) system. In a traditional system, manufacturers and retailers forecast demand independently; the retailer determines an order quantity and the manufacturer selects a production quantity. In contrast, a retailer using a VMI system delegates her inventory decisions to the manufacturer; the manufacturer determines both the order and the production quantities.

To investigate the relation between internal accounting information, the way firms share this information, and the relative profitability of traditional vis-à-vis VMI systems, I develop a model that incorporates (1) the different sales and inventory (i.e., demand) information that the manufacturer and the retailer hold, (2) the extent to which the retailer shares information with the manufacturer, (3) the manufacturer's transmission and receipt of the shared information, and (4) the contract between the two parties about inventory decision rights. Based on the model analysis, I generate conditions under which VMI systems lead to higher supply-chain profits than do traditional systems.

Traditionally, manufacturers and retailers operate independently. Each uses its forecast of demand to determine the quantity to produce (manufacturers) or to buy (retailers) (Cooke 1992, 57). By contrast, in VMI systems, the retailer relinquishes inventory decision rights to the manufacturer and communicates inventory and/or sales information to the manufacturer. The manufacturer views the retailer's accounting information as a proxy for consumer demand and uses this information to plan production and delivery. The parties expect that through coordination and information sharing, VMI will increase supply-chain profits and efficiency.

The supply-chain-profit benefits the retailer and manufacturer derive from VMI depend on the information the retailer transmits to the manufacturer. The level of detail of the information varies. For example, some retailers share regional warehouse inventory information, while others share store-level sales and inventory information; the latter more finely captures underlying consumer demand. The detail with which the retailer chooses to disclose internal accounting information to the manufacturer (I label this *information precision*) affects the ultimate success of the VMI system. Additionally, the manufacturer must correctly capture the retailer's internal accounting information (I label this *information reliability*). The information has no value if the manufacturer cannot incorporate it into his decision making. Thus, the properties of the information shared between parties influences the success of—and consequently the use of—VMI systems.

The study analytically models both a traditional and a VMI system, compares the expected profits derived under each system, and details the effect of the precision and reliability of the retailer's internal accounting information on the resulting profits. I formulate a double newsboy model in which both parties make quantity decisions and the wholesale price is endogenously determined. In the model, the retailer holds information about consumer demand. This information varies in its precision, representing the extent to which the retailer shares internal accounting information with the manufacturer. The retailer communicates the information to the manufacturer; however, the information transfer may be imperfect. Thus, the consumer demand information received by the manufacturer may be less reliable than the retailer's information. I use the model to compare the traditional and VMI systems in order to generate specific hypotheses about the conditions under which firms will choose to use VMI.

In selecting a system, the manufacturer compares the profit earned from a traditional system with that earned from a VMI system. In the traditional system, the retailer uses a more precise demand signal to make order-quantity decisions, but does not consider total supply-chain costs. In the VMI system, the manufacturer determines the production and order quantity based on total supply-chain costs and less precise and/or less reliable demand information. The model suggests that VMI systems do not necessarily lead to higher expected profits than traditional systems. Rather, the success of VMI depends on the choices both parties make about the information properties. If the manufacturer receives information from a relatively imprecise or unreliable system, then the traditional relationship results in higher expected profits.

I use survey data on 53 manufacturer divisions to test these predictions. The empirical tests support the hypothesis that manufacturers' use of VMI increases with the detail of information transmitted from the retailer and with the manufacturers' ability to capture this information.

The study's findings are relevant to management accountants and supply-chain management executives. The information shared between supply-chain partners influences the type of relationship formed (VMI or traditional) and the success of that relationship. The implication that VMI does not necessarily improve supply-chain performance may surprise management accountants and supply-chain management executives considering implementing VMI. To maximize the supply-chain-profit benefits of VMI, the retailer and the manufacturer should ensure that the internal sales and inventory information the retailer offers is as precise as possible and that the manufacturer is able to receive and use this information accurately.

The paper is organized as follows: Section II describes the related literature. Section III presents the theoretical model and related analyses. Section IV details the hypotheses derived from the models. Section V describes the field-based investigation and empirical tests, and Section VI presents the empirical results. Section VII summarizes the findings and concludes the study.

II. LITERATURE REVIEW

This research relates to four distinct streams of prior work: (1) practitioner and case research, (2) accounting research, (3) operations management research, and (4) transfer-pricing research.

Prior practitioner and case evidence highlights the relation between VMI and supply-chain profitability. Vergin and Barr (1999) find that 80 percent of manufacturers polled increased sales due to VMI (which the authors term "continuous replenishment program"). However, only two out of ten improved the production process, and only one lowered inventory. The authors suggest that these companies may not have adequately integrated the VMI process into their corporate cultures. McKenney (1994, 1995) and Hammond (1995) document that supply-chain management initiatives at Procter & Gamble, Campbell Soup Company, and Barilla SpA are associated with increased profits, lower costs, and more efficient operations, respectively. My research questions whether VMI unambiguously leads to improved supply-chain performance. I specifically examine the condit which VMI systems might lead to higher profitability and focus on the effect of of the internal accounting information on VMI's success.

The accounting literature contains limited theoretical development on how i counting information affects the success of supply-chain management initiatives.[1] focuses on the relative bargaining power of supply-chain partners and the effect has on the resulting contract. Narayanan and Raman (1998) model the use of su contracts, such as VMI, to mitigate the misalignment of retailers' and manufa centives regarding stockouts. My study extends this analytical work to show hov erties of the internal accounting information can influence the profitability of VN

The operations management literature analyzes various supply-chain manag tracts, both analytically and, to a lesser extent, empirically. Tsay et al. (1999) detailed review of this research. I extend the newsboy model from operations formulate models of both the VMI and the traditional systems. Additionally, I the model is consistent with Lee et al.'s (1997) finding that consumer demand is amplified up the supply-chain. In one of the few studies empirically investig and the related benefits, Clark and Hammond (1997) find that VMI and elec interchange (EDI) are associated with better supply-chain performance, and that VMI is associated with greater benefits. Cachon and Fisher (1997) find that V stem partially from sharing demand information. Both studies concentrate on facturer's extensive use of VMI.

My study adds to the existing literature on supply-chain management by m interaction among the properties of the information that the retailer shares, the turer's use of this information, and the resulting inventory management contract. the analytical predictions using a sample of 53 manufacturer divisions (rathe company's adoption).

Finally, the transfer-pricing literature studies decentralized decision making mation sharing within a firm. One may view VMI as an extreme form of decen However, Lambert (2001) points out that inter- and intrafirm relationships dif cantly. Within a firm, no real money transfer occurs; the transfer price acts as a mechanism. By contrast, the wholesale price exchanged between firms results i transfer that directly affects both firms' profits. Thus, it is important to study ho initiatives affect profits. Harris et al. (1982) and Melumad et al. (1992) investigat off between delegating decisions to informed managers and these managers' make myopic decisions. Similarly, this paper examines the allocation of decision the potential for myopic decision making. In the current study, the retailer hol decision rights and the information; the manufacturer must set the wholesale pric the retailer to transfer both to the manufacturer.[2]

III. THEORETICAL MODEL

Basic Model

Consider a manufacturer-retailer relationship in which both parties are risk- inventory-related costs are common knowledge. The manufacturer is the princi retailer is the agent.[3] I compare two asymmetric information models of the manufacturer-retailer relationship.[4] In the first, the manufacturer selects the production quantity and the retailer determines the order quantity, without either party's sharing information (traditional system). In the second, the manufacturer selects both the production and the (retailer's) order quantities, after the retailer provides demand information (VMI system). In selecting a system, the manufacturer must trade off the retailer selecting a myopic, but informed, order quantity (traditional system) against the manufacturer making an "optimal," but less informed, quantity decision (VMI system).

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[1] Baiman and Rajan (2002) review the incentives literature related to buyer-supplier transactions.

[2] Refer to Lambert (2001) for a comprehensive review of research on transfer pricing and informati

The retailer purchases a finished product from the manufacturer and resells it at an exogenously determined price, p, to the end consumer; the manufacturer produces each unit at a cost, c. The parties contract on wholesale price, w. The manufacturer sets the wholesale price to maximize his expected profit while offering the retailer at least a reservation amount of profit, U_r, in expectation. I assume, without loss of generality, that U_r is zero.[5]

I use a double newsboy model, in which the contracting parties select both the production quantity, Q_m, and the order quantity, q_i, to analyze the quantity and wholesale price choices under the traditional and VMI systems. The subscript, i, refers to the party making the related decision; i = r if the retailer makes the decision and i = m if the manufacturer makes the related decision. The theoretical results generalize to models that include return policies, stockout penalties, and additional layers of stockout and holding costs (Cohen 2000). Figure 1 presents the timeline of both the traditional and the VMI systems. The systems differ in the manner in which the retailer communicates information to the

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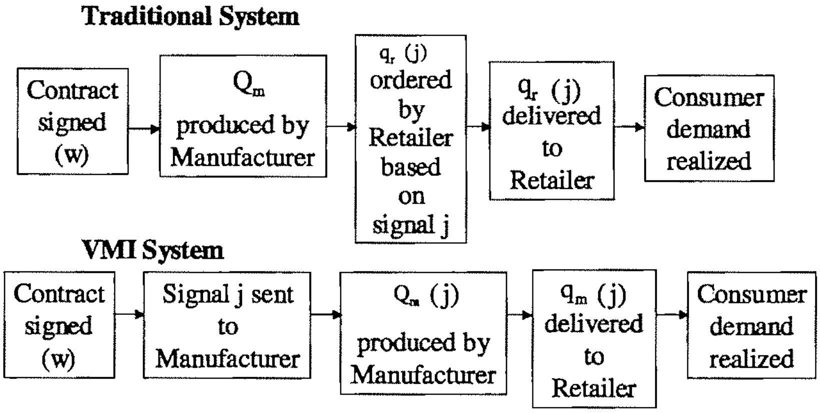
[3] If I reverse the principal and the agent (i.e., if the retailer becomes the principal), then the traditional system will achieve first-best profits; the retailer will have both precise information and decision rights. The addition of manufacturer moral hazard and/or asymmetric information generates less than first-best profits. The empirical tests focus on the manufacturer's use of VMI and the information flow between the parties rather than on manufacturer or retailer effort. Accordingly, I model the manufacturer as the principal and omit moral hazard from the model.

[4] A solution to the first-best, full information setting is available from the author upon request.

[5] The reservation level of profit reflects the retailer's bargaining power, the opportunity cost of shelf space, and manufacturer competition. See Cohen (2000) for further analysis without assuming zero reservation level of profits.

FIGURE 1
Model Timeline of Traditional and VMI Systems


Traditional System
Contract signed (w)
Q_m produced by Manufacturer
q_r (j) ordered by Retailer based on signal j
q_r (j) delivered to Retailer
Consumer demand realized
VMI System
Contract signed (w)
Signal j sent to Manufacturer
Q_m (j) produced by Manufacturer
q_m (j) delivered to Retailer
Consumer demand realized

manufacturer. In the traditional system, the retailer does not reveal the demand signal directly to the manufacturer. The retailer orders $q_r(j)$ based on the actual signal received $(j \in \{1,2,...,J\})$ and the manufacturer produces $Q_m()$ according to his priors (i.e., the manufacturer does not receive a signal from the retailer prior to production).

The VMI system incorporates communication. The retailer communicates the demand signal directly to the manufacturer, who produces $Q_m(j)$ and delivers $q_m(j)$ based on the transmitted signal. I assume that the retailer reports the information truthfully. The contract ensures that using a VMI system will not harm the retailer. Distorting the information would cause the manufacturer to determine the order quantity based on inaccurate information; a suboptimal order quantity and higher inventory costs would result. Thus, it is in the retailer's best interest to communicate the signal truthfully.

The retailer truthfully communicates the information; however, the transmission and/or receipt of the information still may be unreliable (i.e., information may be lost during or omitted from the transmission). I operationalize the reliability of the manufacturer's information through α, the probability that the signal is correctly communicated to and received by the manufacturer (i.e., the level of signal reliability). That is, α represents the amount of information not lost. Due to lost information, the signal is unreliable with probability $1 - \alpha$.[6]

In both systems, the parties have the same initial priors about consumer demand, D (i.e., $D \sim U[\mu - \sigma/2, \mu + \sigma/2]$). The length of the distribution, σ, represents consumer demand variability and μ represents expected demand.[7] As σ increases, the variance of consumer demand also increases. After signing the contract but before placing an order, the retailer receives refined consumer demand information and updates her priors. The retailer's private internal accounting information about sales and inventory proxies for consumer demand. The retailer's information indicates which of J equal-sized partitions of the demand distribution will occur. Consumer demand is distributed uniformly on this partition. For example, signal j indicates that $D \sim U[\mu + (\sigma(2j - J - 2))/2J, \mu + (\sigma(2j - J))/2J]$. The variable J represents information precision. As J increases, the distribution is segmented into finer divisions. Small J may be associated with the transmission of warehouse information, whereas large J may be associated with more precise point-of-sales (POS) data. Therefore, a large J enables the retailer to predict consumer demand more accurately.

After the manufacturer transfers the goods at the wholesale price, demand is realized. The retailer incurs stockout costs, s, if demand exceeds available product, and holding/disposal costs, h, if demand falls short of available product.[8] Consistent with the operations management literature, I assume that a manufacturer that does not have enough inventory on hand must produce or obtain the shortfall and deliver the units to the retailer prior to consumer demand realization.

Based on the above description, the manufacturer's profit, Π, given inventory decision-maker i, signal j, and inventory management system k can be expressed as (variable definitions are summarized in Table 1):

$$\Pi_m^k\,(q_i(j),Q_m(j),w) = wq_i(j) - cQ_m(j) \tag{1}$$

$$k \in \{Trad,VMI\},\ i \in \{r,m\},\ j \in \{1,2,...,J\}.$$

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[6] I model only the retailer as possessing private information. I expect greater gains if the manufacturer also possesses private information and combines the two sets in his estimation of expected demand.

[7] $E[D] = \mu$, $Var[D] = \sigma^2/12$.

[8] If the retailer sells a substitute product, then s represents the difference in the selling prices of the two products. A negative h implies that excess inventory has a positive value.

TABLE 1
Glossary of Notation

Endogenous Variables	
$q_i(j)$	Quantity ordered by party i where $i \in \{r,m\}$, $j \in \{1,2,\ldots,J\}$
$Q_m(j)$	Quantity produced by the manufacturer where $j \in \{1,2,\ldots,J\}$
w	Wholesale price per unit
π_m^k	Expected manufacturer profit given system k where $k \in \{\text{Trad,VMI}\}$
π_r^k	Expected retailer profit given system k where $k \in \{\text{Trad,VMI}\}$
Exogenous Variables	
p	Retail price per unit
c	Manufacturer's production cost per unit
h	Holding cost per unit for the retailer
s	Stockout cost per unit for the retailer
μ	Expected consumer demand
σ	Length of the initial consumer demand distribution
D	Initial priors about consumer demand, $D \sim U\left[\mu - \frac{\sigma}{2}, \mu + \frac{\sigma}{2}\right]$
α	Probability that the signal is reliable
J	Number of partitions offered by the information system; information precision
U_r	Reservation utility of the retailer

The manufacturer sells the quantity ordered, $q_i(j)$, to the retailer at a price of w per unit and incurs a unit production cost of c on the amount produced, $Q_m(j)$. The party responsible for determining the order (i.e., i) differs between the two systems; the retailer selects $q_i(j)$ in the traditional system, and the manufacturer chooses it in the VMI system.

Similarly, given inventory decision-maker i, signal j, and inventory management system k, the retailer's profit is represented by:

$$\begin{aligned}\Pi_r^k\,(q_i(j),w) = {} & \int_{\mu+\sigma(2j-J-2)/2J}^{q_i(j)} (pD - wq_i(j) - h(q_i(j) - D)) \left(\frac{J}{\sigma}\right) dD \\ & + \int_{q_i(j)}^{\mu+\sigma(2j-J)/2J} (pq_i(j) - wq_i(j) - s(D - q_i(j))) \left(\frac{J}{\sigma}\right) dD \qquad (2)\end{aligned}$$

$$k \in \{\text{Trad,VMI}\},\ i \in \{r,m\},\ j \in \{1,2,\ldots,J\}.$$

The retailer purchases $q_i(j)$ from the manufacturer. The first integral reflects the retailer's expected profit if the inventory on hand exceeds consumer demand. The retailer meets demand and incurs a holding cost, h, on each unit remaining. The second integral represents the retailer's expected profit when consumer demand exceeds the quantity on hand. The retailer sells current inventory and incurs a stockout cost on each unit order that remains unfilled.

I assume that the wholesale price is determined prior to any information transmission or quantity decision. The contract cannot be renegotiated. Refer to Baiman (1990) for a discussion of renegotiation research in both complete and incomplete contract settings.[9]

Traditional System

In the traditional system, the retailer receives the consumer demand signal and indirectly transmits the signal to the manufacturer via her order, $q_r(j)$. Because the retailer's cost and information structure are common knowledge, the manufacturer can compute the retailer's order, given each of the J signals. Thus, the manufacturer faces a discrete retailer demand distribution with J points and a variance of $1/J \sum_{j=1}^{J} q_r(j)^2 - [1/J \sum_{j=1}^{J} q_r(j)]^2$. After she receives the signal, the retailer faces consumer demand with a variance of $\sigma^2/12J^2$. The manufacturer's problem is:

$$\max_{w,Q_m(),q_r(j)} E[E[\Pi_m^{Trad}(q_r(j),Q_m(),w)|j]] \tag{3}$$

subject to:

$$E[E[\Pi_r^{Trad}(q_r(j),w)|j]] \geq 0 \tag{4}$$

$$q_r(j) \text{ maximizes } E[\Pi_r^{Trad}(q_r(j),w)|j] \ \forall j \in \{1,2,\ldots,J\}. \tag{5}$$

Based on his cost structure and the retailer demand distribution, the manufacturer selects a production quantity to maximize expected profits (Equation (3)). Equation (4) represents the retailer's participation constraint. The manufacturer must ensure that the retailer earns at least her reservation level of profits in expectation. Finally, the retailer selects an order quantity to maximize her expected profits (Equation (5)).

Proposition 1: With J = 2, the results of the traditional system are:

$$q_r(1) = Q_m() = \frac{\mu}{2} - \frac{\sigma(p + 2c + 3h + s)}{8(p + h + s)} \quad q_r(2) = \frac{\mu}{2} + \frac{\sigma(3p - 2c + h + 3s)}{8(p + h + s)} \tag{6}$$

$$w = \frac{\sigma(p + 2c - h + s) + 4\mu(p + h + s)}{4\sigma} \equiv w^{Trad} \tag{7}$$

$$E[E[\Pi_m^{Trad}(q_r(j),Q_m(),w)|j]] = \frac{\begin{pmatrix} 8\sigma\mu((p + s)(p + s - 2c) - h(h + 2c)) \\ + 16\mu^2(p + h + s)^2 + \sigma^2((p - 2c - h + s)^2) \end{pmatrix}}{32\sigma(p + h + s)} \tag{8}$$

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[9] I omit some distinguishing features of VMI, including more frequent deliveries, transportation costs, and system set-up and maintenance costs. The survey I use in the empirical analysis does not encompass these issues. Because the related hypotheses are not currently testable, I exclude these features from the theoretical model.

$$E[E[\Pi_r^{Trad}\,(q_r(j),w)|j]] = \frac{\begin{pmatrix}3(p+h+s)^2(4\mu-\sigma)^2 - 16\sigma\mu(p+h+s)(p-c)\\ +\,4\sigma^2(p-c+s)(c+h)\end{pmatrix}}{32\sigma(p+h+s)} \equiv \Pi_r^T. \tag{9}$$

Proof. All proofs are presented in the Appendix.

The retailer selects an order quantity (Equation (6)) based on both parties' cost structures and the consumer demand signal. The retailer bases the order quantity on expected demand, $\mu/2$, and adjusts this estimate up or down based on the signal observed. As shown in the proof of Proposition 1 in the Appendix, in selecting the production quantity, the manufacturer minimizes his production costs and produces at the lower bound. The absence of communication means the manufacturer must produce before receiving the order. This results in a potential mismatch between the production and order quantities and an increased probability of a shortfall at the production site. The wholesale price, reflected in Equation (7), serves two purposes: it both induces the correct quantity choice and allocates profits between the two parties. Because it cannot do both tasks perfectly, the manufacturer decides which task w should emphasize, given the parameter values. For example, if the supply-chain profit margin is high, the manufacturer may use the wholesale price to influence the retailer's order quantity. In this case, the manufacturer would sacrifice a portion of his expected profits (Equation (8)) to induce the retailer to order the desired quantity.[10] Consequently, the retailer earns expected profits equal to the expression in Equation (9).

VMI System

VMI delegates the inventory decision to the manufacturer. The retailer truthfully communicates the consumer demand signal prior to production. With probability α, the manufacturer receives full information and uses it in his quantity choice. With probability $1 - \alpha$, information is lost in transmission ($j = (\,)$), and the manufacturer bases production on his initial priors. The probability proxies for information reliability and is public knowledge. The manufacturer's problem is:

$$\begin{aligned}\max_{w,q_m(j),Q_m(j)}\ & \alpha(E[E[\Pi_m^{VMI}\,(q_m(j),Q_m(j),w)|j]])\\ & +(1-\alpha)E\,[\Pi_m^{VMI}\,(q_m(\,),Q_m(\,),w)]\end{aligned} \tag{10}$$

subject to:

$$\alpha(E[E[\Pi_r^{VMI}(q_m(j),w)|j]]) + (1-\alpha)E[\Pi_r^{VMI}(q_m(\,),w)] \geq \Pi_r^T. \tag{11}$$

The manufacturer selects an order and a production quantity to maximize his expected profits (Equation (10)). The participation constraint (Equation (11)) ensures that the retailer expects to earn at least as much as she would earn in the traditional model. The retailer will not agree to use a VMI system unless the manufacturer guarantees the retailer will be at least as well off as in the traditional system.

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[10] There are parameter values for which the wholesale price primarily allocates profits, and the retailer's participation constraint binds. The retailer does not internalize total supply-chain costs in this scenario; w and $q_r(\,j)$ depend only on the retailer's costs. Because this scenario produces similar qualitative results, I do not present it in the text.

Proposition 2: With J = 2, the results of the VMI system are:

$$q_m(1) = Q_m(1) = \mu - \frac{\sigma(c + h)}{2(p + h + s)} \qquad q_m(2) = Q_m(2) = \mu + \frac{\sigma(p - c + s)}{2(p + h + s)} \tag{12}$$

$$q_m() = Q_m() = \mu + \frac{\sigma(p - 2c - h + s)}{2(p + h + s)} \tag{13}$$

$$w = \frac{4(\mu p - \Pi_r^T)(p + h + s) + \sigma(\alpha - 2)(c^2 + h(p + s))}{\sigma(\alpha - 2)(2c - p + h - s) + 4\mu(p + h + s)} \equiv w^{VMI} \tag{14}$$

$$E[E[\Pi_m^{VMI}(q_m(j),Q_m(j),w)|j]] = -\Pi_r^T + \mu(p - c) + \frac{\sigma(\alpha - 2)(p - c + s)(c + h)}{4(p + h + s)} \tag{15}$$

$$E[E[\Pi_r^{VMI}(q_m(j),Q_m(j),w)|j]] = \Pi_r^T. \tag{16}$$

When delegated the inventory decision, the manufacturer selects the production and order quantity that optimizes the entire supply-chain, given the demand information received from the retailer.[11] Equations (12) and (13) represent the manufacturer's quantity choice, given the signal received from the retailer. The production and order quantities are equivalent in the VMI system; these quantities weigh the supply-chain's stockout costs against its holding costs. The wholesale price and manufacturer's expected profit, Equations (14) and (15), respectively, depend on the reliability of the information received from the retailer, the exogenous parameters, and the profit earned by the retailer in the traditional system. The retailer earns the amount that she would have earned in the traditional system (Equation (16)).

Model Comparisons

The following propositions, which form the basis for specifying the empirical hypotheses, characterize the differences between VMI and traditional systems.

Proposition 3:

$$q_m(j) \geq q_r(j) \quad w^{VMI} \leq w^{Trad}. \tag{17}$$

In the VMI system, the manufacturer delivers a quantity to the retailer that exceeds the amount the retailer would have ordered in a traditional system. The manufacturer makes this quantity decision based on the higher supply-chain unit margin of p − c rather than on the (smaller) retailer's margin of p − w. This higher supply-chain margin results in a more expensive cost of a stockout, so the manufacturer delivers a larger amount to the retailer to avoid a stockout.

The manufacturer charges the retailer a lower wholesale price in the VMI system than he charges in a traditional system. The amount the manufacturer delivers to the retail store exceeds the amount the retailer would order if given the decision rights. To induce the retailer to switch to VMI, the manufacturer guarantees that the retailer's expected profit will remain the same. The information gains derived from a VMI system enable the manufacturer to lower the wholesale price, increase profits, and yet still ensure that the retailer earns at least as much as she would have earned in a traditional system.

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[11] If $\alpha = 0$, then this model reduces to a fully collaborative model given no signal, and if $\alpha = 1$, then this model reduces to a full-information, fully collaborative model.

Proposition 4: Holding p, c, h, s, and E[D] constant and letting $\Delta\Pi$ equal the difference in the manufacturer's profits between the VMI and traditional systems ($\Pi_m^{VMI} - \Pi_m^{Trad}$), $\Delta\Pi$ is:

1. Increasing in information reliability (α);
2. Ambiguous in consumer demand variability (σ); and
3. Larger with higher information precision (J), for $J = \{2,3\}$.

In selecting a system, the manufacturer trades off an order that is based on full information, but that does not reflect his cost structure, against one that considers total supply-chain costs but is based on potentially incomplete information. The greatest benefits of VMI occur when the demand signal is reliable (high α). High information reliability corresponds to low information asymmetry. The more accurate manufacturer forecast results in higher manufacturer profits, stable retailer profits, and, thus, increased total supply-chain profits. Increases in consumer demand variability lead to ambiguous effects on the difference between the two systems. The direction of this comparative static depends on the values of the exogenous parameters. Finally, more precise information (higher J) leads to greater benefits from VMI because the manufacturer can predict consumer demand more accurately.

IV. HYPOTHESES DEVELOPMENT

Comparisons between the VMI and traditional systems lead to the following empirical predictions.

Relationship between Information, Product Characteristics, and VMI Use

I expect manufacturers to use VMI when the expected profits under VMI exceed those under the traditional system (i.e., when the consumer demand information is reliable and precise). Proposition 4 posits that the benefits of VMI depend partially on the reliability and precision of information about consumer demand. VMI users require sophisticated information linkages to ensure accurate data transfers to the manufacturer, and the retailer must reveal precise sales and inventory information in order for the information to benefit the manufacturer. Proposition 4 shows that the difference in profits between the VMI and traditional systems increases as the information reliability (α) and information precision (J) increase.[12] Furthermore, as the difference between the expected profits under the two systems increases, I expect the manufacturer to use VMI to a greater extent (i.e., with a higher percentage of volume).[13]

The theoretical results in Proposition 4 also imply an ambiguous relation between consumer demand variability and VMI use. Prior research also finds conflicting results about the relation between VMI use and consumer demand variability. Xu (1996) argues that manufacturers find it difficult to forecast when consumer demand is variable, so information sharing provides more accurate information on consumer demand and allows the manufacturer to plan more effectively. In contrast, Fisher (1997) contends that manufacturers should use VMI for products with more stable demand. Given the ambiguous sign in Proposition

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[12] This hypothesis does not reflect the additional cost of gathering and communicating more precise information.

[13] This expected effect of the benefits of VMI on the volume flowing through the VMI system is consistent with Clark and Hammond (1997).

4 and the conflicting results from prior research, I make no prediction about the direction of the relation between consumer demand variability and the extent of VMI use.

To summarize, I test the following hypotheses (stated in alternative form):

H1a: Manufacturers use VMI systems to a greater extent when the retailer transfers more reliable and precise information.

H1b: Manufacturers use VMI systems to either a greater or a lesser extent when consumer demand variability is high.

Effect of VMI Use on Wholesale Price

Proposition 3 states that the manufacturer charges a lower wholesale price in the VMI system than in the traditional system. The lower wholesale price represents the retailer's information rents and her portion of the efficiency gains. I expect more intensive use of VMI to increase efficiency gains, thereby leading to further reductions in the wholesale price. This leads to the following hypothesis:

H2: The more intensively the manufacturer uses VMI, the lower the wholesale price.

V. EMPIRICAL RESEARCH DESIGN

Research Setting

The empirical analysis focuses on the food and consumer packaged goods (F&CPG) industry. This highly competitive industry has been an early developer and adopter of information sharing and collaboration among supply-chain members. Accordingly, the F&CPG industry provides fertile ground for exploring manufacturer-retailer logistic partnerships. However, the results might not generalize to other industries.

Research Procedure

Survey Instrument

Written jointly with Andersen Consulting, the Stanford University Supply-Chain Management Forum, and Research, Inc., the survey encompasses a broad range of supply-chain management initiatives and is based on conversations with executives about collaborative techniques and performance measurement in the industry. During pre-survey interviews, we found that most companies/divisions do not track the financial impact of collaborative partnerships; managers can only approximate the financial effect of, say, a switch to VMI. Therefore, the survey uses ordinal scales. The survey comprised six parts: business strategies and characteristics, new product collaboration, information linkages, material linkages, outsourcing and distribution, and inventory collaboration. We asked respondents to answer the questions as they related to the primary products in the respondents' divisions. I specifically formulated a subset of questions related to VMI that included the company's use of VMI, the type and extent of information sharing, and the sophistication of the manufacturer's information linkages. We mixed the VMI questions with other questions to mitigate any bias that might occur were a single section to ask questions only about VMI.

An external market research firm conducted a 30- to 45-minute phone interview with each executive responsible for collaboration and/or logistics at the division level. Telephone inquiries result in fewer nonresponses and allow interaction between the interviewer and interviewee. One-on-one contact allows the interviewer to define various terms in the survey consistently.

Sample

The sample consists of 100 divisions of manufacturers in the F&CPG industry. Sixty-one respondents are divisions of public companies, and many are different divisions of the same entity. Due to incomplete responses to the VMI questions, I analyze only 53 divisions. Ideally, I would use financial data to analyze the effect of VMI adoption and link manufacturers to their retail partners to assess individual and total supply-chain changes. However, it would be difficult to assess any relationship between a company's consolidated financial statements and initiatives adopted at the division level. Additionally, because the data are cross-sectional rather than longitudinal, I am unable to test the effects of VMI on division performance over time. Thus, the empirical analysis is based solely on the survey responses and relies on self-reported assessments of costs, benefits, information sharing, and collaboration. The estimates offer insight into the information and division characteristics associated with VMI use.

Empirical Variables

The hypotheses suggest that manufacturers' use of VMI depends on the precision and reliability of the information the retailer shares, and possibly on consumer demand variability. I also expect manufacturers' use of VMI to lead to lower wholesale prices. The following sections describe the empirical variables used to test these theoretical predictions.

Dependent Variables

The dependent variable used to test H1a and H1b is the extent of VMI use. Each responding manufacturer division indicated whether the division uses VMI and, if so, the percent of sales that flow through the VMI system. Extent_VMI categorizes the divisions based on their response to a six-point scale ranging from "no VMI" to "VMI with 91 percent or more of your sales."

To test the relation between VMI and the wholesale price transferred between the manufacturer and the retailer (H2), I use the wholesale price (wholesale_price) the manufacturer charges the retailer as the dependent variable. The related question asked the responding manufacturer division to rate the price the company charges the retailer on a five-point scale ranging from "much lower than the industry average" to "much higher than the industry average."

Independent Variables

Precision and reliability of information the retailer shares with the manufacturer. The survey asked a series of questions about the type of internal accounting information the retailer shares with the manufacturer, the percent of sales volume for which the manufacturer receives this information, and the information system's sophistication (i.e., the use of an EDI system and the amount of data that can be seamlessly transferred via the system). Because many of the question responses are correlated, I use principal components analysis to identify the underlying constructs and the specific questions underlying each construct. The first construct, precision, reflects the fineness of the information the retailer shares with the manufacturer, and corresponds to information precision in the model (J). It comprises four items detailing the extent to which retailers share information with the manufacturer, each measured on a six-point scale. Specifically, the items detail the percent of volume for which the retailer provides the manufacturer information on store inventory levels, warehouse inventory levels, warehouse withdrawals, and point-of-sales data. The second construct, reliability, relates to the manufacturer's information linkages (i.e., quality of the information transfer) and proxies for information reliability (α). It includes seven yes/no responses about the use of an EDI system and the information available through the division's EDI system, including purchase orders, invoices, warehouse inventory data, and production schedules. The more information that flows seamlessly through the system, the more accurate the information that the manufacturer receives and, thus, the more reliable the information.

Consistent with prior literature (Burke 1984), I identify the constructs (described above) via principal components analysis and form indices by adding the scores on the individual components of the constructs.[14] Adding the scores gives each question an equal weight rather than a weight based on the variability in the response.[15] The resulting constructs, precision and reliability, have Cronbach's coefficient alphas of 0.8091 and 0.8335, respectively.

Consumer demand variability. Fisher (1997) argues that consumer demand variability depends on the product life-cycle stage. Early-stage products have unstable and/or unknown consumer demand. Therefore, I use the percentage of products in the introduction and growth stages (life_cycle) to proxy for consumer demand variability.

Control variable. I also control for another variable that may affect a division's use of VMI.[16] Companies adopt VMI if the related gains exceed the costs. Larger companies are more likely to have the resources to afford the high set-up and maintenance costs of adopting VMI in their various divisions. Consequently, the company size, rather than the division size, affects the adoption of large initiatives such as VMI. I use the company sales level (sales) as a proxy for size. The survey asked respondents into which of six groups their companies' sales levels fell. Thus, the companies are grouped categorically rather than continuously on this dimension.

Data Limitations

Similar to other survey research, virtually all the measures I use contain measurement error because respondents used an arbitrary response scale. The respondents rated the levels of information sharing and collaboration on a six-point scale, and their divisions' performance relative to that of the industry on a five-point scale. The answers may be prone to interpretation error. For example, respondents may interpret the phrases "above the industry average" and "significantly above the industry average" differently. As long as these errors are unsystematic, this biases coefficients in the statistical analyses toward zero. Using a phone survey helps alleviate these problems because the interviewer consistently defines terms. To further diminish the measurement error and to simultaneously capture the multidimensional quality of the data, I group related responses into the two constructs identified previously, precision and reliability.

Another limitation is that some respondents may have systematically biased their answers. For example, an upward bias would result if respondents do not reveal that their divisions' performances are lower than the industry average. As long as VMI respondents do not bias their answers in a manner systematically different from those of non-VMI respondents, this should not affect the results.

The empirical analyses test only for associations between information properties and VMI; I do not make any inferences about causality. The hypotheses, as stated, imply that information precision and reliability lead to VMI use. However, companies that have precise and reliable information already may be more inclined to use VMI. Alternatively, adopting VMI may have led the manufacturer to demand more precise and reliable information from the retailer. Without time-series data, one cannot determine the direction of causality between information properties and VMI use.

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[14] Labovitz (1970) shows that the correlation between almost any ordinally scaled measure (such as the scale I derive using equal weights) and the "correct" intervally scaled measure is very high as long as the number of levels in the ordinal scale exceeds 15 or 20, as is the case with the current measures.

[15] I repeated the analysis using principal components and factor analysis scores to form the constructs. The empirical results are qualitatively similar.

[16] The analytical results assume the retailer earns equal profits in the two systems. The survey did not ask about retail partners, their power, or their performance. Thus, my empirical analysis cannot control for retailer profits.

Empirical Model Specification

Hypotheses 1a and 1b predict the extent of VMI use will increase with the precision and reliability of the information the retailer shares and will be ambiguous in consumer demand variability. Because my measure of the extent of VMI use is an ordered, categorical variable, I use the following ordered PROBIT model to analyze the relation between the independent variables and the extent of VMI use. Multinomial LOGIT and standard linear regression result in similar qualitative inferences.

$$\text{Prob}(\text{Extent\_VMI} = i) = F(\alpha + \beta_1\text{precision} + \beta_2\text{reliability} + \beta_3\text{life\_cycle} + \beta_4\text{sales}) \quad (18)$$

where

F(•) = the normal cumulative distribution function;
Extent_VMI = extent of VMI use;
precision = index for precision of information the retailer shares;
reliability = index for reliability of manufacturer's information linkages;
life_cycle = percentage of products in introduction or growth stages (a proxy for consumer demand variability); and
sales = sales of the entire firm.

I test H1a and H1b through the variables precision, reliability, and life_cycle.

Hypothesis 2 predicts that the wholesale price transferred from the retailer to the manufacturer will decrease with the extent of VMI use. The dependent variable, wholesale_price, is ordered and categorical. Therefore, I test H2 using the following ordered PROBIT model:

$$\text{Prob}(\text{wholesale\_price} = i) = F(\alpha + \beta_1\text{Extent\_VMI}) \quad (19)$$

where:

F(•) = the normal cumulative distribution function; and
wholesale_price = wholesale price charged by the manufacturer.

VI. EMPIRICAL RESULTS

Descriptive Statistics

The descriptive statistics for the empirical variables and the related survey questions appear in Table 2. To be included in the test sample, a respondent must answer all of the related questions. Thus, the number of observations in the test sample is lower than the number in the overall sample.[17] The standard deviations indicate reasonable variation among respondents' answers to each survey question. This alleviates the concern that respondents

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[17] Note that only 52 divisions responded to the question about the wholesale price. Consequently, further tests related to H2 will only analyze 52 observations.

TABLE 2

Descriptive Statistics for the Extent of VMI Use, Wholesale Price, Precision and Reliability of Information Shared, Consumer Demand Variability, Control Variable, and Questions that Comprise the Factors (precision and reliability)

Variable[a]	*n*	*Mean*	*Std. Dev.*	*Min.*	*Median*	*Max.*
Extent_VMI	53	2.38	1.61	1	2	6
Wholesale_price	52	2.98	0.75	1	3	4
Precision	53	9.66	5.38	4	8	24
Reliability	53	3.25	2.13	0	4	7
Life_cycle	53	32.26	28.95	0	25	100
Sales	53	3.70	1.46	1	4	6
Questions Measuring Precision of Information Shared Factor[b]						
Sharing Store Inventory Levels	53	2.32	1.37	1	1	6
Sharing Retail Warehouse Inventory Levels	53	2.77	1.86	1	2	6
Sharing Warehouse Withdrawals	53	2.32	1.73	1	1	6
Sharing Point-of-Sale Data	53	2.24	1.51	1	2	6
Questions Measuring Reliability of Information Transmission Factor[c]						
Basic EDI system	53	0.81	0.39	0	1	1
Transmit purchase orders via EDI	53	0.73	0.45	0	1	1
Transmit invoices via EDI	53	0.66	0.48	0	1	1
Transmit advance ship notices via EDI	53	0.43	0.50	0	0	1
Transmit warehouse inventory data via EDI	53	0.17	0.38	0	0	1
Transmit production schedules via EDI	53	0.16	0.36	0	0	1
Transmit transportation information via EDI	53	0.30	0.46	0	0	1

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[a] Variable definitions:

Extent_VMI = the extent that the manufacturing division uses VMI on a six-point scale where 0 = no VMI and 6 = use VMI for more than 91 percent of sales;

Wholesale_price = measure for the price the manufacturer charges the retailer. The related survey question asks, "Would you characterize the prices your company charges the retailer as..." The answer is categorical, ranging from "much lower than the industry average" to "much higher than the industry average";

Precision = index for the precision of the information the retailer shares (J). The index is the sum of scores from a series of questions regarding the extent to which retailers share information with the manufacturer, based on a six-point scale. Higher numbers indicate the retailers share more precise information;

Reliability = index for the reliability of the manufacturer's information linkages (α), The index is the sum of scores from a series of yes/no questions about the information available through the manufacturer's system. Higher numbers indicate more reliable information systems.

Life_cycle = percentage of products in the introduction and growth stages (σ). The related survey question asks, "About what percentage of your products are in the introduction and growth stages?" This variable proxies for variability in consumer demand; and

Sales = company sales, based on the answer to the survey question "Company Sales." The categorical answer uses a six-point scale; higher numbers correspond to higher sales.

[b] These items are measured on a six-point scale where 0 = no information sharing and 6 = share the given information for 91 percent or more of sales.

[c] Items are measured through yes/no questions: 0 = no and 1 = yes.

simply answered that their divisions were "average" on all dimensions. Most of the univariate correlations among the independent variables are not significant at conventional levels (not tabulated). However, reliability is significantly positively correlated with precision and significantly negatively correlated with life_cycle at the 5 percent level.

Average firm-wide sales for the manufacturers in the empirical analyses appear in Table 3. The food industry comprises 68 percent of the test sample, with average (median) company sales of \$760 (\$500) million. The sample also includes companies specializing in beverage, health and beauty aids, and household products. Annual sales range from less than \$100 million to more than \$10 billion, with a mean (median) of \$849 (\$999) million for the sample manufacturers.[18] Untabulated results also show that 49 percent of the respondents use VMI, and they have been using the system for an average of three to four years.

Relation between VMI Use and Information Precision, Information Reliability, and Consumer Demand Variability

The ordered PROBIT model results of tests of H1a and H1b for 53 manufacturer divisions appear in Table 4. Positive coefficients mean that higher values of the independent variable are associated with more extensive VMI use.[19] Consistent with the hypothesis that manufacturers use VMI to a greater extent when the retailer shares more precise and more reliable information (H1a), information precision ($p = 0.001$) and information reliability ($p = 0.06$) are significantly positively related to the extent of VMI use.[20] By contrast, the proxy for consumer demand variability, life_cycle, is not significant at conventional levels ($p = 0.21$). Thus, the empirical analysis does not support H1b. Company size (proxied by sales) is associated with more extensive VMI use ($p = 0.02$).[21] In sum, the extent of VMI use increases with information precision, information reliability, and company size.[22]

Effect of VMI Use on Wholesale Price

Table 5 presents the results for the ordered PROBIT of wholesale price on the extent of VMI use. The analysis reveals that the extent of VMI use is marginally associated with the wholesale price transferred between the retailer and the manufacturer ($p = 0.097$). Manufacturer divisions that use VMI to a greater extent charge their retailer partners a lower wholesale price. (Controlling for the independent variables associated with the extent of VMI use leads to identical inferences, and the control variables are not statistically significant at conventional levels.)

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[18] The composition of the test sample is similar to that of the entire sample of 100 respondents.

[19] One should interpret the coefficients carefully. If a given variable increases the VMI use "rating," then the probability of extensive VMI use increases and the probability of no VMI use decreases. However, the probability of being in the intermediate categories could move in either direction (Greene 1993, 672–676).

[20] One can interpret *reliability* as the minimal technology that the manufacturer possesses. However, VMI also may depend on the manufacturer's ability to interpret the information. Unfortunately, the survey does not measure this variable. The study's inferences should not be affected by this omission unless the manufacturer's ability to interpret the data is correlated with the included independent variables.

[21] Lee et al. (1997) find that the demand variability that the manufacturer faces from its retailer partners differs from the underlying consumer demand variability. High variability of *retailers'* demand often leads to supply-chain management collaboration, such as VMI. I also run the empirical analyses with a variable that proxies for retailer demand. The related question asked respondents to rank the demand variability they experience with their retailer partners on a three-point scale. The inferences do not change when this variable is included in the analysis.

[22] I also test the hypothesis using an alternative dependent variable where 1 = VMI adopters, and 0 = nonadopters. The results are qualitatively similar except that the information precision variable only marginally affects the probability of using VMI ($p = 0.12$).

TABLE 3
Sales Levels for the Sample Manufacturer Firms (n = 53)

Sector	*Number*	*Mean Sales (in millions)*	*Median Sales (in millions)*
Food	36	$760	$500
Beverage	12	$4,780	$10,000
Health and Beauty Aids	2	$999	$999
Household Products	3	$200	$200

VII. CONCLUSION AND FUTURE RESEARCH

Supply-chain management requires partners to share information, typically including internal accounting sales and inventory information. However, little evidence exists on the effect of information sharing on supply-chain management contracts and the resulting firm performance. This study models the effects of information structure and demand variability on two alternative inventory management systems that differ in the delegation of decision rights: (1) a traditional system where the retailer makes the inventory decision and (2) a vendor managed inventory (VMI) system where the retailer delegates the inventory decision to the manufacturer. In selecting between the traditional and VMI systems, the manufacturer weighs the use of potentially unreliable information to determine quantity based on total supply-chain costs against allowing the retailer to use reliable information to make a strategic quantity decision based only on her own cost structure.

The model suggests that the extent of VMI use increases with the amount of internal accounting information the retailer reveals to the manufacturer (i.e., precision) and the manufacturer's ability to capture this information (i.e., reliability). The model also suggests that the extent of VMI use leads to lower wholesale prices because the manufacturer must share some of the efficiency gains that result from VMI with the retailer.

I test these theoretical predictions using survey data from manufacturer divisions in the food and consumer packaged goods industry. The results support the first theoretical prediction that the extent of VMI use increases with precise and reliable flows of internal accounting information from the retailer to the manufacturer. I also find that VMI use increases with the size of the manufacturer. The evidence suggests that the extent of VMI use is only marginally associated with a lower wholesale price.

This study shows that the precision and reliability of internal accounting information, and the retailer's willingness and ability to share this information, should affect the way manufacturers and retailers structure their relationship, and the inventory management decision rights in particular. The results suggest that VMI is more likely to lead to higher supply-chain profits if both companies commit to sharing precise internal accounting information and reliably transmitting, receiving, and using this information for inventory decisions.

APPENDIX
PROOFS

Proof of Proposition 1

The solution to the traditional system is found by solving the following problem with $J = 2$:

TABLE 4

Results of Ordered PROBIT Analysis of the Extent of VMI Use on the Precision and Reliability of Information, Consumer Demand Variability, and Control Variable for 53 Manufacturer Divisions

$$Prob(Extent\_VMI = i) = F(\alpha + \beta_1 precision + \beta_2 reliability + \beta_3 life\_cycle + \beta_4 sales)$$

Variable	*Predicted Sign*	*Coefficient*	*Std. Error*	*p-value*[a]
Intercept		3.11	1.01	0.002***
Precision	+	0.11	0.04	0.001***
Reliability	+	0.13	0.09	0.063*
Life_cycle	?	−0.01	0.01	0.205
Sales	+	0.22	0.11	0.025**

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*, **, *** Indicate significance at the 10, 5, and 1 percent levels or better, respectively.

[a] The p-value for the variables are one-tailed probabilities, except for the intercept term and life-cycle, which are two-tailed probabilities.

$$\max_{w, Q_m(), q_r(j)} \sum_{j=1}^{J} \frac{1}{J} (wq_r(j) - cQ_m()) \tag{20}$$

subject to:

$$\sum_{j=1}^{J} \frac{1}{J} \left(\begin{array}{l} \displaystyle\int_{\mu+\sigma(2j-J-2)/2J}^{q_r(j)} (pD - wq_r(j) - h(q_r(j) - D)) \left(\frac{J}{\sigma}\right) dD \\ \displaystyle\quad + \int_{q_r(j)}^{\mu+\sigma(2j-J)/2J} ((p - w)q_r(j) - s(D - q_r(j))) \left(\frac{J}{\sigma}\right) dD \end{array} \right) \geq 0 \tag{21}$$

$$q_r(j) \text{ maximizes } \left(\begin{array}{l} \displaystyle\int_{\mu+\sigma(2j-J-2)/2J}^{q_r(j)} (pD - wq_r(j) - h(q_r(j) - D)) \left(\frac{J}{\sigma}\right) dD \\ \displaystyle\quad + \int_{q_r(j)}^{\mu+\sigma(2j-J)/2J} ((p - w)q_r(j) - s(D - q_r(j))) \left(\frac{J}{\sigma}\right) dD \end{array} \right) \tag{22}$$

The incentive compatibility constraint (22) is solved to determine the quantity ordered given each of the two signals. Taking the derivative of the constraint with respect to $q_r(j)$, setting it equal to zero, and solving for $q_r(j)$ gives:

$$q_r(1) = \mu - \frac{\sigma(w + h)}{2(p + h + s)} \tag{23}$$

$$q_r(2) = \mu + \frac{\sigma(p - w + s)}{2(p + h + s)}. \tag{24}$$

When the participation constraint (21) is not binding, the derivative of Equation (20) with respect to $Q_m()$ gives:

TABLE 5
Results of Ordered PROBIT Analysis of the Wholesale Price on the Extent of VMI Use for 52 Manufacturer Divisions

$$Prob(wholesale\_price = i) = F(\alpha + \beta_1 Extent\_VMI)$$

Variable	*Predicted Sign*	*Coefficient*	*Std. Error*	*p-value*[a]
Intercept		2.47	1.23	0.043**
Extent_VMI	–	−0.13	0.10	0.097*

*, ** Indicate significance at the 10 and 5 percent levels or better, respectively.
[a] The p-value for the Extent_VMI variable is a one-tailed probability.

$$-\frac{1}{2}c. \tag{25}$$

The above expression implies that the manufacturer will produce at the lower bound, $q_r(1)$.

If Equation (25) is negative, then the manufacturer will produce $q_r(1)$; if positive, then $q_r(2)$.

I substitute the solutions to the order and production quantities into the original optimization problem. Taking the derivative with respect to w and setting the equation equal to 0, one can solve for w. The retailer's participation constraint may or may not be binding.

When the constraint is not binding, the first-order condition on w is:

$$\frac{\sigma(p+2c - 4w - h + s) + 4\mu(p + h + s)}{4(p + h + s)} = 0. \tag{26}$$

Solving the first-order condition for the wholesale price gives:

$$w = \frac{\sigma(p + 2c - h + s) + 4\mu(p + h + s)}{4\sigma}. \tag{27}$$

Substituting the wholesale price into the quantity decisions gives:

$$q_r(1) = \frac{\mu}{2} - \frac{\sigma(p + 2c + 3h + s)}{8(p + h + s)} \tag{28}$$

$$q_r(2) = \frac{\mu}{2} + \frac{\sigma(3p - 2c + h + 3s)}{8(p + h + s)}. \tag{29}$$

Substituting the wholesale price and the quantity decisions into the expected manufacturer and retailer profits (Equations (20) and (21), respectively) gives:

$$\Pi_m^{Trad} = \frac{8\sigma\mu((p+s)(p+s-2c)-h(h+2c))+16\mu^2(p+h+s)^2 + \sigma^2((p-2c-h+s)^2)}{32\sigma(p+h+s)} \tag{30}$$

$$\Pi_r^{Trad} = \frac{3(p+h+s)^2(4\mu-\sigma)^2-16\sigma\mu(p+h+s)(p-c) + 4\sigma^2(p-c+s)(c+h)}{32\sigma(p+h+s)} \equiv \Pi_r^T. \tag{31}$$

When the participation constraint is binding, the manufacturer sets the constraint equal to 0 and solves for the wholesale price:

$$w = -\frac{h(\sigma-4\mu)-(p+s)(\sigma+4\mu) + \sqrt{(\sigma-4\mu)^2(p+h)^2+2(\sigma^2+16\mu^2)(p+h)s+(\sigma+4\mu)^2s^2}}{2\sigma}. \tag{32}$$

Substituting the wholesale price into the order quantity formulas ((23) and (24)) and the objective function (20) gives the following:

$$q_r(1) = -\frac{\sigma}{4} + \frac{\sqrt{(p+h)^2(\sigma-4\mu)^2+s^2(\sigma+4\mu)^2+2(\sigma^2+16\mu^2)(p+h)s}}{4(p+h+s)} \tag{33}$$

$$q_r(2) = \frac{\sigma}{4} + \frac{\sqrt{(p+h)^2(\sigma-4\mu)^2+s^2(\sigma+4\mu)^2+2(\sigma^2+16\mu^2)(p+h)s}}{4(p+h+s)} \tag{34}$$

$$\Pi_m^{Trad} = \frac{-(p+h+s)((\sigma-4\mu)^2(p+h)^2+s^2(\sigma+4\mu)^2) + (\sigma(p-2c-h+s)+4\mu(p+h+s)) \times \sqrt{(p+h)^2(\sigma-4\mu)^2+s^2(\sigma+4\mu)^2+2(\sigma^2+16\mu^2)(p+h)s}}{8\sigma(p+h+s)}. \tag{35}$$

Q.E.D.

Proof of Proposition 2

In the VMI system, the manufacturer solves the following problem:

$$\max_{w,q_m(j),Q_m} \alpha\left(\sum_{j=1}^{J}\frac{1}{J}((w-c)q_m(j))\right) + (1-\alpha)((w-c)q_m|j=\phi) \tag{36}$$

subject to:

$$\alpha\left[\sum_{j=1}^{J}\frac{1}{J}\left(\begin{array}{l}-wq_m(j)+\int_{\mu+\sigma(2j-J-2)/2J}^{q_m(j)}(pD-h(q_m(j)-D))\\ \times\left(\frac{J}{\sigma}\right)dD+\int_{q_m(j)}^{\mu+\sigma(2j-J)/2J}(pq_m(j)-s(D-q_m(j)))\\ \times\left(\frac{J}{\sigma}\right)dD\end{array}\right)\right]$$

$$+(1-\alpha)\left[\begin{array}{l}-wq_m()+\int_{\mu-\sigma/2}^{q_m()}(pD-h(q_m()-D))\\ \times\left(\frac{J}{\sigma}\right)dD+\int_{q_m()}^{\mu+\sigma/2}(pq_m()-s(D-q_m()))\\ \times\left(\frac{J}{\sigma}\right)dD\end{array}\right]$$

$$\geq \Pi_r^T. \tag{37}$$

The manufacturer adjusts the wholesale price to make Equation (37) binding. After solving Equation (37) for w, I place the solution back into the objective function. I then take the derivative of the objective function with respect to $q_m(1)$, $q_m(2)$, and $q_m()$. I set the derivative equal to 0 and solve for the variables. The solution to the entire problem is as follows:

$$q_m(1) = \mu - \frac{\sigma(c + h)}{2(p + h + s)} \tag{38}$$

$$q_m(2) = \mu + \frac{\sigma(p - c + s)}{2(p + h + s)} \tag{39}$$

$$q_m() = \mu + \frac{\sigma(p - 2c - h + s)}{2(p + h + s)} \tag{40}$$

$$w = \frac{4(\mu p - \Pi_r^T)(p + h + s) + \sigma(\alpha - 2)(c^2 + h(p + s))}{\sigma(\alpha - 2)(2c - p + h - s) + 4\mu(p + h + s)} \tag{41}$$

$$\Pi_m^{VMI} = -\Pi_r^T + \mu(p - c) + \frac{\sigma(\alpha - 2)(p - c + s)(c + h)}{4(p + h + s)} \tag{42}$$

$$\Pi_r^{VMI} = \Pi_r^T. \tag{43}$$

Q.E.D.

Proof of Proposition 3

Comparing the quantities selected in the traditional and VMI models, the difference is:

$$q_m^{VMI}(1) - q_r^{Trad}(1) = q_m^{VMI}(2) - q_r^{Trad}(2) = \frac{\sigma(p - 2c - h + s) + 4\mu(p + h + s)}{8(p + h + s)}. \tag{44}$$

In the VMI system, $q_m() = (\mu + \sigma(p - 2c + s - h))/2(p + h + s) = (2\mu(p + h + s)$

$+ \sigma(p - 2c + s - h))/2(p + h + s)$. The denominator is always positive. Assuming that the quantity ordered must be nonnegative, the numerator must be positive also; but $2\mu(p + h + s) + \sigma(p - 2c + s - h) < \sigma(p - 2c - h + s) + 4\mu(p + h + s)$. Therefore, the numerator in Equation (44) is positive. The quantity produced and ordered in the VMI system exceeds that ordered in the traditional system.

The difference between the wholesale price in the VMI and traditional systems is:

$$w^{VMI} - w^{Trad} = \frac{(p + h + s)((p + h + s)((\alpha - 2)\sigma^2 - 16\mu^2) - 4\sigma\,\Pi_r^T + 4\sigma\mu(2c(1 - \alpha) + (3 - \alpha)(h - s)))}{4\sigma(\sigma(2 - \alpha)(p - 2c - h + s) + 4\mu(p + h + s))}. \tag{45}$$

In simplifying and expanding the numerator, all but two terms are negative. These terms are less than the other terms in absolute value, making the numerator negative. Again:

$$2\mu(p + h + s) + \sigma(p - 2c + s - h) \geq 0 \Rightarrow 4\mu(p + h + s) + 2\sigma(p - 2c + s - h) \geq 0.$$

Since $2 - m \leq 2$ and $4\mu(p + h + s) > 0$, the denominator is positive and the difference is negative. The wholesale price charged in a VMI system is less than that charged in a traditional system.

Q.E.D.

Proof of Proposition 4

Let:

$$\Delta\Pi = \Pi_n^{VMI} - \Pi_m^{Trad}. \tag{46}$$

Taking the derivative of Equation (46) with respect to information reliability, α, gives the following:

$$\frac{\partial\Delta\Pi}{\partial\alpha} = \frac{\sigma(c + h)(p - c + s)}{4(p + h + s)} > 0. \tag{47}$$

Turning now to the effect of consumer demand variability, σ, I use proof by construction to show that the relationship between the change in the difference in expected profits between the two systems is ambiguous in σ. Suppose that $p = 10$, $c = 5$, $h = 1$, $s = 1$, $\mu = 50$, $\alpha = 0.5$. When $\sigma = 5,10,15$, the values for Π_m^{VMI}, Π_m^{Trad}, and $\Delta\Pi$ are as follows:

	$\sigma = 5$	$\sigma = 10$	$\sigma = 15$
Π_m^{VMI}	244.37	238.75	233.13
Π_m^{Trad}	243.67	237.39	231.16
$\Delta\Pi$	0.70	1.36	1.97

For the above example, $\partial\Delta\Pi/\partial\sigma > 0$.

Now suppose that $p = 100$, $c = 5$, $h = 1$, $s = 1$, $\mu = 50$, $\alpha = 0.5$. When $\sigma = 45,50,55$, the values for Π_m^{VMI}, Π_m^{Trad}, and $\Delta\Pi$ are as follows:

	$\sigma = 45$	$\sigma = 50$	$\sigma = 55$
Π_m^{VMI}	6073.24	5663.33	5330.68
Π_m^{Trad}	4070.00	3799.08	3579.67
$\Delta\Pi$	2003.24	1864.25	1751.01

For the above example, $\partial\Delta\Pi/\partial\sigma < 0$.

Based on the two examples above, $\partial\Delta\Pi/\partial\sigma$ is ambiguous.

Finally, I show that $\Delta\Pi$ is increasing in information precision. That is, $\Delta\Pi$ is higher when $J = 3$ than when $J = 2$.

Solving the above models when $J = 3$ and comparing $\Delta\Pi$ between $J = 2$ and $J = 3$ gives the following:

$$\Delta\Pi|J = 3 - \Delta\Pi|J = 2$$

$$= \frac{1}{576\sigma}\left(\begin{array}{l} -4c\sigma((15 + 148\alpha)\sigma + 36\mu) \\ + p((27 + 2272\alpha)\sigma^2 - 216\mu\sigma + 1152\mu^2) \\ + 3((-11 + 560\alpha)\sigma^2 - 120\mu\sigma + 384\mu^2)h \\ - ((-27 + 1184\alpha)\sigma^2 - 360\mu\sigma - 1152\mu^2)s \\ \\ -\dfrac{1}{p + h + s}\left(4\sigma^2\left(\begin{array}{l} c^2(-15 + 76\alpha) - 288cp\alpha + 684\alpha p^2 \\ + h\left(\begin{array}{l} -2c(15 + 68\alpha) + 1008\alpha p \\ + (-15 + 436\alpha)h \end{array}\right) \end{array}\right)\right) \end{array}\right). \tag{48}$$

Because $1/576\sigma > 0$, the sign of the above equation depends on the sign of the numerator. Rearranging the terms in the numerator gives the following:

$$\begin{aligned} &\sigma^2(1 - \alpha)(60c^2 + 27p^2 + 48ch + 27h^2 + 27s^2) + \sigma s(p - c)(144\mu + 54\sigma + 592\mu\sigma) \\ &+ \mu(p + h)(165.6p\mu - 144c\sigma) + p^2\mu(248.4\mu - 216\sigma) + ph\mu(662.4\mu - 576\sigma) \\ &+ \mu h^2(414\mu - 360\sigma) + s\sigma(360\mu - 313\alpha\sigma) + hs(7.92\mu^2 - 6\sigma^2) + s^2(1114\mu^2 \\ &- 844\alpha\sigma^2) + h^2(48.84\mu^2 - 37\alpha\sigma^2) + ph(7.92\mu^2 - 6\sigma^2) + ph(105.6\mu^2 - 80\alpha\sigma^2) \\ &+ s(7.92\mu^2 - 6\sigma^2) + p^2(386.76\mu^2 - 293\alpha\sigma^2) + p(79.2p\mu^2 - 60c\sigma^2) + \alpha c\sigma^2(560p \\ &- 244c) + \mu^2(272.04p^2 + 1362.4ph + 689.16h^2 + 2296.08ps + 2296hs + 38s^2) \\ &+ \sigma^2(12ch + 496\alpha ps + 496\alpha hs). \end{aligned}$$

Based on the following assumptions, it can be shown that the above expression is nonnegative and, therefore, that the difference is higher when $J = 3$ than when $J = 2$.

Assumptions

$p > c, 0 \le \alpha \le 1$, and that demand must be positive: $E[D] \ge 3 \cdot SD[D]$ where $SD[D]$ is the standard deviation of the consumer demand distribution. The retail price must exceed the unit production cost or the product would not be produced and sold. The probability that the manufacturer receives the information, α, must be between 0 and 1 by definition. Finally, it seems intuitive that consumer demand is positive. Therefore, I assume that three standard deviations below expected demand is also positive; the probability of negative demand given this assumption is extremely low.

Q.E.D.

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Compensation to Managers of Charitable Organizations: An Empirical Study of the Role of Accounting Measures of Program Activities

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ABSTRACT: We investigate how unique features of charities affect the manner in which they compensate their executives. We find that changes in compensation are significantly positively associated with changes in spending on programs that advance organization objectives, whether changes in program spending are attributable to changes in revenue raised or to changes in the relative costs of administering the charity. The results suggest that accounting performance measures can play a role in nonprofit organizations whose objectives are typically subjective and nonfinancial, and thus, whose progress toward objectives is difficult to quantify.

Keywords: *charity; nonprofit; compensation.*

Data Availability: *Data are publicly available.*

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I. INTRODUCTION

This paper investigates empirical associations between accounting-based performance measures and compensation paid to chief executive officers (CEOs) of charitable organizations. We consider three research questions. The first is whether changes in executive compensation are positively associated with changes in *program spending,* which is spending on activities that advance the charitable mission. The second question is whether associations between compensation changes and program spending changes are attributable to changes in total revenue received or to changes in the *program spending ratio*, which is the average proportion of revenue spent on program activities. The third question is whether the relation between changes in compensation and changes in program spending ratios is higher for charities where the program spending ratio is most likely to be a concern. In particular, we focus on charities with low program spending ratios, which oversight organizations or potential donors are likely to evaluate unfavorably, and on charities monitored by well-known oversight agencies.

Unique characteristics of charitable organizations suggest that prior evidence from the for-profit sector about the relation between executive compensation and accounting-based performance measures may not apply to charities. Donors to charities are typically not beneficiaries, and therefore, donors have less incentive to monitor the disposition of resources than do investors in private enterprise (Fama and Jensen 1983a, 1983b). Market-based performance measures, which discipline managers in for-profit firms, are absent since capital is donated and services or products are not "priced" in the conventional sense (Williamson 1983). These features of charities expose them to malfeasance by management, which may be attenuated by using accounting measures to ensure stewardship of contributed resources (Gjesdal 1981; Smith and Watts 1992; Sloan 1993).

On the other hand, philanthropic objectives are often subjective and nonfinancial, and returns to both donors and managers are more likely to be nonpecuniary, even altruistic, than returns to participants in other kinds of organizations (Andreoni 1989; Rose-Ackerman 1982; 1996; Handy and Katz 1998). In addition, financial statements do not reveal whether managers direct program expenditures toward meaningful projects, or whether spending is efficient or effective with respect to specific objectives. Moreover, charities' objectives are not clear-cut and their outputs are difficult to measure. Using accounting information for setting compensation is dysfunctional when contracting parties focus on the subset of tasks where performance can be measured objectively and de-emphasize other tasks where evaluations are subjective (Holmstrom and Milgrom 1991).

The potential for malfeasance suggests charities need external monitoring to reduce the agency costs of obtaining and distributing contributions (Akerlof 1970; Stigler 1971; Olsen 1971; Williamson 1983). Federal law requires charities to file financial information annually with the U.S. Internal Revenue Service (IRS). In addition, many charities voluntarily submit financial statements to private oversight agencies that report whether organizations comply with standards. These agencies, along with the popular press and individual donors, frequently cite the ratio of program expense to revenue (or the ratio of program expense to total expense) as a performance indicator for charities (e.g., Gold 1993; Hardman 1993;

Schuman 1993; Smith 1993).[1] Evidence in Weisbrod and Dominguez (1986), Harvey and McCrohan (1988), and Khumawala and Gordon (1997) indicates that donation levels increase with program spending ratios. To investigate the relative importance of the program spending ratio, we distinguish changes in program spending that are attributable to changes in program spending ratios from those attributable to changes in the level of revenue that the charity raises. If charities benefit more from improving the program spending ratio than from increasing the level of revenue, then we expect compensation to be more closely associated with changes in spending ratios.

We use financial data from IRS Form 990 to obtain a sample of 331 charities. We find that changes in CEO compensation vary directly with both current- and prior-year changes in program spending, whether those changes are attributable to program spending ratios or to revenue raised. Moreover, we cannot convincingly reject the null hypothesis that charities reward increases in program spending that result from higher program spending ratios equally to those resulting from raising more revenue.

Next we focus on charities that are most likely to be concerned about program spending ratios reported to potential donors. We distinguish charities with relatively low program spending ratios and charities monitored by large, nationally recognized oversight agencies. Again, we cannot persuasively reject the null hypothesis that charities in these subsamples weigh changes in the program spending ratio equally with changes in revenue raised.

Evidence that compensation paid to executives of charitable organizations is associated with accounting measures parallels evidence documented for commercial firms (Rosen 1992). Specifically, we find that charities reward executives for increasing resources allocated to the charitable objective. We find no evidence that the use of program spending ratios by outsiders distorts the charities' incentives.

Section II develops empirical specifications and hypotheses. Sample selection procedures appear in Section III, and results are reported in Section IV. Concluding remarks are in Section V.

II. EMPIRICAL SPECIFICATIONS AND HYPOTHESES

We characterize *charities* as organizations that broker philanthropic resources from donors to beneficiaries. The brokering process consumes some fraction of the contributed capital, but the objective is to maximize spending on program activities. This characterization implies that management (1) raises funds, and (2) distributes the net of revenues less the costs of brokering contributions to program activities.

We consider measures of managers' performance on these two dimensions. Let period t program spending $PSPENDING_t = REVENUE_t \times RATIO_t$, where $REVENUE_t$ is period t total revenue and $RATIO_t$ is the period t ratio of program expense to total revenue. Period t change in program spending is:

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[1] At the time when the research was conducted, The National Charities Information Bureau (NCIB) and the Philanthropic Advisory Service of the Council of Better Business Bureaus (PAS) were the two largest private oversight agencies (the NCIB and the PAS combined into one agency during 2001). NCIB standards are (1) at least 60 percent of annual expenditures be allocated to program services; (2) fundraising expenses be reasonable over time; and (3) net assets available for use are not more than twice the greater of next year's expenses or next year's budget (NCIB 1996). PAS standards are that (1) at least 50 percent of total revenue and at least 50 percent of public contributions be spent on program services; (2) fundraising expenditures are less than 35 percent of related contributions; and (3) combined fundraising and administrative costs are less than 50 percent of total revenue (CBBB-PAS 1982). Both the NCIB and the PAS use nonfinancial as well as accounting-based performance standards.

$$\begin{aligned}\Delta\text{PSPENDING}_t &= [\text{RATIO}_t \times \text{REVENUE}_t] - [\text{RATIO}_{t-1} \times \text{REVENUE}_{t-1}] \\ &= [\text{RATIO}_{t-1} \times \Delta\text{REVENUE}_t] + [\Delta\text{RATIO}_t \times \text{REVENUE}_t]. \quad (1)\end{aligned}$$

We interpret $[\text{RATIO}_{t-1} \times \Delta\text{REVENUE}_t]$ in Equation (1) as the change in period t program spending that is explained by the change in the level of funds the charity raises. We interpret the product $[\Delta\text{RATIO}_t \times \text{REVENUE}_t]$ as the change in period t program spending that is explained by the change in the average fraction of each revenue dollar the charity spends on program activities.[2]

To control for scale effects, we deflate Equation (1) by period $t - 1$ program spending. Thus, the empirical measures indicate percent changes, not absolute changes, in program spending. Deflating the first term in Equation (2) by PSPENDING_{t-1} and then substituting $\text{RATIO}_{t-1} = \text{PSPENDING}_{t-1}/\text{REVENUE}_{t-1}$ yields $[\Delta\text{REVENUE}_t/\text{REVENUE}_{t-1}] = \%\Delta\text{REVENUE}_t$. That is, the period t percent change in program spending attributable to changes in available funds is simply the period t percent change in revenue. Deflating the second term in Equation (1) gives $\Delta\text{YIELD}_t = [\Delta\text{RATIO}_t \times \text{REVENUE}_t]/\text{PSPENDING}_{t-1}$, which indicates the extent to which the period t percent change in program spending is attributable to changes in the program spending ratio.

To summarize, we decompose the percent change in program spending as:

$$\Delta\text{PSPENDING}_t/\text{PSPENDING}_{t-1} = \%\Delta\text{REVENUE}_t + \Delta\text{YIELD}_t, \quad (2)$$

where the percent change in revenue ($\%\Delta\text{REVENUE}_t$) indicates changes in fundraising performance, and ΔYIELD indicates changes in the average cost of brokering contributed capital from contributors to beneficiaries.[3]

Next we posit that executive compensation, either explicitly or implicitly, depends in part on changes in program spending:

$$\%\Delta\text{COMP}_t = \beta_0 + \beta_1\ \%\Delta\text{REVENUE}_t + \beta_2\ \Delta\text{YIELD}_t + \varepsilon_t, \quad (3)$$

where $\%\Delta\text{COMP}_t$ is the period $t - 1$ to period t percent change in compensation paid to the CEO.[4]

We expect compensation changes to be positively associated with changes in program spending, whether those changes are attributable to changes in revenue raised (%ΔREVENUE) or to changes in the program spending ratio (ΔYIELD). Thus, we anticipate both $\beta_1 > 0$ and $\beta_2 > 0$. In addition, widespread use of the program spending ratio by outsiders suggests that charities may reward increases in the program spending ratio (ΔYIELD) more than they reward increases in revenue (%ΔREVENUE). Thus, we predict $\beta_1 < \beta_2$.

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[2] This decomposition of program spending changes is analogous to the decomposition of profits into volume and margin effects described in managerial accounting texts. Following convention, we consider the joint effect of changes in the program spending ratio and changes in revenue (ΔRATIO × ΔREVENUE) as part of the ratio effect (e.g., Hilton 1997, 552–553).

[3] The ratio of program expense to revenue varies inversely with the "price" effect, which is the focus of prior studies of the relation between program spending ratios and fundraising success (Weisbrod and Dominguez 1986; Posnett and Sandler 1989; Callen 1994; Tinkelman 1999).

[4] We use compensation changes, rather than levels, to control for cross-sectional differences in factors (e.g., executive age, experience, gender, tenure with the organization, organization size) that potentially affect CEO compensation. Although CEOs of large organizations tend to earn more than CEOs of small organizations (Werner and Gemeinhardt 1995; Oster 1998), annual changes in compensation are unlikely to be attributable to changes in the organization size from one year to the next. That is, annual changes in size are not likely to warrant increasing (or decreasing) salaries to continuing CEOs. We omit observations in which the CEO changes during the period, so cross-sectional differences unique to the individual (such as age or gender) are unlikely to affect compensation changes.

Next we investigate cross-sectional differences in sensitivity to changes in the program spending ratio. Charities are more likely to be concerned about the cost of brokering contributions when the program spending ratio is relatively low than when the ratio is high. We therefore partition the sample according to whether the ratio is above or below the median value. We expect charities' compensation to be relatively more sensitive to ΔYIELD (and relatively less sensitive to %ΔREVENUE) when the program spending ratio is below the median value.

We also consider whether monitoring by oversight agencies—in contrast with monitoring by donors in general—affects CEO compensation. To do so, we partition the sample according to whether the NCIB or the PAS monitor the charity. If concern about oversight groups' use of the program spending ratio influences compensation, then we expect compensation to be relatively more sensitive to ΔYIELD for charities that are monitored, than for charities that are not.[5]

Finally, if charities reward current-period performance by providing salary increases, rather than one-time bonuses, then current compensation changes are correlated with prior (not current) performance. We therefore estimate specifications that include both current and lagged performance measures:

$$\begin{aligned}\%\Delta\mathrm{COMP}_t = {} & \gamma_0 + \gamma_1\%\Delta\mathrm{REVENUE}_t + \gamma_2\%\Delta\mathrm{REVENUE}_{t-1} \\ & + \gamma_3\Delta\mathrm{YIELD}_t + \gamma_4\Delta\mathrm{YIELD}_{t-1} + \varepsilon_t, \qquad (4)\end{aligned}$$

The foregoing reasoning predicts $\gamma_j > 0$, $j = \{1,\ldots,4\}$, and $(\gamma_1 + \gamma_2) < (\gamma_2 + \gamma_4)$.

III. SAMPLE AND DATA

Table 1 provides the sample selection procedures. We identify 458 charities evaluated by the NCIB or the PAS during 1996 and 1997. We then randomly select 200 additional charities monitored by neither oversight agency from files maintained at the Charitable Organizations Division, Office of the Maryland Secretary of State. Data for 541 charities are from IRS Form 990 filings obtained primarily from the charities directly, although some filings are from the Maryland Secretary of State. We exclude 210 charities with missing or unusable information, as detailed in Table 1. We use compensation changes rather than levels, and therefore, we lose the first year of data for each organization. We also eliminate charities whose chief executives change in the current or the prior year (thus, CEO changes do not explain compensation changes). Finally, we eliminate potentially influential observations specified as those with absolute standardized residuals greater than 2.0 from a regression of year t percent change in compensation on year t percent change in program spending.

Panel B of Table 1 shows that these sample selection criteria yield 664 observations from 331 charities for the primary analysis. Panel B also shows that 484 observations are

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[5] We obtain similar results: when we partition the sample into thirds, quartiles, or quintiles based on the program spending ratio; when we simultaneously classify the observations according to both the prevailing program spending ratio and whether the organization is monitored; when we use alternative financial performance indicators, such as fundraising as a percent of contributions, contributions as a percent of total revenue, or program spending as a percentage of total spending; and when we compute the program spending ratio as a percent of total expense, rather than total revenue.

TABLE 1
Selection of a Sample of Charities with Available Compensation and Financial Data

Panel A: Procedures Used to Identify Sample Organizations

Total requests[a]	658
Organizations not required to file 990 information[b]	(57)
No response[c]	(60)
Information received or obtained	541
Form 990 information unusable[d]	(57)
Chief executive changes during the sample period[e]	(69)
Officer compensation is not disclosed[f]	(84)
Total organizations in the sample	331

Panel B: Profiles of Charity-Year Observations

Years of Data Available	*Total*		*Monitored*[a]		*Not Monitored*[a]	
	Number of Charities	*Number of Observations*	*Number of Charities*	*Number of Observations*	*Number of Charities*	*Number of Observations*
One	77	77	57	57	20	20
Two	190	380	116	232	74	148
Three	49	147	45	135	4	12
Four	15	60	15	60	0	0
Total	331	664	233	484	98	180

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[a] All organizations listed in the February 1996 and June 1997 editions of the National Charities Information Bureau's (NCIB) Charities Index and the 1996 edition of the Annual Charity Index (CBBB-PAS 1996), plus 200 randomly selected organizations not in these publications, but registered in 1996 with the Charitable Organization Division, Office of the Maryland Secretary of State. Organizations from the indices are designated "Monitored"; organizations from the Maryland Secretary of State are designated "Not Monitored."

[b] Includes subsidiaries of other organizations, and organizations not required to file Form 990 (churches or organizations with total receipts less than $25,000).

[c] Organizations (1) that fail to respond to two mail requests; (2) for which requests are returned with no forwarding address; (3) that respond either by stating that they do not provide Form 990s or by omitting or obscuring compensation information; and (4) for which usable information is not available at the Maryland Office of the Secretary of State.

[d] Includes organizations (1) for which Form 990 contains obvious errors; (2) that do not report either program, fundraising, or administrative expenses; (3) for which consecutive fiscal years are not available; and (4) eliminated in tests for influential observations.

[e] Includes organizations for which the chief executive moves from part-time to full-time.

[f] Either Form 990 filing does not include compensation information, or no employee receives more than $50,000.

from 233 charities monitored by the PAS and/or the NCIB, and that 180 are from 98 charities that are not monitored.

Table 2 displays descriptive information about the sample organizations. Entries are means of mean values for each organization, computed using sums for all years that the organization appears in the sample. The first entry is for all organizations, the second entry is for 233 organizations monitored by the NCIB or the PAS, and the third entry is for the

TABLE 2
Descriptive Statistics for CEO Compensation, Revenue, and Program Spending Ratios in Charitable Organizations[a]

(entries are means of mean values computed for each organization using all years the organization is represented in the sample)

Panel A: Total CEO Compensation (cash payments and benefits)

	Mean	*First Quartile*	*Median*	*Third Quartile*	*Standard Deviation*
Total CEO compensation (in $1,000)					
Total sample	130.77*	75.69	113.06	176.62	77.48
Monitored charities	140.66	85.42	127.50	186.96	74.64
Charities not monitored	107.24	56.31	83.74	126.69	79.40
Total CEO compensation as a percent of revenue					
Total sample	3.48*	0.07	1.79	4.03	4.90
Monitored charities	2.57	0.05	1.24	2.96	4.01
Charities not monitored	5.64	1.74	3.24	7.81	5.94
Change in total CEO compensation (in $1,000)					
Total sample	7.14	1.23	5.35	10.84	19.01
Monitored charities	6.80	1.71	6.68	12.63	18.86
Charities not monitored	7.96	1.05	4.15	8.84	19.40
%ΔCCOMP percent change in total CEO compensation					
Total sample	6.84	0.02	5.65	10.51	13.26
Monitored charities	6.39	1.38	5.76	10.10	14.06
Charities not monitored	7.90	1.02	5.61	11.46	11.11

Panel B: Descriptive Information on Revenues and Program Spending Data

	Mean	*First Quartile*	*Median*	*Third Quartile*	*Standard Deviation*
Total revenue (in $millions)					
Total sample	23.05*	2.10	7.22	23.07	44.92
Monitored charities	29.09	3.95	9.76	31.16	50.93
Charities not monitored	8.70	0.91	2.24	8.14	19.20
Program spending ratio, percent program expenses to total revenue (RATIO)					
Total sample	72.63	64.46	74.59	82.12	16.82
Monitored charities	73.41	65.93	75.20	82.63	16.82
Charities not monitored	70.78	63.72	73.97	81.59	16.77
Percent contributions to total revenue					
Total sample	63.30*	38.22	72.91	91.92	31.39
Monitored charities	71.41	55.33	81.13	93.64	27.51
Charities not monitored	44.02	15.18	40.37	70.92	31.75

(*Continued on next page*)

TABLE 2 (Continued)

Panel B: Descriptive Information on Revenues and Program Spending Data

	Mean	*First Quartile*	*Median*	*Third Quartile*	*Standard Deviation*
Annual percent change in program spending (%ΔPSPENDING)					
Total sample	11.69	0.72	7.69	17.34	24.15
Monitored charities	10.41	−0.72	6.00	15.23	25.99
Charities not monitored	14.73	4.20	9.81	18.26	18.83
Annual percent change in total revenue (%ΔREVENUE)					
Total sample	13.87	1.04	8.63	20.91	26.31
Monitored charities	12.46	−0.35	7.70	19.07	25.61
Charities not monitored	17.22	3.34	10.90	28.23	27.75
Annual percent change in program spending attributed to change in the program spending ratio (ΔYIELD)					
Total sample	−2.18	−7.82	−0.40	5.37	19.66
Monitored charities	−2.05	−7.34	−0.61	5.27	17.22
Charities not monitored	−2.49	−16.17	0.03	5.68	24.60

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* Differences in means between charities monitored and not monitored by the NCIB or PAS are statistically significant (one-tailed $p < 0.05$).

[a] The full sample has 331 charities, 233 are monitored and 98 are not monitored by the National Charities Information Bureau (NCIB) or the Council of Better Business Bureaus, Philanthropic Advisory Services (PAS) during 1996.

98 sample charities that are not monitored. Asterisks indicate that monitored organizations differ significantly from nonmonitored organizations on that characteristic.

Compensation profiles appear in Panel A of Table 2. If Form 990 does not designate an employee as the chief executive officer (or a comparable title), then we use compensation for the highest-paid employee. We specify compensation as the sum of current and deferred salary and benefits (Form 990, Part V, columns C and D). Including expense accounts as compensation yields comparable results. Compensation is higher for monitored charities than for charities that are not monitored, but compensation as a percent of revenue is lower.

Panel B of Table 2 displays distributions for other characteristics of the sample, including explanatory variables in the regression analysis. Total revenue includes direct and indirect contributions, government contributions, program service revenue, membership dues, investment revenue (including gains/losses on investment transactions), and both related and unrelated business income. Monitored charities have higher revenues, on average, than charities that are not monitored. Mean (median) percent program expenses to total revenues (RATIO) for all charity-year observations is 72.63 percent (74.59 percent). Thus, about 75 cents of each dollar contributed to our sample charities goes to the charitable objective. Mean (median) percent contributions to total revenue is 71.41 percent (81.13 percent) for monitored charities and 44.02 percent (40.37 percent) for charities that are not monitored. Thus, monitored organizations rely more heavily on contributions, as opposed to other revenue sources. Differences in the percent change in program spending (%ΔPSPENDING) and in the measures that result from partitioning percent change of program spending (%ΔREVENUE and ΔYIELD) are not statistically significant, however.

Finally, analysis not tabulated indicates that 79 charity-year observations are from environmental/animal-related charities; 165 are from medical research, disease/disorder, or health-related charities; 66 serve the international community; 100 are from social service charities; 72 are from arts or educational charities; 24 are from civil rights organizations; and 158 are from organizations that pursue other objectives.

IV. RESULTS

Table 3 reports the results of estimating Equation (3). Results for the entire sample, displayed in Column A, indicate that percent changes in CEO compensation and percent changes in program spending vary directly.[6] For example, increasing revenues by 10 percent is associated with a 0.90 percent compensation increase, or about 13.2 percent of the mean compensation increase displayed in Table 2. Similarly, increasing program spending by 10 percent by reducing the average cost of acquiring and administering funds is associated with a 0.76 percent compensation increase, or about 11.2 percent of the mean increase. In contrast, the average increase in compensation not related to program spending is 5.8 percent (intercept $\beta_0 = 0.058$). Prior studies of compensation paid to executives of publicly traded, for-profit firms indicate similarly modest relations between compensation changes and accounting performance measures (Rosen 1992).

For the sample as a whole, we cannot reject the null hypothesis $\beta_1 = \beta_2$. This result suggests that there is no significant difference in the extent to which charities reward increases in program spending attributable to revenue changes vs. increases in program spending attributable to changes in program spending ratios.

Columns B and C report results of estimating the regressions after partitioning the data according to whether the program spending ratio is above or below the median ratio. Between-sample statistical comparisons, obtained from a specification that uses dummy variables to allow the parameters β_i to differ between subsamples, appear in Column D. Results for subsamples partitioned according to whether the NCIB or the PAS monitors the charity, and the corresponding comparisons of parameter estimates, appear in Columns E, F, and G. Chow tests of whether the overall structure of the specification differs between subsamples appear at the bottom of the table.

Comparisons in Columns B and C of results for charity-years with relatively high program spending ratios (RATIO > median) with results for those with relatively low program spending ratios (RATIO < median) support the premise that charities weigh increases in program spending that result from reducing average costs of brokering resources more heavily when prevailing program spending ratios are low. In particular, we reject the null hypothesis $\beta_1 = \beta_2$ in favor of $\beta_1 > \beta_2$ for observations with relatively high program spending ratios (Column B), but not for observations with low ratios (Column C). Moreover,

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[6] The contemporaneous relations between percent changes in compensation and percent changes in program spending (designated %ΔPSPENDING) is:

$$\%\Delta COMP = 0.0892 + 0.0749\ (\%\Delta PSPENDING).$$

Respective t-statistics for parameter estimates are 9.510 and 4.775, and the adjusted $R^2 = 0.318$. These results confirm statistically significant relations between compensation changes and changes in program spending when program spending is not decomposed as in Equation (2).

TABLE 3

Regressions of Percent Change in Compensation (%ΔCOMP) on Percent Change in Program Spending Attributable to:
(1) Percent Change in Revenue (%ΔREVENUE), and
(2) Change in the Program Spending Ratio (ΔYIELD)
(full sample and partitioned by the program spending ratio (RATIO) and whether the charity is monitored by NCIB or PAS)

$$\%\Delta COMP_t = \beta_0 + \beta_1\ \%\Delta REVENUE_t + \beta_2\ \Delta YIELD_t + \varepsilon_t$$

		Sample Partitioned by Program Spending Ratio			*Sample Partitioned by whether Charity Is Monitored by NCIB or PAS*		
	(A)	*(B)*	*(C)*	*(D)*	*(E)*	*(F)*	*(G)*
Sample	*All Observations*	*RATIO >Median*[a]	*RATIO <Median*[a]	*Difference*[c]	*Monitored Charities*[b]	*Charities Not Monitored*[b]	*Difference*[c]
Intercept	0.058	0.058	0.052	0.006	0.053	0.072	−0.020
β_0	(9.17)	(6.29)	(5.91)	(0.47)	(7.16)	(5.81)	(−1.37)
%ΔREVENUE	0.090	0.109	0.072	0.037	0.106	0.058	0.048
β_1	(4.69)	(3.48)	(2.95)	(0.93)	(4.34)	(1.77)	(1.19)
ΔYIELD	0.076	0.035	0.107	−0.072	0.086	0.052	0.034
β_2	(3.38)	(0.88)	(3.80)	(−1.48)	(2.82)	(1.44)	(0.72)
Adjusted R^2	0.031	0.035	0.041	NA	0.014	0.007	NA
Test: $\beta_1 = \beta_2$	t = 0.07	t = 2.24	t = 1.26	NA	t = 0.66	t = 0.21	NA
Sample size	664	332	332	664	484	180	664
Chow test[c]	NA		F = 2.61			F = 0.79	
(d.f. = 3,658)			(p = 0.05)			(p = 0.50)	

Entries are parameter estimates and t-statistics in parentheses.

[a] The third and fourth columns indicate regression parameter estimates for the sample partitioned according to whether the program spending ratio (RATIO) in the prior year exceeds the median for all 664 observations.

[b] The sixth and seventh columns indicate regression parameter estimates for the sample partitioned according to whether either or both the National Charities Information Bureau (NCIB) or the Council of Better Business Bureaus, Philanthropic Advisory Services (PAS) monitor the charity during 1996.

[c] Indicates results for between-sample statistical comparisons of estimates. Entries for parameter estimates are differences in parameters and corresponding t-statistics for tests of between-sample differences. Results for Chow tests of between-sample differences in overall statistical relations appear in the last row of the table.

Variable definitions:

$\%\Delta COMP_t$ = year t − 1 to year t percent change in compensation paid to the chief executive officer. Compensation is the sum of current and deferred salary and benefits, displayed on Form 990, Part V, columns C and D, respectively;

$\%\Delta REVENUE_t$ = year t − 1 to year t percent change in total revenue. Total revenue is the amount on Form 990, Part I, line 12. Contributions are the sum of lines 1a and 1b, direct and indirect contributions. Indirect contributions are received from a central fundraising organization such as the United Way; and

$\Delta YIELD_t$ = the product of year t − 1 to year t percent change in the ratio of program expense to total revenue and year t total revenue, deflated by year t − 1 program expense. Program expenses are the sum of amounts reported as program services on Form 990, Part II, column B of line 44 and as payments to affiliates on Part I, line 16.

the parameter estimate β_2 on ΔYIELD is statistically significant for the low, but not for the high, ratio sample. Finally, the Chow test indicates statistically significant between-sample differences in the overall structure of the specification ($p = 0.05$). In contrast, comparisons in Columns E, F, and G indicate that relations do not differ significantly between charities that large oversight groups do or do not monitor.

Results for Equation (4), which include performance measures for both the current and the prior years, appear in Table 4.[7] These results consistently indicate significantly positive associations between current-period compensation changes and both current- and prior-year changes in program spending; however, associations are weaker for lagged than for contemporaneous relations. This investigation of relations between accounting-based performance measures for the prior year and current year compensation distinguishes the analysis from approaches used in studies of compensation paid to CEOs of publicly traded firms that typically restrict the analysis to contemporaneous relations between compensation and accounting performance measures (Gaver and Gaver 1998; Baber et al. 1999). This feature of the study is inspired by the possibility that charities may be more likely than commercial firms to use salary increases, rather than one-time bonuses, to reward accounting performance (Haber 1995).

In contrast with the result in Table 3, we cannot reject $(\gamma_1 + \gamma_2) = (\gamma_3 + \gamma_4)$ for the two partitions based on the program spending ratio. This suggests that executive compensation does not significantly differ between program spending changes resulting from revenue changes vs. those from changes in program spending ratios. Finally, differences in relations between subsamples (Columns B vs. C, and E vs. F) generally are not statistically significant at conventional levels. The exception is that the sensitivity of CEO compensation to the percent change in revenue in the prior year ($\%\Delta\text{REVENUE}_{t-1}$) is greater for charities not monitored, than for charities monitored, by large oversight organizations. In sum, the results in Table 4, where we consider performance measures in the prior year, generally reinforce the Table 3 results that compensation changes vary directly with changes in program spending. They do not support a conclusion that compensation changes are differentially sensitive to revenue changes vis-à-vis changes in the program spending ratio.

We conduct additional tests to evaluate whether the primary results are robust to competing interpretations. In particular, we consider whether results can be attributed to the availability of revenue in excess of expenses. Donors can interpret undistributed surplus as evidence that the organization is overfunded, which likely discourages contributions. On the other hand, donors are likely reluctant to contribute to financially distressed charities. Thus, except perhaps for reasonable allocation for contingencies, managers of charities have incentives to report a "breakeven condition." If so, then discretionary spending—salary increases, in particular—are more likely when the residual of revenues less essential program spending is positive than when residual resources are at or less than breakeven. We investigate this possibility by including the period t ratio of total revenue to total expenses as a control variable. Parameter estimates for this ratio typically are positive and statistically

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[7] We assess the relation between current-year percent change in compensation and current- and prior-year percent change in program spending by estimating:

$$\%\Delta\text{COMP}_t = 0.0313 + 0.1452\,(\%\Delta\text{PSPENDING}_t) + 0.0706\,(\%\Delta\text{PSPENDING}_{t-1}).$$

Respective t-statistics for parameter estimates are 4.464, 6.480, and 4.297, and the adjusted $R^2 = 0.1359$. These results confirm statistically significant relations between current-year compensation changes and changes in prior-year program spending when program spending is not decomposed as in Equation (2).

TABLE 4

Regressions of Current Period Percent Change in Compensation (%ΔCOMP) on Current and Prior Year Percent Change in Program Spending Attributable to Percent Change in Revenue (ΔAREVENUE) and Current and Prior Year Change in the Program Spending Ratio (ΔYIELD)

$$\%\Delta COMP_t = \gamma_0 + \gamma_1\ \%\Delta AREVENUE_t + \gamma_2\ \%\Delta AREVENUE_{t-1} + \gamma_3\ \Delta YIELD_t + \gamma_4\ \Delta YIELD_{t-1} + \varepsilon_t$$

		Sample Partitioned by Program Spending Ratio			*Sample Partitioned by whether Charity Is Monitored by NCIB or PAS*		
	(A)	*(B)*	*(C)*	*(D)*	*(E)*	*(F)*	*(G)*
Sample	*All Observations*	*RATIO >Median*[a]	*RATIO <Median*[a]	*Difference*[c]	*Monitored Charities*[b]	*Charities Not Monitored*[b]	*Difference*[c]
Intercept	0.034	0.040	0.027	0.013	0.035	0.029	0.007
γ_0	(4.67)	(4.48)	(2.19)	(0.87)	(4.38)	(1.53)	(0.34)
$\%\Delta AREVENUE_t$	0.128	0.088	0.151	−0.064	0.139	0.138	0.001
γ_1	(5.09)	(2.76)	(3.82)	(−1.21)	(5.00)	(2.18)	(0.02)
$\%\Delta AREVENUE_{t-1}$	0.069	0.069	0.074	−0.005	0.033	0.126	−0.094
γ_2	(3.72)	(2.23)	(2.93)	(−0.11)	(1.31)	(3.64)	(−2.31)
$\Delta YIELD_t$	0.167	0.095	0.191	−0.100	0.157	0.238	−0.081
γ_3	(6.27)	(1.98)	(5.36)	(−1.43)	(5.51)	(3.23)	(−1.10)
$\Delta YIELD_{t-1}$	0.085	0.065	0.085	−0.021	0.071	0.144	−0.072
γ_4	(3.90)	(1.62)	(2.86)	(−0.36)	(2.43)	(3.58)	(−1.53)
Adjusted R^2	0.137	0.056	0.167	NA	0.140	0.143	NA
Test: $(\gamma_1 + \gamma_2) = (\gamma_3 + \gamma_4)$	t = 1.44	t = 0.04	t = 0.95	NA	t = 1.15	t = 1.69	NA
Sample size	336	168	168	336	251	85	336
Chow test (d.f. = 5,327)	NA		F = 0.54 (p = 0.75)			F = 1.19 (p = 0.22)	

Entries are parameter estimates and t-statistics in parentheses.

[a] The third and fourth columns indicate regression parameter estimates for the sample partitioned according to whether the program spending ratio (RATIO) in the prior year exceeds the median for all 664 observations.

[b] The sixth and seventh columns indicate regression parameter estimates for the sample partitioned according to whether either or both the National Charities Information Bureau (NCIB) or the Council of Better Business Bureaus, Philanthropic Advisory Services (PAS) monitor the charity during 1996.

[c] Indicates results for between-sample statistical comparisons of estimates. Entries for parameter estimates are differences in parameters and corresponding t-statistics for tests of between-sample differences. Results for Chow tests of between-sample differences in overall statistical relations appear in the last row of the table.

Variables are defined in Table 3.

significant, but relations for other variables are comparable to those reported in Tables 3 and 4.

The FASB issued standards during our sample period that could change the accounting methods some charities use to report revenue.[8] Adopting new accounting standards for revenue recognition potentially confounds empirical relations. Eliminating from the sample 25 charity-year observations for which the potential effects of accounting changes exceed 5 percent of the current-year beginning account balances yields comparable results.[9]

V. CONCLUDING REMARKS

We investigate the relations between changes in the compensation charities pay their CEOs and changes in accounting performance measured as amounts directed toward the charity's main purpose—program activities. We provide empirical evidence on competing expectations about the role of accounting performance indicators in charities' compensation practices. Because outputs of charitable enterprises are typically not priced in the conventional sense, market-based performance measures that discipline managers of commercial enterprise are absent. Accounting indicators may therefore help charities monitor managers' performance. On the other hand, charitable organizations typically pursue nonfinancial objectives where success is difficult to quantify, so emphasis on accounting performance measures may prove dysfunctional.

Our results support the relevance of accounting-based performance measures as a basis for contracting with executives of charitable organizations. We document relations between changes in CEO compensation and changes in program spending, whether changes in program spending are attributable to changes in revenue raised or to changes in the amount of each contributed dollar that is allocated to the program activity.

We also investigate whether the use of program spending ratios to monitor charities affects relations between compensation and accounting measures. We distinguish changes in program spending attributable to changes in revenue raised from those attributable to changes in program spending ratios. We find no consistent evidence that relations between compensation and these two components of program spending differ, even for charities where concern about the program spending ratio is likely to be high—charities for which the prevailing program spending ratio is relatively low, and those monitored by large, well-known charity oversight groups that use program spending ratios to assess organization effectiveness. Such results suggest that outsiders' use of specific accounting measures to monitor performance does not cause differential weighting of changes in program spending that result from changes in program spending ratios.

As with studies of executive compensation in general, we cannot observe whether charities explicitly *use* accounting measures for setting executive compensation. Even so, we document relations that are consistent with the premise that charitable organizations reward managers for actions that increase funds directed toward the charitable objective.

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[8] Potential consequences of SFAS No.116, issued in 1994 and effective for most organizations for years beginning after December 15, 1995 (after 1994 for large organizations), are particularly relevant because the standard focuses on the recognition of both pledges to contribute and restricted donations. SFAS No. 117, issued at the same time, addresses the form and content of financial statements and is less likely to compromise the data reported on Form 990. For details, see Anthony (1995), Chase (1995), Khumawala and Gordon (1997), or Granof (1998).

[9] We compare beginning balances reported on the current year's return with ending balances reported on the prior year's returns for current asset and liability accounts affected by revenue recognition practices. Specifically, we compare accounts receivable (Form 990, item No. 47), pledges receivable (item No. 48), grants receivable (item No. 49), and deferred revenue (item No. 62). We assume that differences between these amounts are attributable to changes in accounting methods used to recognize revenue.

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BOOK REVIEWS

Jane L. Reimers, Editor

Editor's Note: The first book review in this issue introduces (or re-introduces) us to a special book selected by Bill McCarthy, a groundbreaking researcher and well-known scholar in the area of accounting information systems. His review gives us insights into the rich scholarly foundation of this exciting area of teaching and research. The second review addresses a very current topic in accounting, intangible assets. An outstanding accounting scholar, John Hand, has provided insights useful for both teaching and research. Thanks to both of these exceptional scholars for challenging us to expand our horizons in these important areas.

SOWA, JOHN F., *Knowledge Representation: Logical, Philosophical, and Computational Foundations* (Pacific Grove, CA: Brooks/Cole, 1999, pp. 512).

This is a splendid book for those academic accountants who want to expand their computer science and information systems horizons. For most readers of *The Accounting Review*, the topic of *knowledge representation* exists only at the periphery of their teaching and research interests, especially as defined by Sowa (xii) as "the application of logic and ontology to the task of constructing computable models for some domain." However, Sowa's mindset portends the way in which the principles of accounting information systems (AIS) will be taught in the five- to ten-year future, so if you are thinking of branching out to that part of accounting, you should start here. Moreover, the impending arrival of new business technologies like strongly typed enterprise systems (with business object components), e-commerce and e-reporting ontologies, and the semantic web makes some expertise in knowledge representation appealing for all accountants in the near term. For an overview of all of these trends, this book is an unparalleled source.

As I (and other semantic modeling instructors in AIS) rediscover every term in interactions with students, the most fundamental principles of knowledge representation come from the field of philosophy, followed in turn by those emanating from the fields of linguistics and behavioral science. Computer science and its semantic subsets (database theory, object-oriented analysis and programming, and artificial intelligence) are the Johnny-come-latelies of the field, and Sowa makes this intellectual progression and heritage obvious in every chapter. His dedication sets the tone: "To the spirits of the great knowledge engineers, Aristotle, Leibnitz, Kant, Peirce, and Whitehead."

I have been using Sowa's ideas since the publication of his first book, *Conceptual Structures: Information Processing in Mind and Machine*, in 1984, and I would guess that many accounting readers will find his present book a difficult first read. My advice is to skim it on the first time through—a strategy that the author seemingly encourages when he recommends that any chapter can be surveyed by reading just the first section and the first paragraph of each remaining section. In my opinion, it is Chapter 1 (Logic), Chapter 2 (Ontology), and Chapter 7 (Knowledge Acquisition and Sharing) that constitute the strongest message for accounting scholars. The intervening chapters—3 (Knowledge Representation), 4 (Processes), 4 (Purposes, Contexts, and Agents), and 6 (Knowledge Soup)—add more computational, logical, and notational detail. Although Sowa might not recommend it fully, I am convinced that a noncomputer-scientist can glean many valuable lessons here without incurring the additional overhead of learning his twin notations of predicate calculus and conceptual graphs. I have only a passable knowledge of both, yet I find the reference material here eminently usable.

As a liberal arts undergraduate in economics at Boston College, I was required to take four years of philosophy. (By contrast, I was only allowed to take two courses in accounting, and I had to sleep in a campus corridor overnight to get those precious slots.) In retrospect however, my logical, epistemological, and ethical training is a debt I will owe forever to the Jesuits there. I became a semantic modeler long before people like Ted Codd, Peter Chen, John and Diane Smith, and the Unified Modeling Language triplets (Rumbaugh, Jacobson, and Booch) invented ways for me to use that modeling. I often tell my students that my modeling mentor was my demanding metaphysics teacher: Father "D-minus" Murphy, S.J. It was he, following Plato, who first ingrained in me that the concept of "a horse" (which can describe an occurrence like Traveler or Secretariat) was vividly different from the concept of "horseness" (which can describe classes like nonexistent unicorns). This is a knowledge representation lesson I remember every time I have a student denote the difference between a "course" and a "course offering," or between "the actual time it took to install an Oldsmobile windshield" and "the engineered time it was supposed to take."

I mention my personal history in this review because I think Sowa's book offers a similar opportunity for those prospective conceptual modelers who missed people like Father Murphy while they were students. Sowa is one of the world's acknowledged experts in logic, ontology, and modeling. His book takes some effort, but the exertion is well worthwhile. I remembered how worthwhile just a few short weeks ago, when I had a protracted debate in class with an Executive M.B.A. student who insisted that a piece of inventory that had undergone an expensive restoration process *must* be given a new "item#" for database purposes. She said that her large automobile company handled its inventory that way, so it had to be correct. This is an "identity" problem that is impossible to solve out of context, but she was unwilling to accept that solution. I finally resolved matters when I able to expound philosophically on the "ship of Theseus" problem from the Greeks (Sowa, 270). The fact that philosophers had been debating a similar paradox for centuries made it easier for her (and the class) to accept an imprecise solution. The person who put that example at the tip of my tongue was John Sowa, and maybe he can do the same for you in your work.

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Michigan State University

LEV, BARUCH, *Intangibles: Management, Measurement, and Reporting* (Washington, D.C.: Brookings Institution Press, 2001, pp. 150).

Every ten years or so an accounting book is written that is truly important and eye opening—to businesspeople, to scholars, and to policymakers. This is such a book, and I strongly recommend it. It is crisply written, expansive, and challenging. It will educate and inform M.B.A. students, executives, Ph.D. students, and faculty alike. If there is only one book that you read in 2002, I urge that it be this one.

Intangibles: Management, Measurement, and Reporting by Baruch Lev is a fine blend of positive and normative economics. Over a decade ago, Lev realized that accounting scholarship had completely missed the boat on intangibles, viewing them as either irrelevant (e.g., recognized goodwill), impossibly hard to quantify (e.g., R&D), or suspiciously fuzzy (e.g., organizational capital). Drawing on both his own extensive research and that of other scholars in accounting, economics, finance, organization, and strategy, Lev convincingly demonstrates that none of those views is correct. In doing so, he greatly advances the literature on intangibles along four dimensions: the economics of intangibles; the empirical record of intangibles; the problems resulting from the lack of information about intangibles; and the set of available solutions to fix the problems arising from the currently poor financial reporting for intangibles.

The book is organized into six chapters. Chapter 1 addresses three questions: What are intangible assets? Why is there such a strong interest in intangibles today? Who should be concerned? Lev persuasively argues that the central thread in the answer to these questions is the realization that globalization, deregulation, and technological changes have forced firms to rely increasingly more on innovation in their bid to create and sustain abnormal profitability. Since intangibles are the primary driver of the supply of innovation, this has brought intangibles to center stage in the real world of business.

Chapter 2 presents an economic framework for analyzing both the value drivers and value detractors of intangibles. This is a vital chapter because it puts front and center the "nonstandard" economic features of intangibles—the benefits of scalability (nonrivalry), increasing returns, network effects, the costs or limitations of high risk, lack of full control over the benefits, and the absence of well-organized markets.

Chapter 3 presents the evidence bearing on the economic framework laid out in Chapter 2. Lev surveys and synthesizes the empirical record on the contribution of intangibles to corporate value and growth along what he argues to be the three major nexuses of intangibles: discovery (e.g., R&D), organizational capital (e.g., branding), and human resources. The vast majority of the evidence marshaled by Lev is strongly consistent with his economic framework.

Chapter 4 neatly starts the transition to normative economics by documenting and then tracing the measurement and reporting problems of intangibles to their peculiar downsides—from Chapter 2, these are partial excludability, inherent risk, and nontradability. Lev argues that these economic features of intangibles create information failures that combine with a lack of incentives facing corporate executives and auditors to yield serious private and social harms. Among the more persuasive of these are the excessive insider gains to officers of R&D-intensive firms, the "lemons discount" to the prices of intangible-intensive small firms, and the high cost of capital facing knowledge-intensive companies.

From this impressive springboard, Lev devotes Chapter 5 to providing his answer to "the Tolstoyan question: What then must we do?" In terms of disclosure, Lev advocates that the FASB and SEC develop a comprehensive information system that he terms the "Value Chain Scoreboard." The goal of the Value Chain Scoreboard is to provide standardized information to capital markets about a firm's value chain or business model—that is, the fundamental economic processes through which the firm creates and converts innovation into shareholder value. In terms of recognition, Lev urges that the recognition of intangible assets in financial reports be significantly broadened. The book concludes in Chapter 6 with a personal call to action on the part of scholars, businesspeople, and policymakers to comprehensively integrate intangible assets into the formulation and execution of management strategy and security analysis.

In summary, I believe that *Intangibles: Management, Measurement, and Reporting* by Baruch Lev deserves an active place on the desk and in the classroom of every financial accountant and financial economist. It has certainly significantly impacted the ways I teach and research financial accounting, business economics, and valuation. It is intellectually rigorous but at the same time readily accessible to M.B.A.s, Masters of Accounting, and Ph.D. students. As such, it also deserves an active place on the desk of every major decision maker and policy setter in corporate America.

JOHN R. M. HAND

The University of North Carolina at Chapel Hill

Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended February 28, 2002. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s).*

	Number of Manuscripts	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	75	75	25.51
31 ≤ Days ≤ 60	108	183	62.24
61 ≤ Days ≤ 90	77	260	88.44
91 ≤ Days ≤ 120	27	287	97.62
121 ≤ Days	7	294	100.00

The mean review time was 52 days; the median review time was 51 days.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal....No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow (with a slight modification) the B-format of *The Chicago Manual of Style* (14th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's International Dictionary*.

FORMAT

1. All manuscripts should be typed in 12-point font on one side of $8\frac{1}{2} \times 11''$ good quality paper and be double spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, the author's name, title and affiliation, email address, any acknowledgments, and a footnote indicating whether the author would be willing to share the data (see last paragraph in this statement).

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years*. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form*. See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

ABSTRACT/INTRODUCTION

An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings.

Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate by marginal notation where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Work cited should use the "author-date system" keyed to a list of works in the reference list (see below). Authors should make an effort to include the relevant page numbers in the cited works.

1. In the text, works are cited as follows: authors' last name and date, without comma, in parentheses: for example, (Jones 1987); with two authors: (Jones and Freeman 1973); with more than two: (Jones et al. 1985); with more than one source cited together (Jones 1987; Freeman 1986); with two or more works by one author: (Jones 1985, 1987).
2. Unless confusion would result, do not use "p." or "pp." before page numbers: for example, (Jones 1987, 115).
3. When the reference list contains more than one work of an author published in the same year, the suffix a, b, etc. follows the date in the text citation: for example, (Jones 1987a) or (Jones 1987a; Freeman 1985b).
4. If an author's name is mentioned in the text, it need not be repeated in the citation; for example, "Jones (1987, 115) says...."
5. Citations to institutional works should use acronyms or short titles where practicable; for example, (AAA ASOBAT 1966); (AICPA Cohen Commission Report 1977). Where brief, the full title of an institutional work might be shown in a citation: for example, (ICAEW *The Corporate Report* 1975).
6. If the manuscript refers to statutes, legal treatises or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a list of references containing only those works cited. Each entry should contain all data necessary for unambiguous identification. With the author-date system, use the following format recommended by *The Chicago Manual*:

1. Arrange citations in alphabetical order according to surname of the first author or the name of the institution responsible for the citation.
2. Use author's initials instead of proper names.
3. Date of publication should be placed immediately after author's name.
4. Titles of journals should not be abbreviated.
5. Multiple works by the same author(s) in the same year are distinguished by letters after the date.
6. Inclusive page numbers are treated as recommended in *Chicago Manual* section 8.67.

Sample entries are as follows:

American Accounting Association, Committee on Concepts and Standards for External Financial Reports. 1977. *Statement on Accounting Theory and Theory Acceptance*. Sarasota, FL: AAA.

Demski, J. S., and D. E. M. Sappington. 1989. Hierarchical structure and responsibility accounting. *Journal of Accounting Research* 27 (Spring): 40–58.

Dye, R., B. Balachandran, and R. Magee. 1989. Contingent fees for audit firms. Working paper, Northwestern University, Evanston, IL.

Fabozzi, F., and I. Pollack, eds. 1987. *The Handbook of Fixed Income Securities.* 2nd edition. Homewood, IL: Dow Jones-Irwin.

Kahneman, D., P. Slovic, and A. Tversky, eds. 1982. *Judgment Under Uncertainty: Heuristics and Biases.* Cambridge, U.K.: Cambridge University Press.

Porcano, T. M. 1984a. Distributive justice and tax policy. *The Accounting Review* 59 (October): 619–636.

———. 1984b. The perceived effects of tax policy on corporate investment intentions. *The Journal of the American Taxation Association* 6 (Fall): 7–19.

Shaw, W. H. 1985. Empirical evidence on the market impact of the safe harbor leasing law. Ph.D. dissertation, The University of Texas at Austin.

Sherman, T. M., ed. 1984. *Conceptual Framework for Financial Accounting.* Cambridge, MA: Harvard Business School.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be consecutively numbered throughout the manuscript with superscript Arabic numerals. Footnote text should be doubled-spaced and placed at the end of the article.

SUBMISSION OF MANUSCRIPTS

Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state that the work is not submitted or published elsewhere.
2. In the case of manuscripts reporting on field surveys or experiments, four copies of the instrument (questionnaire, case, interview plan or the like) should be submitted. Information that might identify the author(s) must be deleted from the instrument.
3. Four copies should be submitted together with a check in U.S. funds for $75.00 for members or $100.00 for nonmembers of the AAA made payable to the American Accounting Association. Effective January 1990, the submission fee is nonrefundable.
4. The author should retain a copy of the paper.
5. Revisions must be submitted within 12 months from request, otherwise they will be considered new submissions.

COMMENTS

Comments on articles previously published in *The Accounting Review* will be reviewed (anonymously) by two reviewers in sequence. The first reviewer will be the author of the original article being subjected to critique. If substance permits, a suitably revised comment will be sent to a second reviewer to determine its publishability in *The Accounting Review*. If a comment is accepted for publication, the original author will be invited to reply. All other editorial requirements, as enumerated above, also apply to proposed comments.

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POLICY ON DATA AVAILABILITY

The following policy has been adopted by the Executive Committee in its April 1989 meeting.

"An objective of (*The Accounting Review, Accounting Horizons, Issues in Accounting Education*) is to provide the widest possible dissemination of knowledge based on systematic scholarly inquiries into accounting as a field of professional research, and educational activity. As part of this process, authors are encouraged to make their data available for use by others in extending or replicating results reported in their articles. Authors of articles which report data dependent results should footnote the status of data availability and, when pertinent, this should be accompanied by information on how the data may be obtained."

JULY 2002 PLACEMENT ADS

The deadline for information to be included in the "Placement Information" section is two months prior to the date of publication. The ***Review*** *is published on the first day of January, April, July, and October. Placement advertising and replies should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, ATTENTION: ADVERTISING DEPT. All replies to box numbers should be on separate sheets to facilitate forwarding.*

301 MICHIGAN STATE UNIVERSITY, Eli Broad Graduate School of Management, seeks to hire one tenure-track Assistant Professor faculty member in Financial Accounting beginning August 2002. Michigan State provides an excellent environment for new faculty—competitive salary and benefits, summer research support, and an attractive teaching load. An earned doctorate is required and professional certification and teaching experience in Financial Accounting are desirable. ABD candidates in advanced stages of the dissertation will be considered. Review of applications will begin November 26, and continue until position is filled. Candidates are asked to submit an application letter, resume, and the names, addresses, and phone numbers of three references. Contact Professor Thomas J. Linsmeier, N256 Business College Complex, Eli Broad College of Business, Michigan State University, East Lansing, MI 48824-1121; Email: tjl@msu.edu. Michigan State University is an Affirmative Action/Equal Opportunity Institution. Handicappers have the right to request and receive reasonable accommodations.

302 Ph.D., CPA, is seeking a tenure-track or visiting position starting Fall 2002 (could start in January 2003 also). I have over 21 years of teaching experience. Although I can and have taught all accounting courses, I prefer to teach Financial Accounting and Auditing. If you have a position that you want to fill with a quality teacher and active researcher, contact inam1950@yahoo.com. Inam Hussain, member AAA.

303 EAST CAROLINA UNIVERSITY, Department of Accounting, invites applications for an Assistant Professor position in Accounting effective August 15, 2002. Applicants should have or expect to have a Ph.D. in Accounting by December 2002, and appropriate professional certification. Our current needs include Cost/Taxation, and AIS. The position is currently funded. ECU is a comprehensive university (18,000 students) with AACSB accreditation. We offer an undergraduate degree and an M.S.A. (concentrations in Taxation and MIS). We are seeking candidates with excellent teaching skills; appropriate service is also expected. We have reasonable research. Applications will be accepted until the position is filled. Send credentials (application letter, resume) to Dr. Edwin Doty, Chairperson, Accounting Department, School of Business, 3208-A Bate Building, ECU, Greenville, NC 27858-4353. Official transcripts are required upon employment. Applicants must comply with the Immigration Reform and Control Act. ECU is an Equal Opportunity/Affirmative Action University and accommodates individuals with disabilities.

304 KANSAS STATE UNIVERSITY, Department of Accounting, invites applications for a tenure-track position in Auditing and Assurance Services beginning August 2002. The person who fills the position will teach at both the graduate and undergraduate levels. Professional certification and knowledge of Accounting Information Systems are desirable. Commitment to teaching excellence and scholarly research is required. Candidates should have a doctoral degree in Accounting or expect to complete all requirements during the academic year for which he or she is initially hired. Review of applications will begin immediately and continue until the position is filled. A visitor for 2002–2003 will be considered. Send *curriculum vitae*, a list of three references, and evidence of scholarship (the dissertation proposal is acceptable for candidates with degree in progress) to O. Finley Graves, Head, Department of Accounting, Calvin Hall 109A, KSU, Manhattan, KS 66506-0502; Email: fgraves@ksu.edu. The University is an EEO Employer and actively seeks diversity in its pools of applicants.

305 WRIGHT STATE UNIVERSITY, Raj Soin College of Business, invites applications for a tenure-track position in the Department of Accountancy at the rank of Assistant or Associate Professor, beginning Fall 2002. Applicants must possess a doctorate in Accounting and be academically qualified under AACSB International standards. Refereed publications, professional certification, and relevant teaching and business experience are preferred. Applicants should have a commitment to excellence in teaching, service, and ongoing scholarship. The Department of Accountancy offers Bachelor's and Master's programs in Accountancy that are separately accredited by AACSB. Support for summer research, teaching, and/or professional development are available. Screening will begin April 22, 2002 with the position available September 1, 2002. Please send application letter, *curriculum vitae,* and three references with contact information to Dr. Kennard S. Brackney, Chair, Department of Accountancy, Raj Soin College of Business, Wright State University, 3640 Colonel Glenn Highway, Dayton, OH 45435-0001. WSU is an EO/AA Employer.

306 DISTANCE EDUCATION INSTRUCTOR position wanted. Qualifications include three years' experience in Accounting and Taxation courses at the upper and lower levels, both as instructor and course developer; Master's in Taxation and Ph.D. with emphasis in Accounting; CPA. Please contact directly by email at florencecarroll@yahoo.com.

307 DISTANCE EDUCATION INSTRUCTOR position wanted. Three years' experience in Accounting courses at the upper and lower level, both as instructor and course developer. M.B.A., Master's in Accounting, D.B.A. (ABD), CPA, CMA, CFM, and CVA. Please contact directly by email at paulp912002@yahoo.com.

308 WESTERN MICHIGAN UNIVERSITY, Haworth College of Business, invites applications for a tenure-track position as Assistant or Associate Professor of Accounting beginning Fall 2003. Applicants must hold a doctoral degree in Accounting or IS or be ABD and near completion. Ideal candidates will have primary teaching and research interests in Accounting Information Systems and/or Taxation. Excellent English communication skills are required. Western Michigan University is a Carnegie-designated Doctoral/Research-Extensive University, and is only one of four Michigan public universities to be included in Carnegie's top research classification. Both the accounting undergraduate and graduate programs are accredited by the AACSB. Please send *curriculum vitae* and research proposals and/or samples of research to Jack M. Ruhl, Chairperson, Department of Accountancy, Haworth College of Business, Western Michigan University, Kalamazoo, MI 49008; Email: jack.ruhl@wmich.edu. Western Michigan University is an Affirmative Action/Equal Opportunity Employer and has a strong commitment to diversity.

309 UNIVERSITY OF VIRGINIA, McIntire School of Commerce, is seeking applications for one full-time tenure-track position in the Accounting area beginning September 2003. The position is open to experienced candidates at all ranks. Applicants should have a Ph.D. in hand, demonstrate practical business experience, and possess a strong record in Accounting research and teaching in an academic institution. The strongest candidates will have expertise in Financial Accounting research, as well as an ability to teach Financial Accounting at both the undergraduate and graduate levels. The deadline for applications is October 31, 2002; however, review of applicants will begin immediately and continue until the position is filled. Additional information about the McIntire School of Commerce can be found at: http://www.commerce.virginia.edu. Applicants should send a letter of interest and a *curriculum vitae* to Dr. David B. Croll, Accounting Area Coordinator, McIntire School of Commerce, Monroe Hall, University of Virginia, PO Box 400173, Charlottesville, VA 22904. The University of Virginia is an Equal Opportunity/Affirmative Action Employer.

310 JOHNS HOPKINS UNIVERSITY, School of Professional Studies in Business and Education, the Graduate Division of Business and Management—the largest academic unit in the School of Professional Studies—anticipates the availability of an Assistant Professor of Accounting position for the 2002–03 academic year. Current faculty members hold a balance of assignments that includes graduate-level teaching; development and oversight of new and existing programs; research; and outreach to and collaboration with the corporate business community. An appropriate doctorate and CPA are required; relevant professional experience and prior graduate level teaching are preferred. Salaries are competitive with excellent benefits and tuition remission. Forward letters of application and resumes to Dr. Lindsay Thompson, Office of the Associate Dean, Johns Hopkins University, Graduate Division of Business and Management, School of Professional Studies in Business and Education, 10 North Charles Street, Baltimore, Md. 21201. AA/EOE. Smoke Free and Drug Free.

311 GRAND VALLEY STATE UNIVERSITY, Seidman School of Business, invites applications for Assistant/Associate/Full Professor in Accounting. Tenure-track positions are available Fall 2003 and open until filled. Ph.D. or D.B.A. (by December 2003) in Accounting or related field is required. Professional certification is desirable. Cost/Managerial Accounting and Financial Accounting are primary needs; other fields may be considered. Effective teaching, research, and service to the University and community are expected. The Department and School are both accredited by the AACSB. Salary is competitive. For consideration, send resume, three references, and sample of teaching evaluations to Ms. Kathy Goralski, Accounting Search Committee, Department of Accounting and Taxation, Grand Valley State University, 401 West Fulton Street, Grand Rapids, MI 49504. To arrange an interview at the AAA Annual Meeting in San Antonio, please contact Professor Stephen Goldberg, Accounting Department, by August 1, 2002—Phone: (616) 336-7410; Email: goldbers@gvsu.edu. Grand Valley State University is an EO/AA Institution.

312 UNIVERSITY OF SOUTH ALABAMA, Mitchell College of Business, Department of Accounting, invites applications to fill two tenure-track positions at the Assistant, Associate, or Professor levels for Fall 2003. Applicants for the first position must have a primary emphasis in Auditing. Applicants for the second position may have a primary emphasis in any area of Accounting except tax. Applicants for both positions should possess a Ph.D. with a concentration in Accounting or be near completion of the dissertation. The Department offers both master's and undergraduate degrees in Accounting. The Mitchell College of Business is accredited by AACSB International. Applications will be accepted until November 15, 2002. Send letter of application and *curriculum vitae* with three references to Frank R. Urbancic, Department of Accounting, Mitchell College of Business, University of South Alabama, Mobile AL 36688. The University of South Alabama is an EEO/AA/Title VI Title IX/Section 504/ADA/ADEA Employer.

313 UNIVERSITY OF CINCINNATI, Department of Accounting and Information Systems, invites applications for visiting and tenure-track positions in Accounting beginning Fall 2002, contingent upon available funding. Rank is open and all areas of specialization will be considered. Applicants must have a Ph.D. in Accounting, and have the potential for excellence in research and teaching, as well as a commitment to professional service. The College of Business is AACSB-accredited at the B.B.A. and M.B.A. levels and offers a Ph.D. In addition to a B.B.A. in Accounting, M.S. degrees in Accounting and Taxation are offered. Submit letter of application, *curriculum vitae,* teaching evaluations, three letters of reference, and a working paper and/or defended dissertation proposal to Professor Jens Stephan, Department of Accounting and Information Systems, University of Cincinnati, Cincinnati, OH 45221-0211. The University of Cincinnati is an Affirmative Action/Equal Opportunity Employer.

314 OAKLAND UNIVERSITY, Rochester, Michigan (http://www.sba.oakland.edu), seeks full-time tenure-track, Assistant Professor of Taxation/Accounting. Primary teaching responsibilities will be in Taxation. Qualified candidates must possess a doctorate in Accounting, or be ABD with firm completion date (alternatively, L.L.M.-Taxation and CPA; or J.D. and Master of Accounting), and have experience with U.S. income tax system. Accounting Program (and School of Business Administration) accredited by AACSB-International. Research activity supported and expected. Market-competitive salary; excellent fringe benefits; five classes per academic year. Located 25 miles north of Detroit in area of suburban housing and modern technology parks. For interview at AAA Annual Meeting, send resume, evidence of research and teaching, and three letters of recommendation to Chair, Accounting Faculty Search Committee, School of Business Administration, Oakland University, Rochester, MI 48309-4493; Email: dillon@oakland.edu. Oakland University is an Affirmative Action/Equal Opportunity Employer and encourages applications from women and minorities.

315 SOUTHERN ILLINOIS UNIVERSITY EDWARDSVILLE invites applications for a tenure-track position beginning in August 2003. A Ph.D. in Accounting is required, but ABDs nearing completion will be considered. Candidates must be qualified to teach Financial Accounting and must be committed to excellence in teaching and scholarly research. The Department of Accounting offers AACSB-accredited programs at both the undergraduate and master's levels. SIUE is located on a suburban campus of 2,700 acres approximately 25 minutes from downtown St. Louis, MO. Send *curriculum vitae* to Michael L. Costigan, Chair, Department of Accounting, Campus Box 1104, Southern Illinois University Edwardsville, Edwardsville, IL 62026-1104; Email: mcostig@siue.edu. SIUE is an Equal Opportunity/Affirmative Action Employer, and applications from women and minorities are especially encouraged.

316 UNIVERSITY OF SOUTHERN MAINE, Department of Accounting and Finance, invites applications for Faculty/Endowed Chair, Accounting #1931, Associate or Full Professor rank for appointment as the L.L. Bean/Lee Surace Endowed Chair in Accounting, beginning Fall 2003. Applicants must have a Ph.D. or D.B.A. in Accounting from an AACSB-International-accredited program, along with a demonstrated record of quality scholarship and teaching, a commitment to scholarly leadership and mentoring of faculty, and a willingness to work closely with the accounting and business community. The CPA or CMA designation is desirable. Teaching responsibilities are at the graduate and undergraduate levels. Review of applications begins immediately and continues until the position is filled. The School of Business at USM is accredited by AACSB International. Send letter of application, *curriculum vitae,* and three letters of recommendation to Professor John Sanders, USM, Department of Accounting and Finance, RE: 101, PO Box 9300, Portland, Maine 04104-9300; Email: jsanders@usm.maine.edu; Fax: (207) 780-4662. EO/AA Employer.

317 UNIVERSITY OF MARYLAND, College Park, invites applications for a tenure-track/tenured position in Financial Accounting to start in August 2003. A strong research record is required. Send *curriculum vitae,* three letters of recommendation, and evidence of research to Professor Oliver Kim, The Robert H. Smith School of Business, University of Maryland, Van Munching Hall, College Park, MD 20742-1815. The University of Maryland is an Equal Opportunity/Affirmative Action Employer. Women and minorities are especially encouraged to apply.

318 MARQUETTE UNIVERSITY invites applications for a tenure-track position as Assistant Professor of Accounting beginning Fall 2003. Applicants should hold a doctoral degree or be in final stages of dissertation. Primary teaching responsibility will be in Accounting Information Systems and secondary responsibility will be in Managerial or Financial at the undergraduate and graduate levels. The normal teaching load is 15 semester hours per year. Salary and research support are competitive. The department has 11 full-time faculty. Marquette offers AACSB-accredited undergraduate and graduate degrees in Accounting and Business. Please send *curriculum vitae* and inquiries to Don E. Giacomino, Professor and Chair, Department of Accounting, Straz 303, College of Business Administration, Marquette University, Milwaukee, WI 53201; Email: don.giacomino@marquette.edu. Marquette University is an Affirmative Action/Equal Opportunity Employer.

319 NANYANG TECHNOLOGICAL UNIVERSITY, Nanyang Business School, Singapore, invites applications for faculty positions in areas of Financial Accounting, Management Accounting, and Auditing/IS Audit. The School offers undergraduate degrees in Accountancy and Business, M.B.A. degrees, and Ph.D. degrees. Applicants should possess a Ph.D. in Accounting or related area, and be committed to excellence in scholarly research and teaching. Salary and benefits will be competitive. The commencing salary will depend on the successful applicant's qualifications, experience, and the level of appointment offered. Please access our web site for details. To apply, send in your application form (download from our web site) or detailed *curriculum vitae,* which should include areas of your research interest, a list of your publications, and address and email of three refrences, to Vice President (Human Resources), Administration Building, Level 4, 50 Nanyang Avenue, Singapore 639798; Email: EHLEE@ntu.edu.sg; Web Site: http://www.ntu.edu.sg/personnel/.

320 UNIVERSITY OF VERMONT invites applications for a tenure-track position in Accounting at the Assistant Professor rank for Fall 2003. Applicants should have the earned doctorate, evidence of excellent teaching skills, and a research program that is targeted toward publication in high-quality journals. Teaching responsibilities include Introductory and Advanced courses in Financial and Managerial Accounting. Especially encouraged are candidates with teaching and research interests in environmental and social accountability. The School of Business Administration is an AACSB-accredited institution with approximately 800 undergraduates and 75 M.B.A. students. Our M.B.A. program was recently ranked 12th by *U.S. News & World Report*, based on GMAT scores for part-time M.B.A. programs. Salary and benefits are AACSB-competitive. Applications accepted until the position is filled; submission by July 15 is encouraged. Send letter of interest, resume, and list of three references to Stephen Dempsey, School of Business Administration, University of Vermont, 55 Colchester Avenue, Burlington, VT 05405-0157; Email: stephen.dempsey@uvm.edu. The University of Vermont is an AA/EOE.

321 UNIVERSITY OF TOLEDO invites applicants for one tenure-track position at the Assistant or Associate Professor level beginning Fall 2003. Applicants must have an interest in teaching Financial Accounting, Systems, or Cost/Managerial at the undergraduate and graduate levels. Faculty is also expected to be actively involved in scholarly activities. Applicants should have a Ph.D. or D.B.A. in Accounting from an accredited university or ABD with an expected completion by May 2003 is required. Professional certification (CPA, CMA, CIA) and relevant professional work experience are highly desirable. The position is open until filled. Send letters of application and letters of recommendations to Diana Franz, Chair Department of Accounting, College of Business Administration, University of Toledo, Toledo, OH 43606; Email: Diana.Franz@utoledo.edu. The University of Toledo is an AA/EOE.

322 LOUISIANA STATE UNIVERSITY IN SHREVEPORT invites applications for a tenure-track position at the Assistant or Associate Professor level. A doctoral degree or ABD nearing completion and a strong commitment to both research and teaching are required. Professional certification is highly desirable. Applicants from all areas are invited to apply, but Financial Accounting and Auditing are primary needs. Candidates should be active in professional organizations and have the ability and willingness to interact with the business community. LSUS is an AACSB-accredited school located in a growing metropolitan area and offers a competitive salary. Review of candidates will begin immediately and continue until the position is filled. Please submit a resume including three references to Douglas S. Bible, Chair Department of Accounting and Business Law, LSUS, Shreveport, LA 71115; Email: dbible@pilot.lsus.edu. LSUS is an Equal Opportunity/Affirmative Action Employer.

323 EASTERN MICHIGAN UNIVERSITY seeks applicants for three tenure-track Assistant/Associate Professor positions for Fall 2003 (one each in: Accounting Information Systems [Posting #F0301], Managerial Accounting [Posting #F0302], and in Tax [Posting #F0303]), all subject to availability of funding. Flexibility regarding instructional area is necessary along with appointment in the primary areas. Ph.D. or D.B.A. with Accounting major preferred, advanced doctoral candidates considered; substantial evidence of potential for excellence in teaching and research required. CPA certification, professional and teaching experience desired. Other duties include maintaining strong ties to the professional accounting community, developing curriculum, and advising students. Salary is competitive. EMU is AACSB-accredited. Submit letter, *curriculum vitae*, transcripts, teaching evaluations and three reference letters to Posting #F0301 (Accounting Information Systems), Posting #F0302 (Managerial Accounting) or Posting #F0303 (Tax), Eastern Michigan University, 202 Boone Hall, Ypsilanti, MI 48197. Contact Dr. Susan Kattelus by email at susan.kattelus@emich.edu to arrange an interview at the 2002 AAA Annual Meeting. AA/EOE. We encourage applications from women and members of minority groups.

324 UNIVERSITY OF SOUTH FLORIDA, School of Accountancy, invites applicants for tenure-earning Assistant Professor of Accounting beginning Fall 2003. This position will involve teaching in undergraduate and master's programs and involvement with Ph.D. programs. Other duties include conducting research leading to publications in refereed journals and service to the academic and professional communities. Ph.D. or near completion required; preference will be given to candidates specializing in AIS and Auditing. To apply or for more complete description of position and minimum qualifications contact Robert M. Keith, School of Accountancy, College of Business Administration, University of South Florida, 4202 E. Fowler Avenue (BSN3403), Tampa, FL 33620-5500; Phone (813) 974-6516; Fax (813) 974-6528; Email: rkeith@coba.usf.edu. Applications close October 11, 2002. Position is subject to final university funding. USF is an Equal Opportunity/Affirmative Action Institution. For disability accommodations, call (813) 974-6389 a minimum of five working days in advance. Under Florida law, applications and meetings are public.

325 UNIVERSITY OF SOUTH FLORIDA, School of Accountancy, invites applicants for the Johnson Endowed Professor of Accounting beginning Fall 2003. Candidates should have an earned doctorate, credentials commensurate with the rank of Associate or Full Professor, a demonstrated interest in behavioral (judgment/decision-making) research, and an excellent teaching record. Preference will be given to candidates with teaching and research experience in Financial Accounting or Auditing. To apply or for more complete description of position and minimum qualifications contact Robert M. Keith, School of Accountancy, College of Business Administration, University of South Florida, 4202 E. Fowler Avenue (BSN3403), Tampa, FL 33620-5500; Phone (813) 974-6516; Fax (813) 974-6528; Email: rkeith@coba.usf.edu. Applications close October 11, 2002. Position is subject to final university funding. USF is an Equal Opportunity/Affirmative Action Institution. For disability accommodations, call (813) 974-6389 a minimum of five working days in advance. Under Florida law, applications and meetings are public.

326 ILLINOIS STATE UNIVERSITY, Department of Accounting, houses both Accounting and Business Information Systems faculty of the College of Business. The University invites applications for full-time, tenure-track Assistant and/or Associate Professor positions in the Department of Accounting, preferably to begin August 16, 2003 contingent on budgetary approval and appropriate funding. Required qualifications include a Ph.D. or D.B.A. in Accounting, a strong commitment to quality teaching, service, and research. ABDs will be considered if substantial progress has been made on the dissertation. Teaching area is open, but preference will be given to applicants with primary or secondary interest in Accounting Information Systems. Initial review of applications will begin September 1, 2002, and continue until position is filled. To assure full consideration, please send a letter of application and evidence of qualifications by September 1, 2002 to James E. Moon, Chairperson, Department of Accounting, Campus Box 5520, Illinois State University, Normal, IL 61790-5520; Email: jemoon@ilstu.edu. Illinois State is an Equal Opportunity/Affirmative Action University encouraging diversity.

327 UNIVERSITY OF MICHIGAN–DEARBORN invites applicants for a tenure-track Accounting position, available for September 1, 2003. The primary interest in Taxation is required with secondary interest open. Faculty rank is open for candidates possessing a Ph.D./D.B.A. in Accounting, with professional certification preferred. The successful applicant will be dedicated to quality teaching, research, and service. Salary is competitive. Support for research and professional development is available. Interested candidates should send a letter of application, current *curriculum vitae,* graduate transcript, and three letters of recommendation to Julie Dziekan, Assistant Dean, School of Management, Room 2140 SOM, University of Michigan–Dearborn, Dearborn, MI 48128-1491. Applicant review starts October 1, 2002 and will continue until the position is filled. AAA MEETING CONTACT: Professor Brian Green; Email: bpgreen@umd.umich.edu. The University of Michigan–Dearborn is dedicated to the goal of building a culturally diverse and pluralistic faculty committed to teaching and working in a multicultural environment, and strongly encourages applications from minorities and women. The University of Michigan–Dearborn is an Equal Opportunity/Affirmative Action Employer.

328 UNIVERSITY OF NEW ORLEANS invites applications for an anticipated Assistant Professor position for Fall 2003. Preference will be given to those with a primary teaching/research interest in Accounting Information Systems or Cost/Managerial, with a secondary interest in Financial Accounting or Auditing. Candidates must have the Ph.D. with an Accounting major (or ABD with completion of the Ph.D. by the end of 2003). Salary is negotiable and competitive. The Department offers the undergraduate Accounting degree (over 550 majors) and two Master's degrees (including a Tax Option). The Department has separate AACSB Accounting accreditation at both the graduate and undergraduate levels. Candidates should send a resume to Professor Philip J. Harmelink, Chairman, Department of Accounting, College of Business Administration, University of New Orleans, New Orleans, LA 70148; Email: pharmeli@uno.edu. The University of New Orleans is a member of the Louisiana State University System and an Affirmative Action/Equal Opportunity Employer.

329 VISITING POSITION wanted: One semester preferred, possibly one year or more, starting January 2003. AACSB-International-accredited Ph.D. in Accounting, CMA. Prolific, active scholar, top-quality publication/presentation record, 15+ years teaching, multi-year service on peer-reviewed journal boards, administrative/accreditation experience (Chair, Department of Accounting and Finance), ongoing liaison with professional/business community. Courses taught (graduate and undergraduate): Financial Accounting, Cost/Managerial, Performance M&C, Accounting Theory, FSA, International. Contact greatprof@highstream.net.

330 SUFFOLK UNIVERSITY, Sawyer School of Management, invites applications for a tenure-track Accounting position for Fall 2002. All areas of research expertise will be considered, but teaching of International and Financial Accounting are of particular interest. Both entry-level and senior-level appointments will be considered. The Accounting Department is accredited by AACSB-International. Among the programs offered and supported by the Accounting Department are the B.S.B.A. with an Accounting major, the M.S.A., the M.S.T., joint B.S.B.A./M.S.A. and B.S.B.A./M.S.T. programs, and the M.B.A. with an Accounting concentration. The successful candidate will be strongly committed to being an excellent teacher and a productive researcher and scholar. A doctoral degree (or ABD) is required and professional qualification is preferred. Salary is competitive. Submit *curriculum vitae,* names of three references, and a letter outlining teaching and research interests to Professor Morris McInnes, Chairperson, Accounting Department, Frank Sawyer School of Management, Suffolk University, 8 Ashburton Place, Beacon Hill, Boston, MA 02108-2770. Suffolk University is an Equal Opportunity Employer; applications from minority candidates are encouraged.

331 WASHINGTON AND LEE UNIVERSITY, Department of Accounting, Williams School of Commerce, Economics and Politics, invites applications for a tenure-track Assistant Professorship in Accounting beginning in Fall 2003. Applicants should have or be close to completing a doctorate in Accounting. Primary teaching responsibilities will be in Financial Accounting. Evidence or promise of high-quality teaching and scholarship is expected. Salary will be competitive. The Williams School Business program is exclusively undergraduate and has been accredited by the AACSB since 1927. Washington and Lee University is consistently ranked among the best national liberal arts colleges in *U.S. News & World Report* and is the sixth-oldest institution of higher learning in the United States. Send resume to Professor Kevin Green, Head, Department of Accounting, Washington and Lee University, Lexington, Virginia 24450-0303; Email: greenk@wlu.edu. Professor Green and colleagues will interview applicants at the AAA Annual Meeting in San Antonio in August.

332 SEATTLE PACIFIC UNIVERSITY invites applications for an Assistant Professor or Associate Professor tenure-track position for Managerial/Cost Accounting available September 2002 or 2003. Ph.D. in Accounting is required. Successful candidate should have an established record as an effective teacher and a demonstrated capacity to conduct publishable research. Must be capable of teaching at both the graduate and undergraduate levels. Professional certification and experience desired. SPU's School of Business and Economics has approximately 200 undergraduate majors and 175 graduate students and is AACSB-accredited. Salary to be negotiated based on academic record and prior experience. Send letter and full *curriculum vitae* to Jeffrey Van Duzer, Dean, School of Business and Economics, Seattle Pacific University, 3307 3rd Avenue West, Seattle, WA 98119; Phone: (206) 281-2992; Email: ramyers@spu.edu. For more information about School of Bus. and Econ. access our home page at http://www.spu.edu/sbe. Successful candidates must be compatible with SPU's ecumenical Christian mission. Women and ethnic minorities are especially encouraged to apply.

333 UNIVERSITY OF NORTH TEXAS invites applications for a tenure-track or visiting position beginning Fall 2002, Spring 2003, or Fall 2003 at the Assistant rank from individuals with a primary teaching/research interest in the Tax area. Doctorate in Accounting or completion of the requirements by employment date is required. The University of North Texas, located in the Dallas-Fort Worth Metroplex, is a metropolitan research university with an enrollment in excess of 28,000 students. Accounting programs are offered at the undergraduate, master's, and doctoral levels. Applications will be considered until the position is filled. Candidates should submit a letter of application, *curriculum vitae*, defended dissertation proposal or published article, and the names, addresses, and telephone numbers of four references to Chair, Department of Accounting Search Committee, College of Business Administration, University of North Texas, PO Box 305219, Denton, TX 76203-5219. Candidates may use email to submit their application and required documents at mcqueenc@unt.edu. The University of North Texas is an Equal Opportunity Employer. Women and minorities are encouraged to apply.

334 JAMES MADISON UNIVERSITY invites applications for a tenure-track position at the Assistant Professor rank starting January or August 2003. The successful candidate must have an earned doctorate in Accounting from an AACSB-accredited institution or be near completion of dissertation. Business experience and professional certification are highly desirable. Applicants should be committed to excellence in teaching and scholarly and professional research. Primary teaching interests are Advanced Accounting or Cost. James Madison University, a comprehensive institution of 15,000 students, is located in Virginia's beautiful Shenandoah Valley within two hours of Washington, D.C. and Richmond. The Accounting program includes a Master's program and a concentration in Accounting Information Systems. Send resume/*curriculum vitae* to Dr. M. Cathy Sullivan, Director, School of Accounting, MSC 0203, James Madison University, Harrisonburg, VA 22807. JMU is an Equal Opportunity, Affirmative Action Equal Access Employer and especially encourages applications from minorities, women, and persons with disabilities.

335 SAINT LOUIS UNIVERSITY, a Catholic Jesuit institution dedicated to education, research, and healthcare, invites applications for a tenure-track position in Accounting at the Assistant/Associate Professor level beginning Fall 2003. Duties include commitment to teaching excellence; quality research and publication; service to the Department and University. Preference will be given to candidates specializing in Auditing and/or AIS. Ph.D. or near completion is required. Initial review of applications will begin September 1, 2002. Applicants are strongly encouraged to call Dr. John McGowan and arrange an interview at the AAA Annual Meeting in San Antonio. To assure full consideration, please send a letter of application and *curriculum vitae* by September 1, 2002 to Dr. McGowan, Chairperson, Department of Accounting, Saint Louis University, 3674 Lindell Blvd., St. Louis, MO 63108; Phone: (314) 977-3828; Fax: (314) 977-2078; Cell: (314) 707-7225; Email: mcgowanjr@slu.edu. Saint Louis University is an Affirmative Action/Equal Opportunity Employer and encourages nominations and applications of women and minorities.

ANNOUNCEMENT FOR TENURE-TRACK AND VISITING FACULTY POSITIONS DEPARTMENT OF ACCOUNTING, TIPPIE COLLEGE OF BUSINESS, THE UNIVERSITY OF IOWA

The University of Iowa Department of Accounting invites applicants for tenure-track appointments beginning Fall 2003. We will consider candidates in all areas and ranks. We will also consider one- or two-year visiting appointments if we are unable to fill the anticipated position(s) with tenure-track faculty. Required qualifications include a demonstrated interest in and capacity to do research publishable in top-tier accounting journals, a high level of teaching competence, and an earned doctorate (entry-level candidates can be near completion). We offer a competitive compensation package. We will begin screening applicants at all ranks and in all areas starting in October 2002. To apply, please send a résumé, representative research output, evidence of teaching capabilities, and at least three recommendation letters to: The Recruiting Coordinator, Department of Accounting, Tippie College of Business, 108 PBB, Suite W252, The University of Iowa, Iowa City, IA 52242-1000.

The University of Iowa is an Affirmative Action, Equal Opportunity Employer.
Women, minorities and individuals with disabilities are encouraged to apply.

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The American Accounting Association was founded in 1916 as the American Association of University Instructors in Accounting. Its present name was adopted in 1936. The Association is a voluntary organization of persons interested in accounting education, research, and practice. Full membership entitles one to receive *Accounting Education News*, a selection of one, two, or all three of the Association-wide journals (*The Accounting Review*, *Accounting Horizons*, and *Issues in Accounting Education*) and the opportunity to purchase Association publications at a discount.

Membership in the Association is available at the following annual rates:

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Editorial correspondence and all manuscripts should be sent to Professor Terry Shevlin, Senior Editor, *The Accounting Review*, Department of Accounting, University of Washington, Box 353200, Seattle, WA 98195-3200.

The submission fee of $75.00 for members of the AAA or $100.00 for nonmembers must accompany manuscripts when submitted. Books for review should be sent to Jane L. Reimers, College of Business, Florida State University, Tallahassee, FL 32306-1042. Request for reprint permission, subscriptions, advertisements, and dues should be sent to American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399 • Telephone: (941) 921-7747 • Fax: (941) 923-4093 • Email: Office@aaahq.org. Notice of change of address should be mailed at least eight weeks in advance to ensure undelayed receipt of *The Accounting Review*.

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Using Electronic Data Interchange (EDI) to Improve the Efficiency of Accounting Transactions

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ABSTRACT: Electronic data interchange (EDI) is an information technology that standardizes the exchange of information between transacting parties. Using data from a major U.S. office furniture manufacturer that adopted EDI primarily to improve the efficiency of accounting transactions, we evaluate whether EDI reduces order-processing time (the time from sales order receipt to sales order scheduling) and whether this improvement is greater for more complex orders. Our measure of complexity reflects both the mix of different products the dealer orders as well as features and options the dealer selects for each product in the order. We find that EDI is associated with faster order processing, independent of complexity, and that EDI mitigates most of the negative effects of complexity on processing time. We also find that dealers learn to submit error-free orders to the manufacturer, and that previous errors provide feedback that helps dealers submit more accurate orders. However, we find only mixed evidence that order complexity impedes learning.

Keywords: *accounting information systems; complexity; learning; transaction costs.*

Data Availability: *All data are the property of the company. The authors are not permitted to redistribute the data.*

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I. INTRODUCTION

Electronic data interchange (EDI) is an information technology that standardizes the exchange of information between two parties to a transaction. Cushing and Romney (1994) classify EDI as a "transactional" accounting information system that reduces the cost of transactions between firms and provides timely information for decision making. Respondents to an AICPA survey judged EDI to be the most important information technology for the accounting profession, in terms of both organizational productivity and projected impact on the work of the professional accountant (Anonymous 1995).[1] However, there is little evidence that EDI improves the performance of administrative work (e.g., Borthick and Roth 1993).

This study uses data from a large, office furniture manufacturing firm to evaluate the performance effects of EDI. The firm adopted EDI primarily to improve the efficiency of accounting transactions with many of the licensed dealers who order its products (e.g., to expedite acknowledgment of customer orders and to reduce order and billing errors).[2] We evaluate whether EDI reduces order-processing time (the time from order receipt to order scheduling), and whether this improvement is greater for more complex orders. Our measure of complexity reflects both the mix of different products the dealer orders as well as features and options the dealer selects for each product in the order. Data limitations prevent us from determining whether EDI reduced the level of order errors as compared to conventional order submission methods. However, we are able to investigate whether dealers who adopt EDI submit fewer orders with errors over time and with experience (i.e., learning), whether feedback from the EDI system on the dealer's prior errors stimulates their learning, and whether order complexity impedes learning.

Research in accounting and operations management has documented that the production of complex products reduces manufacturing performance, measured as productivity, cost, and quality (Banker et al. 1990; Banker et al. 1995; Foster and Gupta 1990; Banker and Johnston 1993; Datar et al. 1993; Noreen and Soderstrom 1994; Anderson 1995, 2001; Ittner and MacDuffie 1995; MacDuffie et al. 1996; Ittner et al. 1997). Several studies also consider how technology and management practices affect the relation between product complexity and manufacturing performance. In their study of 70 automotive assembly plants, MacDuffie et al. (1996) find that differences in the sophistication of information technology and the extent to which the plants use advanced manufacturing practices account for differences in the effect of complexity on plant performance. Specifically, plants that employ "lean" manufacturing methods "seem to be more capable of minimizing the complexity penalty arising from higher product variety than traditional mass production plants" (MacDuffie et al. 1996, 364). This is consistent with Sakurai's (1990) description of Japanese manufacturers who pioneered advanced manufacturing methods to "overcome the high cost of producing lower volumes and a wider variety of products." In a study of three textile-manufacturing plants, Anderson (1995) finds that the plant where complexity is the norm has a lower marginal cost of complexity than the two plants with less complex product mixes. She posits that managers may "learn" to accommodate complexity, with the associated costs of complexity declining as learning takes place.

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[1] Tsay (1993, 20) describes the accounting applications of EDI: "An important task of accounting information systems is to support business operations. Source data automation allows an EDI-integrated accounting system to provide timely information that supports management decision making."

[2] We use the term "dealer" for the firm that orders products directly from the furniture manufacturer. The term "customer" refers to the end-user of the product. Typically, the customer places an order with the dealer.

In this study, we use data on more than 1.5 million orders submitted through either EDI or conventional methods to investigate whether information technology—in this case EDI—reduces the negative effects of complexity on administrative processes. We find strong support for the hypotheses that order complexity slows order processing and that, independent of order complexity, EDI is associated with faster order processing. We also find that the benefits of EDI, in terms of faster order-processing times, increase in order complexity. The benefit of EDI that increases with order complexity offsets most of the negative effect of order complexity, supporting the proposition that information technologies may render the notion of "complexity" obsolete. Unfortunately, when EDI orders contain errors, the time to correct the errors offsets most of the improvement associated with EDI submission. We find that dealers who adopt EDI learn to submit error-free orders over time and with experience, and that errors in prior periods provide feedback that improves current period performance. We find only mixed support for the hypothesis that order complexity impedes learning.

Our findings have several implications for accounting research and practice. First, research in the operations and the information technology literatures documents benefits of EDI in manufacturing (e.g., reduced inventory holding and spoilage, faster delivery). However, there is no evidence on how EDI affects its primary users—back office, accounting functions of the firm and its transaction partners. We provide evidence that EDI improves performance of administrative work in the adopting firm; however, evidence on how EDI affects the back office operations of transacting partners is still needed.

Second, evidence that order complexity impairs performance in an administrative setting (e.g., slower order completion by the furniture manufacturer and increased errors by dealers) is consistent with earlier evidence that product and process complexity impair manufacturing performance. Our findings suggest that research that focuses on the effect of these characteristics only on manufacturing efficiency underestimates the effects of product and process complexity on firm performance.

Third, evidence that performance improvements associated with EDI increase with order complexity has implications for investment justification. Our results indicate that in addition to making the traditional order-entry process more efficient, EDI also mitigates the negative effect of order complexity on order-entry time. The implication for accountants is that, as with advanced manufacturing technologies, information technologies that enhance flexibility and reduce economies of scale in administrative work may require different evaluation approaches than other capital investments.[3]

Finally, evidence that dealers learn, with time and experience, to submit error-free orders through EDI and that feedback about prior errors helps dealers learn, has implications for the design and implementation of new accounting information systems. Recent experimental research in accounting has explored the role of feedback on learning (e.g., Hirst et al. 1999). Our finding that experience and feedback on prior errors stimulates learning in the EDI task environment is consistent with theory.[4] An implication for accounting system designers is that feedback is an important feature that may reduce the payback period for information technology investments. Moreover, although the furniture manufacturer did not train its dealers to use the EDI system, the negative performance effects associated with errors suggests that dealer training may have further enhanced returns to this investment.

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[3] Kaplan (1986) and Sakurai (1990) discuss how traditional capital investment analysis may cause manufacturing firms to underinvest in technologies that enhance flexibility and reduce economies of scale. Miller and O'Leary (2002) provide a detailed case study of how one firm replaced an obsolete approach to capital investment evaluation in recognition of these problems.

[4] Fine (1986) and Li and Rajagopalan (1997) discuss the theory of how quality failures influence the traditional economic model of the learning curve. Libby (1981) reviews the psychology and accounting literatures on how cues, such as those provided by EDI data entry forms, and feedback influence individual learning in a variety of task environments.

The paper is organized in six sections. In Section II, we review the literatures on the performance consequences of product and process complexity and of information technology investments and develop the research hypotheses. Section III describes the field research setting. We develop our empirical models for testing the hypotheses in Section IV, and present the results of the analysis in Section V. Section VI concludes with a brief summary.

II. LITERATURE REVIEW AND RESEARCH HYPOTHESES

Literature Review

Accounting researchers have proposed that overhead costs are more related to the transactions caused by increasingly complex product mixes and varied production processes than to the volume of production (Miller and Vollmann 1985; Johnson and Kaplan 1987). In general, empirical management accounting research finds that overhead costs and operational performance (e.g., quality, productivity) vary both with the complexity of process transactions (activities) and with production volume (Banker et al. 1990; Banker et al. 1995; Banker and Johnston 1993; Datar et al. 1993; Anderson 1995, 2001; Ittner and MacDuffie 1995; and Ittner et al. 1997, all provide supporting evidence; Foster and Gupta 1990; Noreen and Soderstrom 1994; Maher and Marais 1998, all offer contradictory evidence). However, with the exception of studies of airline costs (Banker and Johnston 1993) and hospital costs (Noreen and Soderstrom 1994; Maher and Marais 1998), empirical research has focused on overhead that is incurred in the factory and allocated to products as "cost of goods sold." As Johnson and Kaplan (1987, 244) confess:

> [W]e have been as guilty as conventional product cost systems in focusing narrowly on costs incurred only in the factory. Manufacturing costs may be important, but they are only a portion of the total costs of producing a product and delivering it to a customer. Many costs are incurred "below the line" (the gross margin line), particularly marketing, distribution, and service expenses.

In this paper, we evaluate the effect of complexity on administrative work (order processing) rather than manufacturing work.

Researchers have argued that advanced manufacturing technologies—including information technologies (IT) such as computer-integrated manufacturing and other factory automation—reduce the marginal cost of complexity in manufacturing (Sakurai 1990; MacDuffie et al. 1996). Discussing mechanisms through which IT increases productivity, King (1998, 70) argues that it is:

> not just increases in computing speeds and capacities or decreases in computing costs, but new ways of using computers....Among the important drivers are the integrated philosophies of...enterprise systems, mass customization, [and] integrated logistics.

Information technology often enhances customer value in ways that are not captured by conventional performance measures (e.g., increased product variety, improved delivery time and personalized service) (Brynjolfsson 1993, 1996). These observations echo assertions in the literature (Piore and Sabel 1984; Abernathy and Clark 1985; Pine 1993; Brynjolfsson 1996) that IT reduces the cost of complexity and negates the competitive advantage of economies of scale. However, we know of no empirical evidence on the proposition that complexity affects administrative work or that IT reduces the marginal effect of complexity on performance in administrative functions. In this paper we analyze how the introduction of one information technology, EDI, affects the relation between complexity and performance.

EDI technology allows business partners with recurring transactions to establish secure computer-to-computer exchange of data in standard formats. Although some have described EDI as the precursor to e-commerce (e.g., the use of the Internet to advertise, transact sales, and collect customer data), EDI differs from e-commerce in its closed community of predefined trading relationships. According to a 1995 study by the U.S. Department of Labor and the Internal Revenue Service, about 40,000 firms use EDI (cited in Socka 1996). EDI modifies traditional accounting information systems and alters the work of accountants (Tsay 1993; Clolery 1994; Cushing and Romney 1994; Bellone 1997). Perhaps more important, as an interorganizational system it is at the forefront of an important emerging area in accounting research—accounting across the boundaries of firms. As Kinney (2001) commented on the future of accounting research:

> We need to consider updating the accounting model to...measure and report on entities with "cloudy boundaries."...[h]ow should we account for an entity whose "inventory" is owned by another party and whose "fixed assets" are owned by a trading partner?

Hopwood (1996), Scapens and Bromwich (2001), and Anderson and Sedatole (forthcoming) extend Kinney's remarks, arguing that the need for new management accounting practices in the "extended enterprise" is equally compelling, and presenting evidence that firms are responding to this need.[5]

The operations management and the information systems literatures yield consistent but incomplete, evidence on the benefits of EDI investments. Weill (1992) finds that the firm that is at the hub of the EDI network enjoys increased productivity and profits. Mukhopadhyay et al. (1995) estimate the dollar value of improved information exchanges between Chrysler Company and its suppliers as approximately $100 per vehicle, or $220 million for Chrysler. They find that EDI is associated with higher inventory turnover and lower obsolete inventory write-offs and premium shipping costs. Turning to effects on suppliers that transact with the hub firm, Kekre and Mukhopadhyay (1992) find in a study of a steel manufacturer and 65 of its suppliers that suppliers that adopt EDI maintain less inventory and earn higher subjective quality ratings and overall performance ratings from the steel manufacturer than do suppliers that use manual processes. Similarly, Srinivasan et al. (1994) conclude that integrated EDI systems reduce Chrysler's suppliers' shipping errors.

Although these studies find that the more timely, accurate information that EDI provides yields improvements in *manufacturing* performance, they are silent on the question that arises in the IT literature: Does EDI improve the *administrative* work of document processing? Mukhopadhyay et al. (1995) add $5 per EDI transaction to their empirical estimates of operational improvements (or $40 of the claimed $100 per vehicle savings associated with EDI) to reflect savings that managers at Chrysler believe they obtain with reduced document-processing work. Riggins and Mukhopadhyay (1994) find faster processing time for managers and buyers when buyers send orders to suppliers via an EDI automated vendor system vs. traditional methods. However, it is difficult to draw a general conclusion that EDI improves performance because, as Kauffman (1988) finds, selective use of technology (e.g., some suppliers use EDI and others do not) tends to generate selection bias between transactions that do and do not use the technology. For example, Kaufmann (1988) found that customers with complex transactions are more likely to use bank tellers, whereas those with simpler transactions use ATMs. Thus, it is unclear whether EDI improves performance independent of the complexity of the orders transmitted, or whether EDI-related improvements increase with order complexity, or both. This distinction is essential to understanding whether EDI can potentially render notions of complexity obsolete by mitigating the effect of complexity on the performance of order-entry work.

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[5] Accounting research that investigates accounting across firm boundaries includes Otley (1994), Gietzmann (1996), Seal et al. (1999), Anderson et al. (2000), Van der Meer-Kooistra and Vosselman (2000), Groot and Merchant (2000), Baiman et al. (2001), Tomkins (2001), Baiman and Rajan (2002), and Kulp (2002).

Order Complexity and Order-Processing Performance

We consider two aspects of order complexity. *Product* complexity refers to the detail required to describe a particular order at the time of order entry. This includes both the mix of products the dealer orders as well as the detailed specification of options and features for each product in the order. For EDI orders, it is primarily dealers who experience product complexity when they enter a detailed order into the electronic order system. For orders that arrive through conventional channels (e.g., mail or fax), both the dealer, who completes the paper order form, and the office furniture manufacturer's (hereafter, OFM) employees, who enter the detailed order into the electronic order system, experience product complexity. *Process* complexity, on the other hand, refers to alternative administrative paths that some orders must follow *after* they are received by OFM and before they are considered complete.[6] OFM experiences process complexity primarily as additional manual work that slows completion of an order. Dealers experience process complexity only to the extent that for some processes it is their responsibility to indicate that OFM must perform additional processes on the orders they submit. Thus, for a dealer, process complexity is similar to product complexity—an attribute or feature of the product that they must specify on the order. The need to process an order through additional manual workstations is unrelated to whether the order arrives through EDI or conventional channels. As a result, we hypothesize that for OFM, EDI reduces the time for order entry of all orders, and that this reduction increases in product complexity. We do not hypothesize that EDI alters the relation between performance and process complexity for OFM, because EDI does not alter an order's path to completion. Thus, we test the following hypotheses:

H1a: Both product and process complexity reduce administrative performance.

H1b: Transactions that arrive through EDI channels (without error) are completed with better performance than are orders that arrive through conventional order channels, independent of order complexity.

H1c: The performance improvements associated with the use of EDI channels (without error) are greater for transactions with greater product complexity as compared to conventional order channels.

We test H1a to confirm that order complexity impairs performance. We test H1b to determine whether EDI improves transactions processing.[7] Even if the benefits of EDI were confined narrowly to eliminating order entry at OFM (because dealers enter orders directly into a common electronic order-entry system rather than submitting paper orders), we would expect the data to support this hypothesis. We test H1c to determine whether the benefits of EDI increase in proportion to the (product) complexity of the orders that OFM processes. Evidence of this relation would support claims that information technology such as EDI significantly reduces the extent to which complexity impairs performance. We refer to H1a–H1c together as H1, the "Direct Effect Hypothesis."

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[6] A completed order represents OFM's acknowledged commitment to produce and deliver on a specified date the products that a dealer orders. Completed orders are transmitted electronically to OFM's manufacturing group.

[7] One problem with estimating the effects of firm practices on performance is that firms choose to adopt a new practice endogenously. If all firms are in equilibrium, then we should observe no difference between the performance of adopters and non-adopters. However, in this paper, we define "performance" more narrowly. Because we do not have data on the cost of adoption or the financial benefits of adoption, we cannot assess whether adoption was optimal. We therefore limit our analysis to the question of operational performance improvements.

EDI and Order Errors

Another reason OFM adopted EDI was to reduce billing and order errors between itself and its dealers. If order complexity reduces the efficiency with which OFM completes orders, then it is also likely to reduce the performance of dealers submitting orders.[8] We do not have access to OFM's dealers or to information on the efficiency effects of EDI at the dealer level. We also do not have data on errors in orders submitted through conventional order channels that would permit us to compare the incidence of errors in EDI *vis-à-vis* conventional channels. However, we do have data on errors in orders that dealers submitted through EDI channels and, by relating these orders to the dealers who submitted them, we can investigate whether dealers who adopt EDI learn to submit error-free orders. Previous research has identified errors in the use of EDI as an important explanation of failures to realize anticipated benefits of the technology. Riggins and Mukhopadhyay (1994) find that suppliers are unlikely to support EDI implementation absent a subsidy or coercive threat from the EDI adopter, and once suppliers decide to adopt EDI, they have little motivation to master the technology.

The data also permit us to explore correlates of learning. Jovanovic and Nyarko (1995) argue that task complexity reduces the speed of learning. Anderson (1995) reports weak evidence that experience in producing a complex product mix is associated with lower marginal costs of complexity. Thus, we investigate whether order complexity impairs learning. The quality literature suggests prior-period errors also affect learning. Fine (1986) modifies the traditional approach to measuring learning as cumulative output by including only cumulative *good-quality* output. Kini (in an unpublished dissertation cited in Li and Rajagopalan [1997]) refines this further, suggesting that good- and bad-quality output influence learning in different ways. Li and Rajagopalan (1997) provide empirical evidence that bad-quality output increases learning, and they conclude that defective units are statistically more significant than good output in explaining learning curves. These models are particularly appropriate in an EDI context because, in contrast to conventional order submission methods, EDI generates an electronic report for the dealer on all changes required to make an EDI order comply with OFM requirements—changes that may or may not have required communication with the dealer. Thus, EDI supports more feedback and error diagnosis than is available for orders placed through conventional channels.

In our setting, dealers repeatedly use EDI to transmit information of varying complexity. There is little motivation to obtain training in the technology and learning is strictly by

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[8] Typically a large firm adopts EDI and encourages smaller transacting partners (at times with the threat of lost business) to become EDI-compliant. Although EDI is associated with benefits for the adopting firm, anecdotal evidence on benefits to transacting partners is less encouraging (e.g., Zeidner 1994; Socka 1996). Riggins and Mukhopadhyay (1994) explore asymmetries in performance improvements between an EDI adopter and its transactions partners and conclude that reduced document-processing times are concentrated at the EDI adopter. A survey by Stern and Kaufmann (1985) reveals that trading partners who adopt EDI believe that the initiating firm gains more from the investment. Although OFM did not require EDI adoption, and we did not detect coercion in our field investigations, OFM also did not provide or subsidize dealer training in the technology.

"doing"; however, errors from prior periods may facilitate dealer learning. Appl arguments above to our setting, we hypothesize:

> **H2a:** Dealer errors in EDI order submission reduce the performance benefits to OFM.
>
> **H2b:** Dealer learning and feedback on prior period errors lead to fewer deal in EDI order submission.
>
> **H2c:** Order complexity impairs the rate of dealer learning.

We test H2a to confirm that errors in EDI order submission reduce administrative ance at OFM. We test H2b and H2c to determine whether EDI is subject to dealer effects, and if so, whether order complexity impairs this learning. Together these hy provide evidence on indirect effects of complexity and the EDI technology on performance. We refer to H2b and H2c collectively as H2, the "Indirect Effect Hyp

III. THE RESEARCH SETTING

OFM provides data for the study. OFM offers more than 65,000 standard pro more than 70 paint colors, dozens of laminate surfaces, and hundreds of upholstery Independent dealers help end-users (customers) plan office configurations, submit and receive and install small orders. OFM and many of its more than 500 inde dealers have adopted EDI.

The field research setting is OFM's central Sales Service Department (SSD). the point of contact between OFM and its dealers. This group receives all orders them (in a process called "order editing") into the information system that contr duction scheduling and dealer billing, manually processes nonroutine orders, and a edges planned shipments to dealers. Figure 1 depicts the process flow from the OFM receives the order (mail stamp date) to the date that OFM commits to pro order (acknowledgment date). The Appendix describes the paths shown in Figure cesses shown in the shaded area of Figure 1 correspond to activities that are the sibility of SSD; processes outside this area are the responsibility of other grou difference between the mail stamp date and the acknowledgment date is the tot completion time. OFM terms this difference "acknowledgment delay," closely mo and uses it to evaluate SSD's performance.[9]

Inputs to the order-entry process are labor and capital. Throughout the perio capacity was stable, with approximately 87 nonsupervisory salaried employees. O used capacity throughout the period; the order-entry queue was never empty. E head-count varied only slightly (a range of 82–90) and variation reflects short-liv syncratic circumstances rather than systematic efforts to alter the number of serve to 1993, OFM adopted EDI to transmit data (e.g., customer orders, invoices, a dealer communications) to and from its dealers. Although it expected EDI to enha productivity by transferring key-entry tasks to dealers, the primary reason for adopt was to improve the efficiency of accounting transactions (e.g., to expedite acknowl of customer orders, to reduce the length of the billing cycle, and to reduce order an

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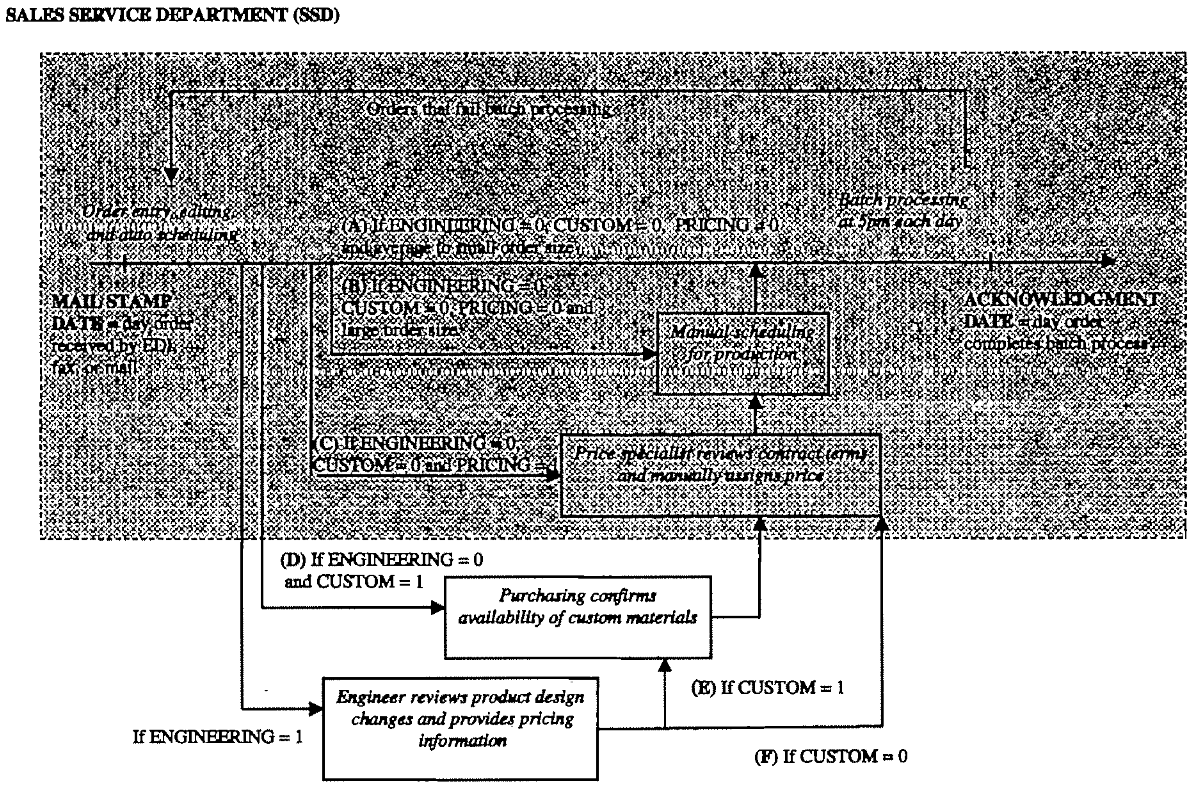
[9] Managers of SSD do not use cost information for performance measurement or control purposes, b department's costs do not change from period to period. They limit their analysis of acknowledgment to studying simple trends and to partitioning orders into priority classes (described in the Appendix) to whether the orders met acknowledgment delay goals.

FIGURE 1
OFM's Order-Entry Process


SALES SERVICE DEPARTMENT (SSD)
Orders that fail batch processing
Order entry, editing, and auto scheduling
(A) If ENGINEERING = 0, CUSTOM = 0, PRICING = 0 and average to small order size
Batch processing at 5pm each day
MAIL STAMP DATE = day order received by EDI, fax, or mail
(B) If ENGINEERING = 0, CUSTOM = 0, PRICING = 0 and large order size
Manual scheduling for production
ACKNOWLEDGMENT DATE = day order completes batch process
(C) If ENGINEERING = 0, CUSTOM = 0 and PRICING = 1
Price specialist reviews contract terms and manually assigns price
(D) If ENGINEERING = 0 and CUSTOM = 1
Purchasing confirms availability of custom materials
If ENGINEERING = 1
Engineer reviews product design changes and provides pricing information
(E) If CUSTOM = 1
(F) If CUSTOM = 0

Definition of partitioning indicator variables when equal to 1:

CUSTOM = pseudo-orders with at least one custom material;
ENGINEERING = pseudo-orders that require engineering design changes;
PRICING = pseudo-orders that require manual pricing;
ORDERSIZE = psuedo-orders in the top quartile for all orders of net discounted value;

errors). Delayed acknowledgments undermine OFM's strategies of quick-shipping standard products and of filling other orders more rapidly than its competitors. Order errors and resulting incomplete or incorrect shipments cause misuse of production capacity and increased transportation, inventory holding, and rework costs, and also cause customer dissatisfaction. EDI implementation by OFM's dealers was gradual and voluntary, with OFM giving priority to dealers with the highest sales volume.

In theory, EDI orders are transmitted electronically to the order-processing system and require little work by an order editor (whose tasks are described in the Appendix), whereas paper orders require painstaking manual data entry. In practice, both EDI and paper orders require significant intervention by the order editor to correct dealer errors and to respond to unusual customer demands reflected in the dealer's order.[10] One form of order editor intervention is splitting orders into two or more "pseudo-orders." The order editor's objective is to minimize acknowledgment delays, subject to constraints on the sequence in which he edits the orders (i.e., queue priorities). Order splitting, described in the Appendix, helps achieve this objective by creating two or more orders that are more easily scheduled.

Our analysis includes all pseudo-orders with an acknowledgment delay of between one and 20 days and we focus on the SSD's performance in processing these pseudo-orders. Therefore, for the remainder of the paper we use the term "order" in place of "pseudo-order." We exclude orders with acknowledgment delays greater than 20 days because they represent problems that require atypical processing. During the three-year period, SSD processed 1,619,396 orders, of which 1,532,354 orders satisfy the acknowledgment delay requirement above.[11]

Table 1 provides descriptive statistics on the flow of the 1.5 million orders (of which 28 percent were from split orders) over each process path (described in detail in the Appendix) and associated acknowledgment delays for the orders included in our subsequent analyses. The average order acknowledgement delay is just under three days, although it is higher for orders that require pricing quotes, engineering quotes, or include some type of customization. The results in Table 1 show that most orders require some type of manual processing (pricing quotes, custom orders quotes, or engineering quotes). The Appendix describes the process flow and relates each path to the associated descriptive statistics. Finally, data in Table 1 show that dealers submit most orders (86 percent) through EDI and about 20 percent of these submissions have errors.

Managers of SSD have generally been satisfied with the department's performance and have no plans to alter what has been a stable complement of employees; however, they are quick to discuss two countervailing forces on performance during the period of study. First, they believe that as orders became more complex (given OFM's strategy of proliferating products and variations), service performance suffered. Order complexity led to a larger menu of products and options with which order editors must be familiar—what we term "product complexity"—as well as in manual processes that induce "process complexity" in response to custom orders. Second, although SSD's managers believe that EDI offsets some of these adverse effects on service performance, they caution that dealer errors in using the EDI technology have been costly to OFM. In sum, managers of SSD found our hypotheses plausible, although they had no evidence to support their intuition.

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[10] An order editor may detect dealer errors that include omissions, such as failure to specify a color or fabric for an upholstered chair, failure to select from a list of options, or selection of options that are inadmissible for a particular product. The order editor does not detect errors that are logically consistent with the product offering (e.g., designation of each part of a chair in a different but admissible color).

[11] We repeated the analyses reported below on the full sample with no qualitative effect on the results.

TABLE 1
Average Acknowledgment Delays and Number of Pseudo-Orders OFM Acknowledged during March 1993–February 1996

Full Sample and Partitions Corresponding to Process Flows in Figure 1	*Figure 1 Process Flow*	*Number of Pseudo-Orders*	*Acknowledgment Delay (Days)*			
			First Quartile	*Median*	*Third Quartile*	*Mean*
Split Orders						
CUSTOM, ENGINEERING, PRICING, ORDERSIZE = 0	A	25,380	1	2	3	2.8
CUSTOM, ENGINEERING, PRICING = 0, ORDERSIZE = 1	B	9,200	1	2	3	2.9
CUSTOM, ENGINEERING = 0, PRICING = 1	C	318,585	2	3	5	3.8
ENGINEERING = 0, CUSTOM = 1	D	19,427	4	6	9	6.7
ENGINEERING = 1, CUSTOM = 1	E	2,241	7	11	15	11.1
ENGINEERING = 1, CUSTOM = 0	F	57,626	5	7	11	8.5
EDIGOOD = 1		261,820	2	3	4	3.8
EDIBAD = 1		107,360	3	5	8	7.5
All Split Orders[a]		432,459	2	3	6	4.5
Unsplit Orders						
CUSTOM, ENGINEERING, PRICING, ORDERSIZE = 0	A	310,645	1	1	2	1.6
CUSTOM, ENGINEERING, PRICING = 0, ORDERSIZE = 1	B	26,234	1	1	2	1.9
CUSTOM, ENGINEERING = 0, PRICING = 1	C	728,016	2	2	3	2.4
ENGINEERING = 0, CUSTOM = 1	D	10,631	3	4	6	5.4
ENGINEERING = 1, CUSTOM = 1	E	982	4	7	11	8.0
ENGINEERING = 1, CUSTOM = 0	F	23,387	4	6	8	6.6
EDIGOOD = 1		780,128	1	2	3	2.1
EDIBAD = 1		165,926	1	2	3	3.0
All Unsplit Orders[a]		1,099,895	1	2	3	2.3
All Orders		1,532,354	1	2	3	2.9

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[a] Sample partitions with process flows labeled A–F are exclusive, together reflecting all orders. Figure 1 does not distinguish between split and unsplit orders, between EDI and non-EDI orders, or between EDI orders that contain errors and that those that do not.

Definition of variables:

Acknowledgement Delay = number of days from order receipt at OFM to the date OFM commits to produce the order.

Partitioning indicator variables (when equal to 1):

Split = a large, single order that a dealer submitted to OFM, which an order editor separated into two or more smaller pseudo-orders;
CUSTOM = pseudo-orders with at least one custom material;
ENGINEERING = pseudo-orders that require engineering design changes;
PRICING = pseudo-orders that require manual pricing;
ORDERSIZE = pseudo-orders in the top quartile for all orders of net discounted value;
EDIGOOD = pseudo-orders dealers transmitted to OFM by EDI without error; and
EDIBAD = pseudo-orders dealers transmitted to OFM by EDI that contain errors.

IV. THE EMPIRICAL SPECIFICATION

The data are daily archival records for three fiscal years of all customer orders, and include prices, quantities, product specifications, and the dates on which OFM received, acknowledged, and anticipated shipping the orders. The sales and marketing group of OFM maintains these records and uses them to analyze market trends and customer demand. The group does *not* use these records in production; consequently, the records do not include information about a product's bill of materials, manufacturing process routing, manufacturing performance, or actual shipping date.[12]

The Direct Effect Hypothesis

Our Direct Effect Hypothesis, H1, implies the following:

1. OFM acknowledges less complex orders faster than more complex orders, for a given EDI/non-EDI submission channel;
2. OFM acknowledges error-free orders that dealers submit through EDI channels faster than orders dealers submit through normal channels, for a given level of complexity;
3. For a given *increase* in the level of complexity, OFM acknowledges error-free orders dealers submit through EDI channels faster than orders dealers submit through normal channels.

We make no directional hypotheses for EDI orders that dealers submit with errors, because the net effect of error correction is unclear.

To test the Direct Effect Hypothesis, we first calculate whether OFM acknowledged the order early or late, relative to goals for its priority class. OFM assigns an incoming order one of five priority ranks. As we explain in the Appendix, order priority reflects the anticipated size of the outgoing shipment of manufactured goods as well as OFM strategies for expediting orders of certain standard products. It is not causally related to whether the order arrives by EDI.[13]

For each order, we calculate the difference between the acknowledgment date and the mail stamp date (adjusted for nonbusiness days—weekends and holidays). We label this variable DELAY. For each order, we then subtract the priority-class target completion time (two, three, five, or seven days) from DELAY to determine the extent to which OFM acknowledged an order early or late relative to its defined goal. We label this variable DEVIATION. That is:

$$\text{DELAY} = \text{Acknowledgment Date} - \text{Mail Stamp Date}$$

and

$$\text{DEVIATION} = \text{DELAY} - \text{Priority Class}.$$

Negative values of DEVIATION thus indicate early completion relative to the target, and positive values indicate orders completed late relative to the target.

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[12] A record in the database corresponds to one pseudo-order as defined in the Appendix.

[13] The database does not record the actual priority assigned to incoming orders; however, reasonable proxies exist for three of the five ranks. There is no mechanism for distinguishing the last two categories; however, interviews with sales service representatives indicate that they rarely use one of these two, and that the other category is the base case. Consequently, although two ranks are unidentifiable, after we identify the three priority classes for which proxies are available, we assume that remaining orders are the base case priority, including the few shipments in the category that is rarely used.

We evaluate the Direct Effect Hypothesis by examining the following general relation:

DEVIATION = f(ProductComplexity, ProcessComplexity, EDIGOOD, EDIBAD).

For a given order, we assign the variable EDIGOOD a value of 1 if the dealer submitted an order through EDI without error, and 0 otherwise. Similarly, we set EDIBAD equal to 1 if the dealer submitted an order through EDI with error, and 0 otherwise. ProductComplexity and ProcessComplexity represent measures of order complexity for an individual order.

Several order characteristics contribute to product and process complexity. We consider total lines in the order, finish codes, unique finish codes, price quotes, engineering quotes, custom quotes, and average item cost. Each order consists of one or more lines describing the product. The first line of an order represents the highest level of specification for a product (e.g., chair). Subsequent lines identify product options (e.g., optional chair arms) or, for modular products, the desired product configuration (e.g., one of three available chair pedestals). Each line may include up to five four-digit codes that designate finishes for the product or part to which the line refers. Finishes include paint colors, laminates, and fabrics. A special four-digit finish code indicates a custom material. Because many orders specify identical colors (finishes) for various parts, the number of *unique* finish codes is generally fewer than the number of finish codes for the order. As we discuss in the Appendix, orders that require price quotes, use custom materials, or require special engineering quotes go through processes outside the normal acknowledgment process, which contributes to the process complexity of the order. Finally, we include average item cost as a potential measure of product complexity. This variable reflects the fact that certain complex products (e.g., modular items) require fewer lines to define than does a collection of simpler items (e.g., desk and chairs), but still requires the same amount of work by the order editor.

We use principal components to identify complexity constructs. Analyzing the seven potential measures of complexity, we find two components with eigenvalues greater than 1, and this structure retains about 48 percent of the variance in the set of variables. The first component loads on order lines, finish codes, unique finish codes, and average item cost. We label this component "product complexity." The second component loads on price quotes, engineering quotes, and custom quotes. We label this component "process complexity." The Cronbach alpha for the four product complexity measures is 0.68, indicating a reasonable level of reliability. The Cronbach alpha for the three process complexity measures is unacceptably low; therefore, in the analysis, we use the three process complexity measures individually.[14] Rather than use the factor scores, we compute a product complexity measure by equally weighting values of the standardized measures for the four individual product complexity measures (Grice and Harris 1998).[15]

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[14] We also ran the analysis using the process complexity construct in place of the individual measures, with no qualitative changes in the results.

[15] The correlation between the factor score and our computed product complexity measure is 0.96. The results in Grice and Harris (1998) suggest this approach retains many of the desirable features of regression-based factor scores while being less subject to the problems associated with using the same data to estimate the coefficients and to compute the factor scores.

To allow for idiosyncratic events on the individual days of the sample period, the regression includes indicator variables for 746 of the 747 days on which OFM acknowledged orders. The empirical specification for the regression equation we use to test the Direct Effect Hypothesis is:[16]

$$\begin{aligned}\text{DEVIATION} = {} & \alpha + \gamma_1\text{Day}_1 + \cdots + \gamma_{746}\text{Day}_{746} + \beta_1\text{ProductComplexity} + \beta_2\text{PriceQuotes} \\ & + \beta_3\text{CustomQuotes} + \beta_4\text{EngineeringQuotes} + \beta_5\text{EDIGOOD} \\ & + \beta_6\text{ProductComplexity} \times \text{EDIGOOD} + \beta_7\text{EDIBAD} \\ & + \beta_8\text{ProductComplexity} \times \text{EDIBAD} + \varepsilon.\end{aligned}$$

The estimated coefficients $\beta_1 \ldots \beta_4$ measure the effect of complexity on performance, allowing a test of H1a. The estimated coefficient β_5 is the basis for testing H1b, the effect of EDI on performance independent of complexity. The coefficient β_6 is the basis for testing H1c, the mitigating effect of EDI on product complexity. Estimated coefficients β_5, β_6, β_7, and β_8 are the basis for testing H2a, that errors in EDI submission offset, at least in part, the benefits of EDI. Specifically, the hypothesis implies that and that $\beta_5 < \beta_7$ and that $\beta_6 < \beta_8$.

The Indirect Effect Hypothesis

We use data on the incidence of errors in orders that more than 300 dealers submitted through EDI to examine whether EDI adoption is associated with dealer learning, whether EDI-facilitated feedback on prior errors facilitates dealer learning, and whether order complexity impedes learning. Specifically, we compute the proportion, p, of EDI orders a dealer submits each month that are error-free.[17] We create this measure for each dealer who submits EDI orders in at least 10 of the 36 sample months. These monthly EDI orders must satisfy the condition $0 < p < 1$ (i.e., the dealer has submitted some, but not all, EDI orders with errors), because the dependent variable (described below) is undefined unless the condition is satisfied.[18]

We use two measures from the economics literature on learning curves to investigate factors associated with dealer errors in submitting EDI orders: elapsed time, an index reflecting the observation month (index value of 1–36 for the three years); and cumulative experience, measured as the cumulative volume of EDI orders dealers submit, where cumulative volume equals zero in the month immediately before the dealer first used EDI. Elapsed time is an appropriate measure of learning if OFM continually enhances the EDI system or if dealers become more proficient as a result of a series of activities other than order entry (e.g., training classes). Cumulative experience measures "learning-by-doing." Using cumulative errors that dealers made in the past, we examine whether feedback on their past errors reduces the incidence of errors in current orders (Li and Rajagopalan 1997).

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[16] We do not include an interaction term for process complexity and EDI submission because, given the acknowledgement process, there is nothing in the choice of submission channel that can affect the speed of the manual processes. The results we report below are not affected in terms of significance or sign if we include this interaction in the regression.

[17] We investigate the effect of dealer errors in EDI orders, but errors also arise with other submission methods. We have no data on the incidence of errors for other submission methods; consequently, we cannot determine whether errors occur with different frequency when dealers use EDI. Even if data for other order submission methods were available, it would be difficult to separate the technology-related effects from self-selection bias induced by the characteristics of dealers who adopt EDI.

[18] This condition resulted in a loss of two dealers.

We adapt methods that economists have used to detect dynamic scale economies, or what has been termed the "learning curve," to estimate the rate of improvement in order-entry accuracy for EDI-equipped dealers as a function of the two measures of learning (L).[19] Specifically, for each dealer, we estimate:

$$\log(p_t/(1 - p_t)) = \gamma_0 + \gamma_1 L_t + \gamma_2 CE_t + \varepsilon, \qquad (1)$$

where p is the proportion of EDI orders that are error-free, L_t is the measure of learning or experience (measured alternately as elapsed time and as cumulative volume of EDI orders submitted).[20] Consistent with H2b, CE_t is the *percentage* of cumulative EDI orders through month $t - 1$ that had errors. After estimating regression (1) for each dealer, we have a sample of learning rates ($\hat{\gamma}_1$) that we use in our test of the Indirect Effect Hypothesis.

To evaluate the Indirect Effect Hypothesis, we test two (related) hypotheses. The first is simply that dealer learning reduces errors in their EDI orders (which, given H2a, should improve OFM's efficiency and performance). That is, H2b predicts that both γ_1 and γ_2 will be positive. The final step is to evaluate the effect of complexity on dealer learning (H2c). We test the effect of complexity on learning by regressing the estimated learning rate, $\hat{\gamma}_{1j}$, for the jth dealer on the complexity constructs:

$$\hat{\gamma}_{1j} = f(\text{ProductComplexity, PriceQuotes, CustomQuotes, EngineeringQuotes}).$$

As with the Direct Effect Hypothesis, we measure product complexity using a construct based on an equally weighted index of order lines, finish codes, unique finish codes, and order value. We measure process complexity using the three process complexity variables—price quotes, custom quotes, and engineering quotes. Recall that process complexity reflects additional manual work that OFM must perform, regardless of whether the dealer submits the order through EDI or conventional channels. In general, process complexity is invisible to the dealer. However, these three forms of process complexity require the dealer to provide correct additional information about products. As a result, we hypothesize that the effect on learning will be similar to that of product complexity. Hypothesis 2c predicts that complexity impairs dealer learning so we expect both complexity constructs to be negatively related to the learning parameter, $\hat{\gamma}_1$.

V. RESULTS

The Direct Effect Hypothesis

Table 2 reports the correlations among the variables used in the analysis. All the correlations are significant, but they are generally less than 0.30. The positive univariate correlations between DEVIATION and the four complexity variables are consistent with H1a, the negative correlation between DEVIATION and EDIGOOD is consistent with H1b, and the positive correlation between DEVIATION and EDIBAD is consistent with H2a. We expect the interaction of product complexity and EDIGOOD to be negatively related to performance, yet it has a positive univariate correlation, contrary to H1c. In a

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[19] Berndt (1991, 66–80) provides an overview of the learning curve literature and discusses econometric issues in estimating rates of learning. Ghemawat (1986) summarizes estimated rates of learning from 97 empirical studies and concludes that manufacturing activities experience greater learning effects than service or administrative activities.

[20] We estimate the learning curves separately for the two learning proxies because of the high correlation between them ($\rho = 0.99$).

TABLE 2
Correlations among the Deviation between Actual and Expected Acknowledgement Delay, EDI Orders Submitted With and Without Error, Product Complexity, Process Complexity, and the Interaction of Error-Free EDI Orders and Product Complexity
(n = 1,532,354)

Variables	*Deviation*	*EDIGOOD*	*EDIBAD*	*Product Complexity*	*Price Quotes*	*Custom Quotes*	*Engineering Quotes*
EDIGOOD	−0.197						
EDIBAD	0.217	−0.679[a]					
Product Complexity	0.059	0.043	−0.040				
Price Quotes	0.151	0.095	−0.031	0.195			
Custom Quotes	0.187	−0.197	0.254	−0.027	0.035		
Engineering Quotes	0.440	−0.256	0.308	0.029	0.045	0.026	
Product Complexity × EDIGOOD	0.025	0.017	−0.012	0.799	0.126	−0.006	0.005

[a] EDIGOOD and EDIBAD are not perfectly (negatively) correlated because some orders are submitted through non-EDI channels.

Definition of variables:

Deviation = difference between actual acknowledgment delay and expected delay based on order priority;
EDIGOOD = percentage orders dealers transmitted by EDI with no errors;
EDIBAD = percentage orders dealers transmitted by EDI with errors;
Product Complexity = an equally weighted index of standardized values of lines per order, number of finish codes, number of unique finish codes, and average item cost;
Price Quotes = percentage of orders completed that require manual pricing;
Custom Quotes = percentage of orders completed that include at least one custom material; and
Engineering Quotes = percentage of orders completed that require manual verification of engineering designs.

univariate correlation, it is not clear whether the positive effect of complexity (increasing DEVIATION) or the negative effect of EDIGOOD (decreasing DEVIATION) will dominate. However, H1c examines the interaction between EDI and complexity, or the effect of EDI on performance for a given increase in complexity.

Turning to the multiple regression analysis, we report the results of estimating the association among acknowledgment delays, order complexity, and EDI use in Table 3.

TABLE 3

Regression Results of the Deviation between Actual and Expected Acknowledgment Delay on Product and Process Complexity, Good and Bad EDI Submissions, and the Interaction between Product Complexity and Good and Bad EDI Submissions

(n = 1,532,354)

$$\begin{aligned} DEVIATION = {} & \alpha + \gamma_1 Day_1 + \cdots + \gamma_{746} Day_{746} + \beta_1 ProductComplexity + \beta_2 PriceQuotes \\ & + \beta_3 CustomQuotes + \beta_4 EngineeringQuotes + \beta_5 EDIGOOD \\ & + \beta_6 ProductComplexity \times EDIGOOD + \beta_7 EDIBAD \\ & + \beta_8 ProductComplexity \times EDIBAD + \varepsilon. \end{aligned}$$

Variables	*Hypothesis*	*Predicted Sign of the Coefficient*	*Estimated Coefficient*	*t-value*
Intercept		?	−1.2663	−1.0
Daily Indicator Variables		?	Not Reported	
Product Complexity (β_1)	H1a	+	0.2718	43.5
Price Quotes (β_2)	H1a	+	0.3175	177.7
Custom Quotes (β_3)	H1a	+	0.3876	215.5
Engineering Quotes (β_4)	H1a	+	1.0219	556.0
EDIGOOD (β_5)	H1b	−	−0.3782	−73.8
Product Complexity × EDIGOOD[a] (β_6)	H1c	−	−0.2281	−33.0
EDIBAD (β_5)	H2a	?	0.0867	11.2
Product Complexity × EDIBAD[a] (β_8)	H2a	?	−0.0650	−10.7
Adjusted R^2			0.268	

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[a] The data reject the hypotheses that the coefficient on Product Complexity is equal to either the absolute value of the coefficient on Product Complexity × EDIGOOD or Product Complexity × EDIBAD at the $p < 0.001$ level.

Definition of variables:

Deviation = difference between actual acknowledgment delay and expected delay based on order priority;

Product Complexity = an equally weighted index of standardized values of lines per order, number of finish codes, number of unique finish codes, and average item cost;

Price Quotes = percentage of orders completed that require manual pricing;

Custom Quotes = percentage of orders completed that include at least one custom material;

Engineering Quotes = percentage of orders completed that require manual verification of engineering designs;

EDIGOOD = percentage orders dealers transmitted by EDI with no errors; and

EDIBAD = percentage orders dealers transmitted by EDI with errors.

Although not shown in Table 3, the daily indicator variables are generally not significant at the 10 percent level.[21] The results in Table 3 are consistent with H1a, that order complexity (both product and process complexity) is associated with significant acknowledgment delays. On average, an increase of one unit of product complexity increased acknowledgment delay by 0.27 days. A standard unit of process complexity increased delays by 0.32 to 1.02 days because process complexity often results in processing delays as the order goes through different channels.

The results indicate OFM acknowledged orders that dealers submitted without error via EDI on average about 0.38 days earlier than other orders. OFM acknowledged orders submitted using EDI, but with errors, on average about 0.09 days *later* than other orders.[22] These results suggest EDI improves acknowledgment time if dealers submit orders without error. Thus, the results are also consistent with H1b. The significantly negative coefficient on the interaction between error-free EDI submissions and product complexity (β_6) supports the hypothesis that EDI submissions without error can mitigate a portion of the delay associated with product complexity, H1c. The results in Table 3 suggest that OFM can eliminate most of the delay (0.23 of the 0.27 days) if dealers submit error-free orders via EDI. To test whether good EDI submissions completely offset the adverse effect of product complexity, we test whether the sum of the coefficients on product complexity and the interaction of complexity with good EDI submissions is equal to zero. This hypothesis (that $\beta_1 + \beta_6 = 0$) is rejected at the 0.001 level. Thus, good EDI submissions only partially offset the adverse effects of complexity.[23] Overall, the data support the Direct Effect Hypothesis.

The Indirect Effect Hypothesis

We can also use the results in Table 3 to test H2a, that EDI orders dealers submitted with error reduce the (performance) benefits OFM reaps from EDI. As noted above, submission of orders through EDI channels (with error) increases acknowledgment delay, independent of complexity. Hypothesis 2a suggests more, however. It implies that, first, orders submitted through EDI with error have longer acknowledgment delays than error-free EDI submissions, for a given level of complexity ($\beta_5 < \beta_7$) and, second, the mitigating effect of EDI for complex orders is lower for EDI orders submitted with errors ($\beta_6 < \beta_8$). The results in Table 3 support both implications; thus, the data support H2a.

Table 4, Panel A reports the results of the model designed to test H2b, namely, that dealer learning reduces EDI errors. We estimated the dealer-specific learning regression (1) for various samples of dealers, requiring different minimum numbers of EDI orders submitted in a month. To evaluate the effect of frequency of EDI ordering on the hypothesis, we separately analyzed dealers submitting a minimum of 2, 3, 5, 10, 20, or 30 orders, respectively. As shown in Table 4, Panel A, this resulted in samples of from 329 dealers

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[21] We computed variance inflation factors to investigate possible estimation problems due to collinearity in the data. Although there are no formal criteria for evaluating variance inflation factors, the largest variance inflation factor for the variables other than the daily indicator variables is 6.5. This is smaller than the threshold of 10 many econometricians consider to indicate serious collinearity (Kennedy 1992, 183). The variance inflation factors were higher for the daily indicator variables. We re-ran the regression without the daily indicator variables and there was no qualitative (and virtually no quantitative) change in the other results.

[22] We do not have data on errors associated with non-EDI submissions; it is possible that EDI improves performance even for orders submitted with errors.

[23] Although we did not hypothesize any effect that EDI submissions with error would have in mitigating product complexity, the results in Table 3 show that EDI submissions do offset some of the effect of product complexity ($\beta_8 < 0$). We can also reject the hypothesis that EDI submissions with error offset completely the effect of product complexity (the hypothesis that $\beta_1 + \beta_8 = 0$ can be rejected at the p = 0.001 level.)

TABLE 4

Analysis of the Relation between Dealers' Experience Using EDI, the Reduction of Dealers' Errors in EDI (Learning), and the Effect of Product and Process Complexity on the Rate of Dealer Learning

Panel A: Analysis of dealer errors in EDI submissions; Z-statistics from tests of the significance of time-based and experience-based learning in EDI order entry across the population of EDI-equipped dealers (H2b)[a]

$$log(p_t/(1 - p_t)) = \gamma_0 + \gamma_1 L_t + \gamma_2 CE_t + \varepsilon$$

		Time-Based Learning		*Experienced-Based Learning*	
Minimum Orders	*Number of Dealers*	$Z(\gamma_1)$	$Z(\gamma_2)$	$Z(\gamma_1)$	$Z(\gamma_2)$
2	329	0.66	4.50*	0.23	4.20*
3	328	1.83*	4.74*	1.28	4.40*
5	327	2.87*	5.25*	2.23*	4.80*
10	311	4.39*	5.73*	3.66*	5.11*
20	284	4.19*	5.83*	3.55*	5.21*
30	255	4.05*	5.83*	3.60*	5.45*

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* Significant at $p < 0.05$ (one-sided).

[a] For each dealer in the sample, we estimate Equation (1) to develop a sample of regression coefficients and their associated t-statistics. The Z-statistics test the hypothesis that the mean t-statistic is equal to 0 (Patell 1976). The number of EDI orders a dealer places in a month determines the dealer's inclusion in a particular dealer-specific sample. For example, if a dealer places four EDI orders in six months and 20 EDI orders in ten months, we include 16 months of data in the dealer-specific regression for the sample of dealers with at least three orders in a month, but only ten months of data in the dealer-specific regression for the sample of dealers with at least five EDI orders in a month.

Definition of variables:

p = the ratio of error-free EDI orders to the total number of EDI orders dealers submitted;

L = one of two measures of learning:

In the first case, L is an index that indicates the month (1–36) of the observation and β_1 is the rate of time-based learning;

In the second case, L is the cumulative number of EDI orders submitted and β_1 is the rate of experience-based learning; and

CE = CE for month t is the percentage of the dealer's orders from month 1 to t–1 that had errors.

(*Continued on next page*)

TABLE 4 (Continued)

Panel B: Regression estimates of the relation between the (1) rate of learning for dealers submitting EDI orders and (2) average product and process complexity (H2c)

$\hat{\beta}_{1j} = f(ProductComplexity, PriceQuotes, CustomQuotes, EngineeringQuotes)$

	Time-Based Coefficients				*Experience-Based Coefficients*			
		Process Complexity				*Process Complexity*		
Minimum Orders	*Product Complexity*	*Price Quotes*	*Custom Quotes*	*Engineering Quotes*	*Product Complexity*	*Price Quotes*	*Custom Quotes*	*Engineering Quotes*
2	−271.5	−159.6*	354.5*	109.2	−6.90	−7.81*	38.21*	1.97
3	−292.4	−142.4	333.7*	139.4	−6.77	−7.58*	38.1*	0.37
5	−245.0	−135.1*	339.1*	15.4	−7.54	−7.61*	38.85*	−4.25
10	−225.9	−133.8*	24.1	282.4*	−0.73	−3.34	2.54	1.06
20	−549.0*	57.3	−49.5	60.8	−7.77*	−1.40	−4.70	0.34
30	−510.3*	−67.2	7.9	66.6	−8.66*	0.25	−0.45	−0.44

* Significant at the $p < 0.05$ level (one-sided).

Definition of variables:

Product Complexity = an equally weighted index of standardized values of lines/order, number of finish codes, number of unique finish codes, and average item cost;
Price Quotes = percentage orders dealers submitted that require manual pricing;
Custom Quotes = percentage orders dealers submitted that include at least one custom material; and
Engineering Quotes = percentage orders dealers submitted that require manual verification of engineering designs.

with a minimum of two orders, to 255 dealers with a minimum of 30 EDI orders in each of the sample months.[24] The Z-statistics reported in Table 4, Panel A generally support the hypothesis that the learning coefficients for both time- and experience-based learning are positive and significant, indicating improved order accuracy (decreased error rates), especially for dealers submitting five or more EDI orders a month. The Z-statistic on the coefficients on the cumulative exception variable indicates that dealers learn from errors made in prior submissions, consistent with dealers learning from feedback on past errors. Thus, we find that EDI submissions are subject to learning, or more generally, to dealer adaptation, following EDI adoption. Evidence that a period of adaptation follows implementation of a new IT is consistent with Brynjolfsson's (1993) concern that a failure to find positive returns to IT investments may stem from lags due to learning and adjustment processes.

Hypothesis 2c posits that order complexity impedes dealer learning. We report the results of testing this proposition in Table 4, Panel B, where we regress the learning rates for each of the dealers (using time- or experience-based specifications of learning) on the average complexity of EDI orders the dealer submitted. Two general results are evident in Table 4, Panel B. First, for the larger sample sizes (including dealers submitting relatively few EDI orders in a month), there is a negative relation between price quotes and learning but a positive relation between custom quotes and learning. These relations appear to be due to those dealers submitting fewer than 20 EDI orders in a month. For the more restricted sample, which includes only relatively frequent EDI users, there is a consistent, though not universal, negative relation between product complexity and learning, but no evidence that process complexity impairs dealer learning. Thus, although some evidence is consistent with an adverse effect of product complexity on learning in Table 4, Panel B, we do not find strong evidence in support of H2c. Together, the results in Tables 3 and 4, Panels A and B provide partial support for the Indirect Effect Hypothesis, with strong support for H2a, generally consistent support for H2b, but only limited support for H2c.

Untabulated results reveal that task complexity explains only a small portion of the variance in the rate of learning across dealers (R^2 is less than 10 percent for all dealers and is typically less than 2 percent). Srinivasan et al. (1994) found that the supplier-specific rate of shipment errors for automotive suppliers was related to the manner of implementing EDI (e.g., level of systems integration with the customer), as well as with task complexity and heterogeneity. Unfortunately, however, we do not have dealer-specific information about EDI implementation, such as the level of training that the dealer may have sought or whether individual sales representatives or a dedicated clerk typically enters the orders.

Evidence on the relation between dealer learning and product and process complexity has implications for future advances in IT. For example, our results suggest that automatic entry of eligible price discounts may help reduce acknowledgement delays caused by manual processing of price quotes. However, evidence on how dealers gain unique mastery in the EDI task through clever process design or human resource management might have even more profound implications. Such evidence might offer simple remedies (beyond increasing the technology investment) for a problem that is costly to the firm but that is not fully internalized by the dealer. We raise this as an issue for future research and as a vivid illustration of an interorganizational approach to management control—specifically,

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[24] Even though many dealers appear in more than one sample, the same data are not included in the dealer-specific regressions in the different samples. For example, if a dealer places four EDI orders in six months and 20 EDI orders in ten months, then we include 16 months of data in the dealer-specific regression for the sample of dealers with at least three orders in a month, but only ten months of data in the dealer-specific regression for the sample of dealers with at least five EDI orders in a month.

SSD could use such data to diagnose problems and suggest improvements for dealers, with an expectation that improvements will cascade throughout the value chain.

VI. SUMMARY

Electronic data interchange is an accounting information technology that reduces key entry and standardizes information exchange between firms and their customers or suppliers. Although EDI reduces total key-entry time, it places greater demands on the customer or supplier to specify orders properly. We use data on more than 1.5 million orders placed with a large, office furniture manufacturing firm by more than 500 dealers to examine two basic questions. First, does adoption of electronic data interchange (EDI) improve performance in the order-entry function? Second, do dealer errors in EDI submission decrease the productivity of firms receiving orders, and if so, do these decreases offset the productivity increases from EDI submission? We also investigate whether dealers make fewer errors in using EDI over time or with experience (i.e., learn to use the technology), whether feedback on prior errors facilitates dealer learning, and whether order complexity impedes dealer learning.

We find that EDI use is associated with better departmental performance (i.e., faster sales order processing); however, these benefits are largely offset when dealers submit EDI orders with errors. After documenting that order complexity (both product and process complexity) impairs order-entry performance, we show that the benefits of error-free orders submitted via EDI offset most of the negative effects of product complexity. For complex EDI orders that contain errors, the benefits of EDI submission partially offset the effects of product complexity, but this effect is only about 40 percent of that of error-free EDI orders.

The second research question considers how dealers have adapted to the new EDI technology. Dealers often submit incomplete or incorrect orders. We find that both the passage of time and cumulative experience in EDI order submission tend to reduce dealers' error rates. We further find that feedback on prior errors facilitates dealer learning. Specifically, current-period errors decline with cumulative errors in prior periods. Finally, we find some evidence, albeit partial and incomplete, that complexity impedes dealer learning.

A major limitation of investigating the performance effects of interorganizational systems such as EDI, when data are available on the performance of only one of the transacting parties, is that such systems may simply transfer wealth between transacting parties with no net benefits. As a result, our conclusions are limited to the operational effects of EDI on the furniture manufacturer's order-processing group and we cannot assess the effect of EDI on the entire value chain.

Anecdotal evidence suggests that EDI trading partners frequently fail to fully implement the technology (Riggins and Mukhopadhyay 1994; Srinivasan et al. 1994). In particular, they frequently neglect employee training in the use of the technology. Our results are consistent with this conjecture. Our results are also consistent with economic models suggesting that task complexity acts as a barrier to learning or improvement; however, these models explain only a modest portion of the variation in dealer-specific learning rates. Future research that considers such dealer-specific determinants of learning could advance our understanding of the learning process and could suggest methods for implementing information technology that would enhance returns to the technology.

APPENDIX

A Process Description of Order Entry, from Order Receipt to Order Acknowledgment

Orders arrive from dealers by EDI, mail, or fax (Figure 1). From 1993 to 1996, orders from EDI-equipped dealers increased steadily from 77 to 88 percent, with 86 percent of all orders on average originating from EDI-equipped dealers (Table 1). Upon receipt, the order-entry system assigns the order a mail stamp date and a rank from 0 to +7 and places the order in a single, priority (lowest rank is highest priority) queue. The incoming rank assigned equals the target acknowledgment delay for the order. Each day the order's rank decreases by 1 and it becomes a higher service priority. SSD uses five ranks that correspond to target acknowledgment classes: (1) zero delay, for small (typically original equipment manufacturer-supplied) parts; (2) two-day delay, for customer service orders and orders against the company's express shipment program (a limited set of products available in limited colors); (3) three-day delay, for "normal" orders of average size; (4) five-day delay, for orders of one to five full truckloads of product; and (5) seven-day delay, for orders of more than five truckloads. Order editors select from the service queue according to priority, selecting orders of the same priority in order of mail stamp date.

The order editor first determines whether splitting an order into two or more pseudo-orders is likely to reduce acknowledgment delays. Two situations prompt order splitting: nonroutine customer demands and very large orders.[25] Order editors cannot do the additional processing customized products require. If an order contains both custom and standard products, then the order editor may split it into two pseudo-orders. As a result of delays associated with manual processes, the pseudo-order with custom products will generally have a longer acknowledgment delay than the pseudo-order with standard products. This is consistent with descriptive evidence in Table 1—that split orders have a higher incidence of engineering design changes (14 percent vs. 2 percent for orders that are not split), and use of custom materials (5 percent vs. 1 percent), and are more likely to be customized in some manner (only 6 percent have *no* custom material, pricing agreements, or design changes, vs. 28 percent of orders that are not split).

A second reason for splitting an order is order size. Order editors are likely to break exceptionally large orders, defined by the number of truckloads of finished goods or the percent of capacity of any single manufacturing plant that the order represents, into smaller pseudo-orders. Splitting orders that consume a large share of any plant's capacity reduces the risk of unused manufacturing capacity or inventory build-ups in the event that the dealer withdraws the order or delays it on short notice. It also decreases the elapsed time between order acknowledgment and subsequent production and shipment of the full order. A scheduling system automatically schedules production in the earliest available period that meets customer delivery requirements. Large production lots are difficult to schedule because large blocks of empty capacity are available only in distant time periods that are likely to violate delivery deadlines. The exception to this is when the system holds blocks of capacity based on an advanced reservation for exceptionally large orders, for which negotiated cancellation penalties apply.

If, after order editing, the pseudo-order does not require manual processing, then it joins the queue of completed jobs (Process Flow A in Figure 1, denoted hereafter as the "basic" process). At the end of the day, a batch-processing system attempts to validate orders in the queue. Orders that do not negotiate the batch process return to a separate (from incoming orders) queue for additional editing. The system acknowledges validated orders to the dealer via the same medium (EDI or U.S. mail) by which the orders are received. The acknowledgment is OFM's commitment to deliver a product to a specified location on a particular date. Dealers are responsible for reviewing acknowledgments and identifying discrepancies.[26] The task of order checking is easier for EDI-equipped dealers because the batch process identifies any change to the original EDI order (termed an "EDI exception flag"). EDI exceptions typically indicate dealer errors in the original order. Both split and unsplit orders arrive via EDI with approximately the same frequency (85 and 86 percent, respectively); however, the incidence of EDI exceptions for split orders is 66 percent greater than for unsplit orders.

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[25] Idiosyncratic reasons for order splitting include temporary stock-outs of a raw material required for a portion of an order or major production delays (e.g., equipment failures) at a plant that produces a portion of an order. We have no data on the incidence of these sources of acknowledgment delay.

Only 22 percent of orders follow the basic routing (Process A). Remaining orders are customized and require manual processing in at least one of four areas: scheduling, pricing, purchasing, or engineering. Order customization may reflect unique features of the business transaction or modifications to standard products. Manual production scheduling (Process B) and pricing (Process C) occur in SSD. Two other manual processes, verifying inventory of custom materials (Process D) and verifying readiness to produce specially engineered products (Processes E and F), cause orders to leave SSD for processing in engineering or purchasing. Customization creates process complexity that increases order acknowledgment delays. For example, Table 1 indicates that delays for unsplit orders that require no manual processing average 1.6 days vs. 1.9 to 8.0 days for custom orders. Order editors use order splitting to separate an order into parts, at least one of which is anticipated to have longer than average delays. The upper portion of Table 1, reports descriptive data on the average acknowledgment delay of *all* parts of split orders; for each process (A–F) the average acknowledgment delay for split orders exceeds that of the unsplit orders, shown in the lower part of Table 1. The following paragraphs describe alternative process flows (B–F).

Most production scheduling occurs automatically. The order editor submits the order to the scheduling system and, if the computer can fit the order into a production window that meets customer delivery requirements taking as given all previously scheduled orders, then it schedules the order. If the computer fails to schedule an order it goes to a scheduler, who may then override system constraints, reschedule previously scheduled orders, or work with a sales representative and the dealer to negotiate later delivery (Process B). The primary reason that an order fails automatic scheduling is size. Large orders (relative to production capacity) require manual intervention unless capacity reservations exist. Although we cannot identify orders in the database that require manual scheduling, we can obtain a conservative indication of scheduling delays by comparing the average acknowledgment delay of uncustomized orders in the top quartile of order value with all other uncustomized orders.[27] For unsplit orders, we observe an acknowledgment delay of 1.8 days—0.2 days more than for the basic process.

Approximately 68 percent of orders receive special discounting arrangements that are unrelated to product customization. Price agreements may reflect promotions on particular products for all dealers or unique arrangements with an end-user or dealer. If any item in an order is covered by a special pricing agreement, then the order editor sends the entire order to pricing after order entry (Process C). Pricing specialists review the agreement and assign a price to items covered by the agreement. On average, manual pricing adds 0.9 days to the acknowledgment delay. After specialists establish the price, the order joins the queue for batch processing or, if the order requires manual scheduling, then it goes to the scheduling department.

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[26] Although the firm replaces products that are ordered in error when the error is attributable to SSD, it is not liable for late shipments of these items. Dealer representatives who handle these claims indicate that the dealer has a significant stake in detecting and reporting these errors, because end-users are very sensitive to incorrect or incomplete shipments.

[27] This estimate of scheduling delay is conservative because very large orders are likely to have reservations that allow automatic scheduling, and our field research indicates that less than 25 percent of orders are scheduled manually.

Pricing and scheduling require manual intervention of SSD employees. The purchasing department processes orders (as shown in Process D) with custom materials (e.g., paint colors or upholstery fabrics) while the engineering department processes product design changes (Processes E and F). Because these orders leave SSD, acknowledgment delays significantly exceed those associated with pricing or scheduling. Before scheduling production of an order with custom materials, the purchasing department verifies acquisition and sufficient inventory of the material. Delays associated with custom materials add approximately five days to the acknowledgment delay of the basic process. Although submitted orders contain only design changes that OFM has accepted, an engineer reviews the order and provides cost data to the price specialists in SSD. After order editing, special orders go first to the engineering department and then to either the purchasing group (if the order requires special materials) or the pricing area of SSD. In the first case (Process E), acknowledgment delays increase 7.7 days over the base case; in the second case (Process F), acknowledgment delays increase 5.5 days.

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An Explanation for Unintentional Optimism in Analysts' Earnings Forecasts

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ABSTRACT: Archival studies report that analysts' annual earnings forecasts are optimistic, particularly for firms reporting recent losses. This study addresses whether forecast optimism is an unintentional consequence of analysts' reactions to the structure of information about managers' future plans. I investigate whether analysts who are forecasting earnings use processes consistent with scenario thinking: envisioning a sequence of events in which proposed actions lead to future outcomes. I study professional sell-side analysts in a 2 × 2 between-subjects experiment with the structure of information (scenario vs. list) and the sign of prior earnings (loss vs. profit) as independent variables. I find that analysts make more optimistic two-year-ahead earnings forecasts when provided information about a manager's future plans framed as scenarios than when provided the same information framed as lists. I also find that scenario-induced optimism is greater for a firm with prior losses than for a firm with prior profits. The results are consistent with scenario thinking causing analysts to issue optimistic forecasts.

Keywords: *scenario thinking; analysts' forecasts; forecast optimism.*

Data Availability: *Contact the author.*

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I. INTRODUCTION

Archival research documents that analysts issue more optimistic annual earnings forecasts for firms reporting recent losses than for firms reporting recent profits (Klein 1990; Ali et al. 1992; Dowen 1996). Consequently, researchers speculate that analysts underreact to prior losses and that the underreaction does not generalize to prior profits (Klein 1990; Ali et al. 1992). Although the literature offers considerable evidence consistent with the hypothesis that analysts issue intentionally optimistic forecasts in response to their incentives (e.g., Francis and Philbrick 1993; Lin and McNichols 1998), it lacks causal evidence for asymmetric forecast optimism across prior-loss and prior-profit firms. I investigate whether analysts' cognitive reactions to the structure of information about a manager's future plans cause patterns of *unintentional* optimism in annual earnings forecasts that are consistent with the asymmetric pattern of optimism that archival researchers observe.

I propose that the detailed scenarios managers commonly use to communicate their future plans affect analysts' annual earnings forecasts. Specifically, I expect that providing analysts with scenarios reduces their cognitive effort and naturally facilitates analysts' abilities to envision how and why a manager's plans will improve future firm performance (i.e., to use scenario thinking [Dawes 1988]). Scenario thinking inflates beliefs about the likelihood that envisioned future outcomes could occur (Koehler 1991). Therefore, I hypothesize that using scenarios to frame information about a manager's plans to increase future earnings facilitates scenario thinking, which in turn leads analysts to issue unintentionally optimistic forecasts. I further hypothesize that, regardless of how a manager frames plans to increase future earnings, analysts provided with this information make relatively more optimistic forecasts for a firm reporting losses than for a firm reporting profits, because managers' plans likely are more informative about future performance for loss firms compared to profit firms. Consequently, I hypothesize that scenario thinking is associated with greater optimism in analysts' earnings forecasts for a firm reporting prior losses than for a firm reporting prior profits.

In Experiment 1, I manipulate the structure of information about a manager's future plans (a scenario vs. a control [a list]) and the sign of prior earnings (loss vs. profit) in materials provided to professional sell-side analysts. I test participants' one- and two-year-ahead forecasts of annual earnings for a pattern in which scenario thinking causes greater relative optimism in the prior-loss condition than in the prior-profit condition. I find that two-year-ahead forecasts fall in the predicted pattern, but one-year-ahead forecasts do not. This finding is consistent with prior archival research documenting that analysts issue more optimistic forecasts for two-year-ahead horizons than for one-year-ahead horizons (DeBondt and Thaler 1990). In Experiment 2, I provide evidence that the greater optimism in the Experiment 1 scenario conditions was driven by differences in information structure, and not by differences in information content between the scenario and list conditions.

I use experiments to investigate the influence of scenario thinking on analysts' forecasts because archival data do not offer a control condition against which to compare the scenarios that firm managers actually provide to analysts in practice. In the experiments, I create a control condition that uses an unstructured list to present information about a manager's future plans. Although such lists do not occur in practice, they provide a basis against which to measure the incremental effect on analysts' forecasts of providing information in scenarios.[1] Additionally, the experiments hold constant investment banking incentives, which enables me to examine *unintentional* sources of forecast optimism. A limitation of my approach is that I cannot measure absolute levels of analyst forecast bias, and thus the evidence is limited to *relative* levels of optimism across experimental conditions.

Because investors do not completely unravel analysts' optimism (Huberts and Fuller 1995; Ackert and Athanassakos 1997; Rajan and Servaes 1997; Dechow et al. 2000), evidence on the causes of optimism may suggest ways to debias forecasts, potentially improving analysts' forecast quality and investors' decisions. My evidence indicates that the structure of information managers provide about their future plans for the firm naturally facilitates scenario thinking and causes analysts to issue unintentionally optimistic earnings forecasts, suggesting that analysts who wish to debias their forecasts will have to analyze the scenarios managers provide and explicitly assess the plausibility and likelihood of alternative potential outcomes.

The remainder of this paper is organized as follows. Section II analyzes relevant literature and develops the hypotheses. Section III describes the design and method for Experiment 1. Section IV presents results of Experiment 1 and the design, method, and results of Experiment 2. Section V summarizes and concludes.

II. LITERATURE ANALYSIS AND HYPOTHESIS DEVELOPMENT

Optimism in Analysts' Forecasts

Optimism in annual earnings forecasts is statistically and economically significant (e.g., Richardson et al. 2002; Darrough and Russell 2002; Duru and Reeb 2002) and increases over the forecast horizon.[2] For example, analysts' two-year-ahead forecasts are relatively more optimistic than their one-year-ahead forecasts (DeBondt and Thaler 1990).

Prior research offers considerable evidence consistent with the hypothesis that analysts issue intentionally optimistic forecasts in response to their incentives: to maintain access to managers' private information (Francis and Philbrick 1993; Krishnan and Sivaramakrishnan 1998; Lim 2001), to motivate stock trades (Kim and Lustgarten 1998), to benefit stocks held by in-house mutual funds (Irvine et al. 1998), and to obtain or maintain investment banking business (Dugar and Nathan 1995; Hunton and McEwen 1997; Lin and McNichols 1998; Dechow et al. 2000).[3] Although incentives to issue accurate forecasts likely temper intentional optimism, forecast optimism persists (for reviews see Schipper [1991] and Brown [1993]).[4] Moreover, market analysts (Darrough and Russell 2002) and students (Affleck-Graves et al. 1990) presumably lack incentives for optimism, but make biased earnings forecasts nevertheless. These studies raise the following question: Is forecast optimism, at least partially, an unintentional consequence of the thought processes provoked by the information managers provide to analysts?

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[1] Libby et al. (2001) discusses the advantages of designing an experiment that creates one level of an independent variable that cannot occur in practice.

[2] In contrast, there is recent evidence that optimism in quarterly earnings forecasts has declined during the past two decades (e.g., Brown 1997, 2001; Matsumoto 2002).

[3] Other explanations for optimistically biased forecasts include justifying favorable stock recommendations (Eames et al. 2002), choosing to report only on stocks viewed favorably (McNichols and O'Brien 1997), and failing to account completely for earnings management (Abarbanell and Lehavy 2000).

[4] Generally, analyst compensation depends on two factors: perceived reputation and ability to generate revenues and profits (Michaely and Womack 1999). One indicator of an analyst's reputation is selection to the *Institutional Investor* All-American Research Team (Stickel 1992) that is based on stock picking, earnings estimates, written reports, and overall service. Accuracy is important to an analyst's reputation because picking stocks and estimating earnings are directly related to accurate forecasts and recommendations (Michaely and Womack 1999). Additionally, greater forecast accuracy decreases the likelihood of termination (Mikhail et al. 1999) and increases the likelihood of moving from a lower to a higher status brokerage firm (Hong and Kubik forthcoming).

Analysts' Forecasting Processes and Scenario Thinking

I argue that providing analysts with information about managers' future plans in scenarios facilitates analysts' use of scenario thinking. However, because no single theory in psychology predicts analysts' forecast behavior uniquely, I rely on a family of psychological theories that invoke the influence of information structure and of imagination on judgment, among them the story-model (Pennington and Hastie 1986, 1988, 1993), availability (Tversky and Kahneman 1973), explanation (see Koehler [1991] for a review), and scenario thinking (Dawes 1988). Taken together, these theories offer predictions consistent with the claim that scenario thinking inflates analysts' beliefs about the plausibility of a manager's future plans and about the likelihood that anticipated future outcomes will occur as the manager describes. I use the term "scenario thinking" throughout this paper to refer to the common prediction of this larger family of theories.

When developing forecasts, individuals typically generate a preliminary forecast from time-series information, consider other relevant information, and revise the forecast (Webby and O'Connor 1996). Consistent with this process, my fieldwork interviewing financial analysts reveals that analysts generate preliminary forecasts from historical time-series information, and then adjust these forecasts based on judgments about managers' qualitative, forward-looking plans that are not otherwise captured in historical information. The key element of this process for my study is that in preparing their earnings forecasts, *analysts use judgment* to integrate historical time-series information with managers' descriptions of their future plans for the firm.

Analysts' communications with managers are an essential source of forecast-relevant information and, in particular, of forward-looking information about a firm (e.g., Epstein and Palepu 1999; Kahn and Rudd 1999). Managers use narratives to provide analysts with information about business strategies and future prospects (Francis et al. 1997), managers' future plans for the firm (Tasker 1998), and the anticipated effects of planned actions on future earnings (Previts et al. 1994). Individuals naturally communicate in narratives because they provide a descriptive and organized structure to convey information (e.g., Baumeister and Newman 1994; McGregor and Holmes 1999). Narratives help managers communicate by describing in detailed, causal order the relations among current states, managers' future plans, and anticipated future outcomes. For example, in conference calls, chief executive officers (CEOs) may discuss in narratives—and often from prepared texts—the planned introduction of new products, new product introduction timelines, favorable relations with critical suppliers, and expected increases in future earnings.[5] Narratives that concretely describe the sequence of events in which proposed actions lead to future outcomes are termed *scenarios* (Tversky and Kahneman 1973; Jungermann 1985; Schnaars 1987; Schoemaker 1991, 1993). Individuals use *scenario thinking* when they contemplate information structured in scenarios (Dawes 1988).

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[5] I monitored conference calls, read press releases, and read annual reports, and found that managers commonly use narratives to communicate this type of information.

I propose that using scenarios to provide analysts with information about managers' future plans reduces analysts' cognitive effort and naturally facilitates scenario thinking.[6] Using scenario thinking, analysts *envision* in concrete detail how a manager will implement plans and, ultimately, how and why the manager expects those plans to improve future firm performance. Scenario thinking inflates analysts' beliefs about the plausibility of a manager's plans and the likelihood that improvements in future performance will occur, because individuals use reasons for an outcome (e.g., Hoch 1984; Levi and Pryor 1987) and ease of imagination (Tversky and Kahneman 1973; Kahneman and Tversky 1982; Kahneman and Lovallo 1993) as cues when making probability assessments. Concrete details strengthen the effect (Hendrickx et al. 1989; Hendrickx et al. 1992). I expect that scenario thinking causes *optimism* in analysts' forecasts because managers have strong incentives to create favorable long-term outlooks for their firms (Kahn and Rudd 1999) and to present plans to increase future earnings.[7] More formally, I propose the following alternative hypothesis:

> **H1:** Analysts provided with information about a manager's plans to increase future earnings make more optimistic earnings forecasts if the information provided is in a scenario than if the information provided is in an unstructured list.

Forecast Optimism and Firm Performance

The most optimistic annual earnings forecasts are associated with firms that have weak prior earnings performance (Abarbanell and Bernard 1992; Elgers and Lo 1994) or that report losses in the prior year (Klein 1990; Ali et al. 1992; Dowen 1996).[8] These findings suggest that analysts asymmetrically underreact to prior earnings information.[9] However, prior earnings performance is not the only source of information that analysts use in forecasting. I propose that analysts' apparent asymmetric underreactions to prior earnings information are at least partly attributable to their reactions to prior earnings *and* to information about managers' future plans.

Analysts tend to emphasize the income statement and related earnings indicators when forecasting earnings (McEwen and Hunton 1999). However, past earnings is not necessarily a good predictor of future earnings, particularly for loss firms. For example, losses are unlikely to represent the long-run equilibrium condition of the firm (Dowen 1996). Because losses are unlikely to persist, they are less informative than profits about a firm's future prospects (Hayn 1995; Jan and Ou 1995). Thus, other forecast-relevant information is likely to become relatively more important to analysts when they are forecasting earnings for loss firms.

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[6] More generally, prior research demonstrates that the structure of decision-relevant information influences both memory and judgment. For instance, auditors recalled a greater number of internal controls when the controls were listed in a manner consistent with transaction flow than when the controls were listed by taxonomic categories (Frederick 1991). Relevant evidence had a greater influence on audit partners' going-concern decisions when structured in a meaningful, causal order than when structured in an artificial working paper order (Ricchiute 1992). I expect the structure of decision-relevant information to influence the judgments of experienced analysts as well.

[7] Managers may provide information to "guide" analysts' quarterly earnings forecasts downward to improve the chances of meeting or beating the forecasts (Matsumoto 2002). In this study, I examine analysts' cognitive reactions to information about a manager's plans to increase future earnings—a phenomenon distinct from analysts' reactions to quarterly earnings guidance.

[8] This finding is consistent with Francis and Philbrick (1993), who argue that, following a sell recommendation for firms with poor performance, analysts issue optimistic forecasts to mend relations with managers. However, Eames et al. (2002) demonstrate that optimistic forecasts are actually associated with *buy* recommendations and that pessimistic forecasts are associated with *sell* recommendations.

[9] Easterwood and Nutt (1999), however, show that analysts underreact to prior losses and overreact to prior profits, thus interpreting all prior earnings information optimistically.

Other information can help forecasters by providing insight into future discontinuities (e.g., changes in trends) in a time-series (O'Connor and Lawrence 1998). In the absence of information to the contrary, analysts likely do not anticipate discontinuities in the earnings time-series for profitable firms and, thus, the time-series of past earnings is relevant to the prediction of future earnings. In contrast, analysts likely anticipate discontinuities in the earnings time-series for loss firms because they expect the managers to implement new, earnings-producing strategies. Thus, I expect analysts to weight information about managers' future plans more heavily than they do time-series information when forecasting for loss firms, because historical time-series information is relatively less informative about the future. As a consequence, I predict that analysts provided with information about managers' plans to improve future earnings make more optimistic forecasts for a firm reporting prior losses than for a firm reporting prior profits, regardless of the information's structure. More formally, I propose the following alternative hypothesis:

> **H2:** Analysts provided with information about a manager's plans to increase future earnings make more optimistic earnings forecasts if a firm reports prior losses than if a firm reports prior profits.

Interaction between Scenario Thinking and Firm Performance

Because analysts increase their focus on information about managers' future plans and weight it more heavily than the historical time-series of earnings when forecasting for loss firms, I expect scenario thinking to have a greater effect on analysts' forecasts for a firm reporting prior losses than for a firm reporting prior profits. More formally, I propose the following alternative hypothesis:

> **H3:** The extent to which analysts make optimistic earnings forecasts when information about a manager's plans to increase future earnings is provided in a scenario vs. in an unstructured list is greater if a firm reports prior losses than if a firm reports prior profits.

Figure 1 presents the specific pattern of analysts' forecasts predicted by H1–H3 collectively.

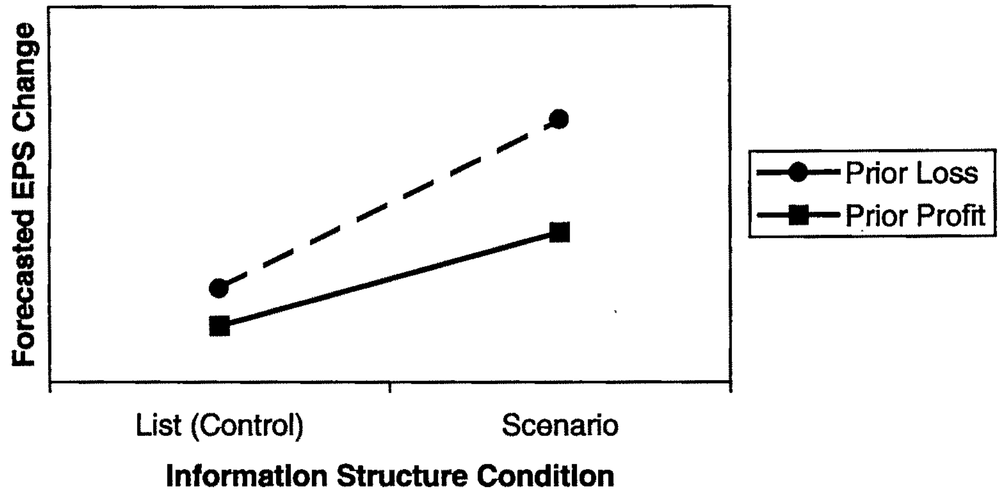
III. EXPERIMENTAL DESIGN AND METHOD

I examine the effects of the structure of information and the sign of prior earnings on analysts' forecast optimism, and whether the effect of scenario thinking is greater for a firm reporting prior losses than for a firm reporting prior profits. I use a full-factorial 2 × 2 between-subjects design with the structure of information (scenario vs. list) and the sign of prior earnings (loss vs. profit) as independent variables. The primary dependent variable is participants' forecasted earnings changes.

Participants

Participants are 86 sell-side financial analysts I recruited from the 1999 *Nelson's Directory of Investment Research*, the 2000 *AIMR Membership Directory*, a business school alumni database, and a brokerage firm. The participants average eight years of experience and cover 21 different industries; 37 are certified financial analysts (CFAs) and 55 hold graduate degrees. I assigned participants to experimental conditions randomly and sent them

FIGURE 1
Predicted Effects of Scenario Thinking on Participants' Forecasted EPS Changes



This figure depicts the predicted pattern of participants' mean forecasted EPS changes. I used this pattern to derive the contrast coefficients for the planned contrasts presented in Table 2. The predicted pattern requires: (1) mean forecasted EPS changes for scenario conditions (points on the right-hand side of the figure) to exceed mean forecasted EPS changes for the list (control) conditions (points on the left-hand side of the figure), (2) mean forecasted EPS changes for the prior-loss conditions (circular points) to exceed mean forecasted EPS changes for the prior-profit conditions (square points), and (3) the influence of scenario thinking on analysts' forecasts for a prior-loss firm (slope of dotted line) to be greater than the influence of scenario thinking on analysts' forecasts for a prior-profit firm (slope of solid line).

the instrument by mail or electronically, depending on their preference. No one experimental condition contained disproportionately more CFAs or more graduate-degree holders.

Case Materials and Procedures

The materials instructed each participant to imagine he or she was initiating coverage of a fictional company (a winery) that did not have (and did not contemplate) an investment banking relationship with the participant's firm.[10] I constructed the case materials from Forms 10-K filed by companies in the wine industry.[11] Participants read three years of summarized financial information, a brief description of the company, and product information before making preliminary forecasts of annual earnings per share (EPS) for one and two years ahead. Next, the participants read excerpts from a conference call held after the company's annual earnings announcement, and then revised their preliminary forecasts. The conference-call excerpts included the CEO's discussion of the industry, the company, and management's plans and expectations for the future. Finally, the participants completed a post-experimental questionnaire.

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[10] The case stipulated that the winery had no prior or contemplated investment banking relationship in order to ensure that investment banking incentives did not influence the optimism of participants' forecasts and, more importantly, to equalize participants' assumptions about incentives across experimental conditions. In the post-experimental questionnaire, one participant reported that he or she assumed the winery had an investment banking relationship and that it was important for him or her to forecast earnings favorably. I excluded this participant's responses from the data analysis.

[11] I chose the wine industry for several reasons. First, the industry is not followed by a large number of analysts (only one participant follows the beverage industry), thereby minimizing the advantage of industry-specific expertise. Second, since most adults are familiar with wine and wineries, I could construct a descriptive, non-technical scenario likely to be accessible to all participants. Third, because the income statements of firms in the wine industry are relatively simple, I could construct materials that captured relatively limited financial data without loss of essential information. Finally, at (and before) the time the experiment was conducted, the press had not reported derogatory news about the wine industry (e.g., excessive supplies, recalls).

Information Structure

In the CEO's conference-call excerpts, I manipulated the structure of information that participants read as: (1) a narrative scenario (scenario condition), or (2) an unstructured list of information (list condition). The scenario condition mirrors practice and captures two important attributes of scenarios: concrete details and causal orderings that link current states, planned actions, and anticipated future outcomes. For example, in conference calls, analysts commonly hear complete, explicit sentences (concrete details) that provide clear, causal connections among the company's current financial performance (current states), the managers' plans (planned actions), and the managers' expectations for future firm performance (future outcomes). In contrast, the list condition does not occur commonly in practice, but is used here as a control. The list-condition participants read the CEO's remarks in a bulleted list of statements that highlighted all forecast-relevant information, but lacked detailed sentences, causal order, and causal links.[12] Because the causality and concreteness inherent in scenarios have psychological implications for forecasters (Kuhn and Sniezek 1996), I manipulated these attributes jointly to maximize differences between experimental groups.[13] The Appendix presents the scenario condition (Panel A) and the list condition (Panel B).[14]

Sign of Prior Earnings

I provided current year and the prior two years of sales, gross profit, and earnings information to participants. I manipulated the sign of prior earnings at two levels: positive and negative for reported earnings and EPS. Consistent with results for the current year, earnings and EPS for the prior two years were positive in the prior-profit condition and negative in the prior-loss condition. To equalize the potential influence of heuristic strategies across experimental conditions, I used the same absolute value of current EPS and held constant the magnitudes of the differences between EPS values across time (i.e., the trends).[15] Furthermore, I used identical descriptions of the company and its products, and I equated yearly sales growth percentages and gross profit percentages across the prior-profit and prior-loss conditions.

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[12] To create the bulleted list of statements, I prepared from the scenario-condition excerpt a causal network (see Trabasso and Sperry 1985) that mapped direct, causal relations. Sixteen doctoral students at a large business school read the materials and underlined information they believed important to an EPS forecast. I summarized all information underlined in a list. From this summary, I eliminated redundancies, removed concrete details and transitional phrases, and disturbed the causal order to create the list condition. To control for order effects, I used four different orderings in the list condition. For consistency, the materials mention an expected profit increase *last* in all orderings and in the scenario condition. Analysis of variance (ANOVA) models reveal that order had no effect on list-condition participants' forecasts ($F_{3,40} = 0.24$, $p = 0.87$ and $F_{3,40} = 0.70$, $p = 0.56$, two-tailed, for years 1 and 2, respectively).

[13] In this manipulation, causal order of information is confounded with content of information. In Section IV, I report a second experiment that provides evidence that results of the scenario manipulation are attributable to the causal order of information.

[14] A reconciliation between the language used in the scenario and list conditions is available from the author.

[15] For instance, a common form of anchoring and adjusting is to use the most recent value in a time-series as an anchor, and to adjust by adding some proportion of the difference between the most recent value and the predecessor value (Goodwin and Wright 1994). I constructed the time-series of earnings to ensure that an anchoring-and-adjustment heuristic would affect EPS forecasts equally in all experimental conditions.

Dependent Measures

Current (year 0) EPS was \$0.64 in the prior-profit condition and –\$0.64 in the prior-loss condition. I calculate the primary dependent measures by subtracting the current EPS from the participants' revised EPS forecasts for both one and two years ahead. I exclude three participants' forecasted earnings changes from the data analyses because they exceeded the overall mean by four standard deviations.[16]

I use participants' responses to relevant post-experimental questions both as manipulation checks and as measures of whether they reported using cognitive processes consistent with scenario thinking. All post-experimental questions use a nine-point scale. Several participants failed to answer one or more questions. I base each analysis on the maximum number of useable responses.

IV. RESULTS AND DISCUSSION

Figure 1 shows the specific pattern of cell means for analysts' forecasted EPS changes, predicted by H1–H3 collectively. To maximize the power of my hypothesis tests, I use planned contrasts to test for the Figure 1 pattern because I have limited degrees of freedom and I predict a specific, nonsymmetric pattern of cell means *a priori* (Buckless and Ravenscroft 1990; Rosnow and Rosenthal 1995).

Manipulation Check

I asked participants several questions to determine whether they used cognitive processes consistent with scenario thinking. To assess whether participants were thinking about the causal relations among current states, planned actions, and anticipated future outcomes, I asked them to report the extent to which they could "clearly determine why management would take certain actions and how those actions were intended to lead to future outcomes" (WHY); "easily fit related pieces of information together to form a story about management's plans and expectations for the upcoming year" (FIT); and "write, from memory, a detailed paragraph explaining management's plans for the upcoming year and their expectations for sales and profits" (WRITE). Participants responded on a scale ranging from 1 = Not at all to 9 = To a large extent.

A principal component analysis reveals that one factor accounts for the majority of variance in these measures (eigenvalue = 2.04, variance explained = 68 percent). I refer to this factor as scenario thinking. A comparison of factor scores for scenario thinking between information-structure conditions reveals that scenario-condition participants reported using processes consistent with scenario thinking to a greater extent than did list-condition participants ($t_{78} = 1.72$, $p = 0.05$, one-tailed). This result is consistent with a successful manipulation.

Test of Hypotheses

Table 1 reports means, standard deviations, cell sizes, and forecasted EPS changes for one-year- (year 1) and two-year- (year 2) ahead revised forecasts. Figure 2, Panels A and B, plots cell means for year 1 and year 2 forecasted EPS changes, respectively. I use a single, planned contrast to test whether participants' forecasts fall in the pattern predicted by H1–H3 collectively, as shown in Figure 1. I derive contrast weights from Figure 1 by subtracting the overall expected mean for all cells from the expected mean for each cell (Rosenthal and Rosnow 1985; Buckless and Ravenscroft 1990). Using this procedure, the

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[16] DFFITS and DFBETA procedures indicate that, in fact, these extreme observations are influential outliers. Thus, owing to small cell sizes, the results would be sensitive to these observations.

TABLE 1
Experiment 1: How Information Structure and Sign of Prior Earnings Affect Participants' EPS Forecasts[a]

			Mean [standard deviation]			
Information Structure	*Sign of Prior Earnings*	*n*	*Revised Forecast Year 1*[b]	*Forecasted Year 1 EPS Change*[c]	*Revised Forecast Year 2*[b]	*Forecasted Year 2 EPS Change*[c]
Scenario	Profit	19	0.79	0.15 [0.13]	1.05	0.41 [0.23]
List (Control)	Profit	24	0.78	0.14 [0.14]	1.00	0.36 [0.26]
Scenario	Loss	17	(0.32)	0.32 [0.34]	0.08	0.72 [0.50]
List (Control)	Loss	21	(0.37)	0.27 [0.18]	(0.12)	0.52 [0.32]

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[a] Forecast optimism is calculated by comparing forecasted EPS changes of scenario-condition participants to forecasted EPS changes of list-condition participants. If forecasted EPS changes in scenario conditions are more positive than forecasted EPS changes in list conditions, then scenario-condition participants are *relatively* more optimistic.

[b] Revised forecast year t = EPS forecast made after the information-structure manipulation for year t with t = 1 or 2.

[c] Forecasted year t EPS change = Revised forecast year t − EPS value provided to participants for year 0 (i.e., revised forecast year t − \$0.64 in the prior-profit conditions and revised forecast year t − (\$0.64) in the prior-loss conditions, respectively) with t = 1 or 2.

contrast coefficients for the test of my hypotheses are −2 for the prior-profit/list condition, −1 for the prior-loss/list condition, +0 for the prior-profit/scenario condition, and +3 for the prior-loss/scenario condition.

The contrast coding reflects the *specific* pattern of analysts' forecasts indicated by H1–H3 collectively. First, the contrast coding shows that participants who are provided with information about a manager's plans to increase future earnings make more optimistic earnings forecasts if the information provided is in a scenario than if the information provided is in an unstructured list (H1). Thus:

- the contrast coefficient for the prior-loss/scenario condition (+3) is greater than the coefficient for the prior-loss/list condition (−1), and
- the contrast coefficient for the prior-profit/scenario condition (+0) is greater than the coefficient for the prior-profit/list condition (−2).

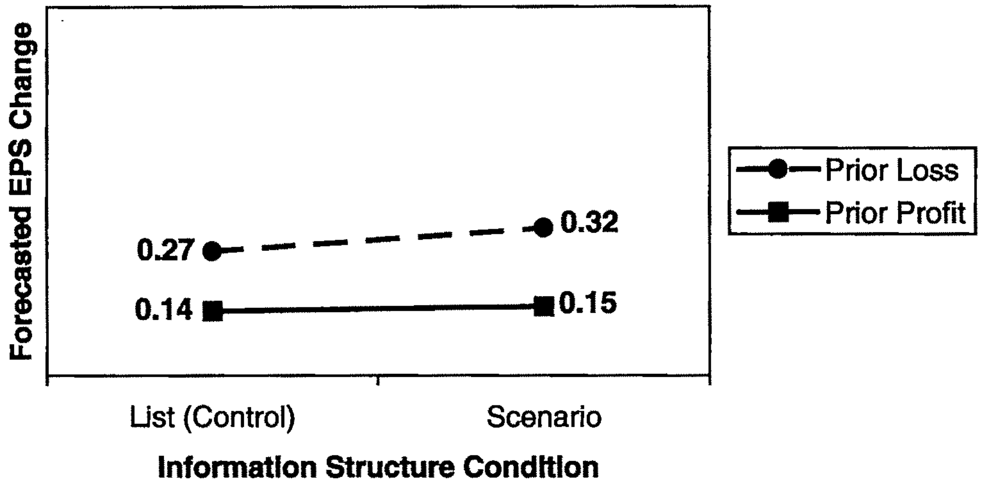
Second, the contrast coding shows that participants who are provided with information about a manager's plans to increase future earnings make more optimistic earnings forecasts if a firm reports prior losses than if a firm reports prior profits (H2). Thus:

- the contrast coefficient for the prior-loss/list condition (−1) is greater than the coefficient for the prior-profit/list condition (−2), and
- the contrast coefficient for the prior-loss/scenario condition (+3) is greater than the coefficient for the prior-profit/scenario condition (+0).

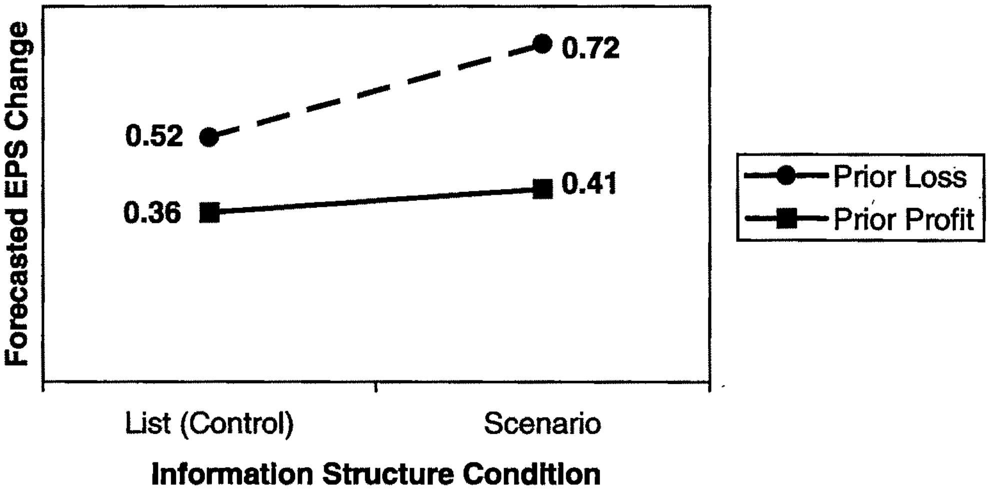
FIGURE 2

Observed Effects of Scenario Thinking on Participants' Forecasted EPS Changes

Panel A: Dependent Variable—Forecasted Year 1 EPS Change



Panel B: Dependent Variable—Forecasted Year 2 EPS Change



This figure depicts the observed patterns of participants' mean forecasted EPS changes. I test these patterns using the planned contrasts presented in Table 2. Panel A presents participants' mean forecasted EPS changes for one year ahead and Panel B presents participants' mean forecasted EPS changes for two years ahead.

Finally, the contrast coding shows that the extent to which participants make optimistic earnings forecasts when information about a manager's plans to increase future earnings is provided in a scenario vs. in an unstructured list is greater if a firm reports prior losses than if a firm reports prior profits (H3). Therefore, the difference between the contrast coefficients for the prior-loss conditions (i.e., +3 for scenario; −1 for list) is greater than the difference between the contrast coefficients for the prior-profit conditions (i.e., +0 for scenario; −2 for list).[17]

Table 2 presents Experiment 1 results and provides F-statistics with two-tailed p-values. However, because I test for a specific, directional pattern, I refer to the one-tailed p-values

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[17] Alternative sets of contrast coefficients for the predicted pattern (reflecting more extreme differences across experimental conditions) are −3, −1, +0, +4; −4, −2, +1, +5; −5, −3, +2, +6; and −5, −3, +1, +7 and yield identical inferences (all $p < 0.05$, one-tailed, for year 2 forecasted EPS changes).

TABLE 2
Experiment 1: Contrast Coding Summary Tables for Planned Contrasts Used to Test Participants' Forecasted EPS Changes for the Pattern Predicted in Figure 1

Panel A: Dependent Variable—Forecasted Year 1 EPS Change[a]

Source of Variation	*SS*	*df*	*MS*	*F-statistic*	*p-value*[b]
Preliminary forecasted EPS change[c] (covariate)	1.220	1	1.220	45.31	0.00
Model contrast[d] (tests for Figure 1 pattern)	0.049	1	0.049	1.82	0.18
Residual[e]	0.106	2	0.053	1.96	0.15
Error	2.046	76	0.027		

Panel B: Dependent Variable—Forecasted Year 2 EPS Change[a]

Source of Variation	*SS*	*df*	*MS*	*F-statistic*	*p-value*[b]
Preliminary forecasted EPS change[c] (covariate)	5.429	1	5.429	136.53	0.00
Model contrast[d] (tests for Figure 1 pattern)	0.166	1	0.166	4.15	0.04[f]
Residual[e]	0.048	2	0.024	0.60	0.55
Error	3.022	76	0.040		

[a] Forecasted year t EPS change = Revised forecast year t − EPS value provided to participants for year 0 (i.e., revised forecast year t − \$0.64 in the prior-profit conditions and revised forecast year t − (\$0.64) in the prior-loss conditions, respectively) with t = 1 or 2.

[b] All reported p-values are two-tailed.

[c] Participants made preliminary EPS forecasts prior to the information-structure manipulation. Preliminary forecasted EPS change = Preliminary forecast year t − EPS value provided to participants for year 0 (i.e., preliminary forecast year t − \$0.64 in the prior-profit conditions and preliminary forecast year t − (\$0.64) in the prior-loss conditions, respectively) with t = 1 or 2.

[d] Contrast coefficients are −2 for the prior-profit/list condition, −1 for the prior-loss/list condition, +0 for the prior-profit/scenario condition, and +3 for the prior-loss/scenario condition.

[e] The residual sum of squares represents the between-group variance unexplained by the model contrast used to test the hypotheses. An insignificant p-value indicates that the model contrast explains all of the between-group variance in the data.

[f] The test of the model contrast is a directional test. Thus, the reported p-value is significant at the 0.02 level, one-tailed.

associated with each F-statistic in the text discussion. Table 2, Panel A, summarizes the planned contrast I use to test whether year 1 forecasted EPS changes fall in the hypothesized pattern. Although I did not expect premanipulation differences in forecasts between the scenario and list conditions, I use differences between participants' preliminary forecasts and the year 0 EPS values (i.e., preliminary EPS changes) as covariates to increase the likelihood of detecting treatment effects.[18] The year 1 forecasts do not support the hypothesized pattern depicted in Figure 1 ($F_{1,76} = 1.82$, $p = 0.09$, one-tailed).

Table 2, Panel B, summarizes the planned contrast for year 2 forecasted EPS changes. The hypothesized pattern is supported for year 2 forecasts ($F_{1,76} = 4.15$, $p = 0.02$, one-tailed). Consistent with expectations, participants in the prior-loss conditions forecast greater positive changes in earnings than participants in the prior-profit conditions and, compared to list-condition participants, scenario-condition participants forecast greater positive earnings changes in the prior-loss condition than in the prior-profit condition.[19]

Mediation Analysis

I perform a mediation analysis to examine whether scenario thinking is driving participants' year 2 forecasted EPS changes. Year 2 forecasted EPS changes are positively related to year 1 forecasted EPS changes ($r = 0.80$, $p < 0.01$) and, thus, I control for the latter to increase the power of my test. Scenario thinking mediates the relation between information structure and participants' year 2 forecasted EPS changes ($F_{1,77} = 3.58$, $p = 0.03$, one-tailed) if three conditions are established: (1) information structure is associated with scenario-thinking measures, (2) variations in forecasted EPS changes are associated with variations in scenario-thinking measures, and (3) after controlling for variations in scenario-thinking measures, the relation between information structure and forecasted EPS changes is no longer significant (see Baron and Kenny 1986).

Results for the manipulation check provide evidence that information structure is significantly associated with scenario-thinking measures, satisfying condition (1). I estimate an analysis of covariance (ANCOVA) model for participants' year 2 forecasted EPS changes to test for conditions (2) and (3). The ANCOVA (not shown) reveals that scenario-thinking measures are significantly associated with year 2 forecasted EPS changes ($F_{1,76} = 3.16$, $p = 0.04$, one-tailed), satisfying condition (2). In addition, after controlling for scenario-thinking measures, the association between information structure and forecasted EPS changes is less significant ($F_{1,76} = 2.38$, $p = 0.06$, one-tailed), partially satisfying condition (3). This analysis is generally consistent with scenario thinking mediating the effect of information structure on participants' year 2 forecasted EPS changes.

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[18] Inferences remain unchanged when I exclude preliminary forecasts from the model ($t_{19} = 1.52$, $p = 0.07$ and $t_{20} = 2.39$, $p = 0.01$, one-tailed, for year 1 and year 2 forecasted EPS changes, respectively). Levene's test for homogeneity of variance of the unadjusted means reveals unequal variances for year 1 (Levene statistic 7.48, $p = 0.00$), but not for year 2 (Levene statistic 2.13, $p = 0.10$). I report t-statistics adjusted for unequal variances in this footnote.

[19] I demonstrate the robustness of my result using two nonparametric tests. The Kruskal-Wallis test (i.e., ANOVA by ranks) replaces original scores on the dependent variable with their ranks and reveals differences among the four experimental groups for both years ($\chi^2 = 7.20$, $p = 0.07$ and $\chi^2 = 9.70$, $p = 0.02$, two-tailed, for years 1 and 2, respectively) (Maxwell and Delaney 1990). Consistent with parametric results, planned contrasts of mean ranks using the contrast coefficients from my hypothesis tests do not support the hypothesized pattern in year 1, but do support the hypothesized pattern in year 2 ($\chi^2 = 0.58$, $p > 0.10$ and $\chi^2 = 6.35$, $p < 0.10$, two-tailed, for years 1 and 2, respectively) (Maxwell and Delaney 1990). Additionally, I apply the Jonckheere test for ordered alternatives to test for an *a priori* ordering of groups using counts for the number of instances that a pattern similar to the hypothesized pattern in Figure 1 holds in the data: forecasted EPS changes made by prior-profit/list-condition participants < forecasted EPS changes made by prior-loss/list-condition participants < forecasted EPS changes made by prior-profit/scenario-condition participants < forecasted EPS changes made by prior-loss/scenario-condition participants. Consistent with parametric results, the year 1 forecasts do not support this pattern, but the year 2 forecasts do ($J = 1.04$, $p > 0.05$ and $J = 2.46$, $p < 0.05$, one-tailed, for years 1 and 2, respectively) (Siegel and Castellan 1988).

Tests for Alternative Explanations

I offer tests to rule out potential alternative explanations for the results. First, I find that experience is not correlated with EPS-forecast or with scenario-thinking measures (all $p > 0.20$), indicating that experience differences are unlikely to drive the results.[20] Second, analysis of variance (ANOVA) models reveal no significant relationship between the independent variables and perceptions of task realism, beliefs about macroeconomic conditions, beliefs about agricultural risks, or the importance of making accurate forecasts (p-values > 0.15 for all *omnibus* F-tests).

Third, an ANOVA reveals no significant effects of the independent variables on perceptions of managerial capability (*omnibus* $F_{3,74} = 0.74$, $p = 0.53$). This analysis indicates that providing scenario-condition participants with detailed conference-call excerpts did not lead them to believe that the managers were more capable, and thus to make relatively more optimistic forecasts than list-condition participants. Fourth, an ANOVA reveals no significant effects of the independent variables on the importance of providing favorable forecasts (*omnibus* $F_{3,78} = 0.98$, $p = 0.41$). Although participants who reported using scenario thinking forecasted greater increases in future earnings, these participants reported that providing favorable earnings forecasts was not important to them (mean [standard deviation] was 2.81[1.93] on a scale ranging from 1 = Not at all important to 9 = Extremely important). This analysis suggests that scenario thinking causes unintentional optimism.

Experiment 2: Disentangling the Effects of Information Content and Information Structure

In Experiment 1, the information I provide to scenario-condition participants differs in content and structure from the information I provide to list-condition participants (a scenario vs. a list). Scenario-condition participants could have made relatively more optimistic EPS forecasts than list-condition participants because they had more information (i.e., *content*), not because they had information in a scenario (i.e., *structure*). For example, the scenario condition provides the names of gourmet restaurants that will carry the winery's products, but the list condition does not. I perform Experiment 2 to address this potential confound by disentangling the effects of manipulating *content* from the effects of manipulating *structure*.

Participants

Participants are 49 executive M.B.A. (EMBA) students at a private university. The participants averaged 12 years of experience in 21 different industries and nine had forecasted earnings in practice. EMBAs would not be good proxies for analysts in the Experiment 1 tests for the effects of information structure and the sign of prior earnings on scenario thinking and forecasts because responses to the sign of prior earnings likely depend upon analysts' knowledge and experience. However, EMBAs are reasonable proxies for analysts in this experiment, since I investigate a phenomenon independent of characteristics of the analyst population—the effect of information structure on scenario thinking (see Libby et al. 2001). I briefed the participants in person, assigned them to experimental conditions randomly, and transmitted all experimental materials to them electronically.

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[20] Evidence about the effects of experience on forecasting ability is mixed. For example, Mikhail et al. (1997) and Clement (1999) find a significant positive association between analysts' experience and forecasting ability. However, Jacob et al. (1999) find that, although analysts differ in average forecasting performance, the differences appear to be related to analysts' aptitude and brokerage firms' characteristics, not to experience.

Case Materials and Procedures

The case materials and procedures were *identical* to those for the prior-loss condition in Experiment 1. I manipulated the independent variables in a 1 × 3 between-subjects experiment with the sign of prior earnings (loss)[21] and the structure of information (scenario, list, and list revised-to-preserve-content) as independent variables. The information-structure manipulations for the scenario and list conditions were identical to those for Experiment 1.

In Experiment 1, I maximize differences between experimental groups by manipulating two important attributes of scenarios: the presence of concrete details and the use of causal orderings to link current states, planned actions, and anticipated future outcomes. To separate the effects of manipulating these two attributes jointly, I prepared for Experiment 2 the list revised-to-preserve-content condition that presents, in list structure, *all* of the information provided in the scenario condition. Thus, *only* the use of causal links and orderings differs between this condition and the scenario condition, and *only* the presence of concrete details differs between this condition and the list condition.[22]

Dependent Measures and Expected Results

Consistent with Experiment 1, the EPS value for the current year (i.e., year 0) was –$0.64 and the main dependent measures are participants' forecasted earnings changes from the year 0 value for both one and two years ahead.[23] If the differences between forecasted earnings changes of list-condition and scenario-condition participants in Experiment 1 were due *solely* to information content, then I would expect to find differences between the list condition and the list revised-to-preserve-content condition, but no differences between the list revised-to-preserve-content condition and the scenario condition in Experiment 2. Alternatively, if the Experiment 1 results were due *solely* to information structure, then I would expect to find no differences between the list condition and the list revised-to-preserve-content condition, but I would expect to find differences between the list revised-to-preserve-content condition and the scenario condition in Experiment 2.

Results

Table 3, Panel A, reports means, standard deviations, cell sizes, and forecasted EPS changes for year 1 and year 2 revised forecasts. Table 3, Panel B, reports tests for information content and information structure differences across experimental conditions. Planned contrasts between the list and the list revised-to-preserve-content conditions reveal no significant difference ($t_{39} = 0.31$, $p = 0.38$ and $t_{39} = 0.64$, $p = 0.26$, p-values one-tailed,

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[21] Enrollment in the EMBA program was inadequate to provide sufficient numbers of participants to test both sign-of-prior-earnings conditions. As a result, Experiment 2 focuses on the condition most important to the conclusions of the study: the prior-loss condition.

[22] The materials provided to participants in the list revised-to-preserve-content condition and a reconciliation of the condition's language to both the scenario and the list conditions (Experiment 1) are available from the author.

[23] I excluded data for seven of the 49 participants from the analysis because their forecasted earnings changes exceeded the overall mean by four standard deviations. DFFITS and DFBETA procedures indicate that these observations are extreme outliers.

TABLE 3
Experiment 2: Descriptive Statistics and Tests for Information Content and Information Structure Differences

Panel A: Descriptive Statistics

		Mean [standard deviation]			
Information Structure	*n*	*Revised Forecast Year 1*[a]	*Forecasted Year 1 EPS Change*[b]	*Revised Forecast Year 2*[a]	*Forecasted Year 2 EPS Change*[b]
List	14	(0.52)	0.12 [0.17]	(0.25)	0.39 [0.37]
List Revised-to-Preserve-Content	16	(0.49)	0.15 [0.30]	(0.35)	0.29 [0.42]
Scenario	12	(0.28)	0.36 [0.27]	(0.01)	0.55 [0.42]

Panel B: Tests for Information Content and Information Structure Differences[c]

Planned Contrasts of: List vs. List Revised-to-Preserve-Content—Information Content Test	*df*	*t-statistic*	*p-value*[d]
Forecasted Year 1 EPS change $\mu_1 < \mu_{1\text{-rev}}$	1	0.31	0.38
Forecasted Year 2 EPS change $\mu_1 < \mu_{1\text{-rev}}$	1	0.64	0.26

Planned Contrasts of: List Revised-to-Preserve-Content vs. Scenario—Information Structure Test	*df*	*t-statistic*	*p-value*[d]
Forecasted Year 1 EPS change $\mu_{1\text{-rev}} < \mu_s$	1	2.16	0.02
Forecasted Year 2 EPS change $\mu_{1\text{-rev}} < \mu_s$	1	1.70	0.05

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[a] Revised forecast year t = EPS forecast made after the information-structure manipulation for year t with t = 1 or 2.

[b] Forecasted year t EPS change = Revised forecast year t − EPS value provided to participants for year 0 (i.e., revised forecast year t − ($0.64)) with t = 1 or 2.

[c] μ represents the cell mean for the information structure (l = list, l-rev = list revised-to-preserve-content, s = scenario) condition.

[d] All reported p-values are one-tailed.

for years 1 and 2, respectively), suggesting that information-content differences likely did not drive the Experiment 1 results. Moreover, planned contrasts reveal that the mean forecasted earnings change for the scenario-condition participants is significantly more positive than for the list revised-to-preserve-content-condition participants ($t_{39} = 2.16$, $p = 0.02$ and $t_{39} = 1.70$, $p = 0.05$, p-values one-tailed, for years 1 and 2, respectively). These findings suggest that analysts likely viewed the forecast-relevant information content as equivalent in the list and scenario conditions, and thus, differences in information structure, not content, likely drove Experiment 1 results.

V. CONCLUSION

Archival research reports that analysts' forecasts of annual earnings are optimistic, particularly for firms reporting losses. I use an experiment to test whether scenario thinking leads analysts to issue *unintentionally* optimistic forecasts. The results of my experiment suggest that using scenarios to provide analysts with information about a manager's plans to increase future earnings causes optimism in analysts' forecasts for two years ahead, but not for one year ahead. Specifically, when information about a manager's plans is framed in scenarios, analysts exhibit behavior consistent with scenario thinking and make more optimistic forecasts than when the same information is framed in lists. Regardless of how information about a manager's future plans is framed, analysts make more optimistic forecasts for a prior-loss than for a prior-profit firm. Finally, scenario-induced optimism is greater for a firm that reports prior losses than for a firm that reports prior profits.

The results for two-year-ahead forecasts offer evidence that scenario thinking creates patterns of optimism in analysts' annual earnings forecasts analogous to patterns observed in archival studies (i.e., greater optimism for firms reporting prior losses). Failure to observe the predicted pattern in one-year-ahead forecasts is not surprising, since participants reasonably could have expected that the manager's plans to increase future earnings might take more than one year to materialize.

This study is relevant to financial analysts because the results demonstrate that the *form* in which managers communicate information can lead analysts to issue unintentionally optimistic forecasts, particularly for loss firms. Prior archival research shows that the incidence of firms reporting losses is high. For example, Hwang et al. (1996) report that approximately one-fourth of all companies reported losses (negative earnings) in at least one year over the prior two decades, and Butler and Saraoglu (1999) report that analysts overestimate actual earnings for loss firms approximately 72 percent of the time. In short, there is room for significant improvement in analysts' forecasts for firms reporting losses, and this study identifies one potential source of decision error. Moreover, the Experiment 2 results with EMBA participants suggest that the effects of scenario thinking on judgment may be a broadly relevant phenomenon. Investors, like analysts, are confronted with scenarios describing managers' future plans in annual reports and in the business press.

Several limitations affect the interpretability of my results. Analysts work in a complex environment and face incentives not easily replicated in the laboratory. For example, although the participants reported that they used thought processes similar to those they use in practice, the experimental materials were by necessity more simplistic than the participants would experience in practice, allowing me to limit time demands to approximately 20 minutes. While the use of a hypothetical firm in the instrument provides necessary control over the financial and background information provided to participants, lack of an earnings realization precludes me from calculating an absolute measure of forecast optimism. Given that this study focuses on the incremental effect of scenario thinking on analysts' forecast optimism, inferences are not constrained by the use of a relative measure. However, I sacrificed some experimental control by both emailing and mailing materials to participants, and this may have added noise that decreased the power of my hypothesis tests.

Future research could examine how to mitigate the effects of scenario thinking on analysts' forecasts. Because the optimism inherent in scenario thinking is an association-based error (Arkes 1991), constraining the optimism would require behavior that activates different associations. For example, there is considerable evidence that counterexplanation (i.e., offering an opposing or alternative explanation) reduces the incremental confidence caused by an initial explanation task (see Koehler 1991 for a review).[24] Similarly, envisioning an opposing or alternative scenario could mitigate the effects of scenario thinking by increasing the plausibility and availability of alternative outcomes.[25]

In this study, I provide analysts with positive scenarios, and thus the results may not generalize to negative scenarios. Some managers routinely provide analysts with pessimistic scenarios.[26] One question this study raises is whether analysts confronted with negative scenarios make more pessimistic forecasts for firms reporting prior losses than for firms reporting prior profits. I leave this question for future research.

APPENDIX

Experiment 1

Scenario Condition and List Condition

Experiment 1 investigates whether the structure of information about a manager's plans to increase future earnings influences analysts' forecast optimism and, in particular, whether the influence is greater for a firm reporting prior losses than for a firm reporting prior profits. The experiment is a 2 × 2 between-subjects design: 2 (prior-earnings conditions: *profit, loss*) × 2 (information-structure conditions: *scenario, list*). This appendix reports the scenario (Panel A) and list (Panel B) conditions.

Panel A: Scenario Condition

Villa Fiore Winery, Inc. hosted a conference call on July 19, 2000 following its earnings announcement for the fiscal year ended June 30, 2000. The following is an excerpt from the remarks made by Villa Fiore's Chief Executive Officer (CEO) during the conference call to provide conference call participants with information about the industry, the winery, and management's plans and expectations for the upcoming fiscal year (year ending June 30, 2001).

> Chief Executive Officer: "We are pleased with our financial results for the fiscal year ended June 30, 2000, and we have plans to further improve financial performance in the upcoming year.[27] The strong economy and the publicity given to the health benefits of moderate wine consumption continue to fuel consumer demand for wine. In fact, industry-wide sales of red wines have more than doubled in the past few years. Because Villa Fiore specializes in red wines, we believe this growing consumer demand provides us with an opportunity to substantially increase future sales. Therefore, in fiscal year 2001, we plan to focus on increasing the visibility and exposure of our wines. This upcoming year, for the first time, you will see our high-end premium wines featured in elaborate, full-page advertisements in important industry publications like *Wine Spectator*. Additionally, we are working to expand our distribution channels to increase our presence in restaurants and retail outlets nationally. As a result of these efforts, our

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[24] Koonce (1992) and Kennedy (1995) examine counterexplanation in an auditing context.

[25] Heiman (1990) examines the effect of considering alternative explanations on auditors' likelihood assessments.

[26] For example, "[Microsoft's] executives treat analysts to a constant patter of cautionary and even downbeat words about the future. After a typically grim presentation by CEO Bill Gates and sales chief Steve Ballmer at an analysts' meeting two years ago, Goldman Sachs analyst Rick Sherlund ran into the pair outside and said, 'Congratulations. You guys scared the hell out of people.' Their response? 'They gave each other a high five,' Sherlund recalls" (Fox 1997).

[27] For the prior-loss conditions this sentence read, "We are disappointed in our financial results for the fiscal year ended June 30, 2000, but we have plans to improve financial performance in the upcoming year." All other sentences were identical.

high-end premium wines will be debuting in several gourmet restaurants including Napa Valley's *French Laundry*, San Francisco's *Rubicon*, and New York's *Aquavit*.

In addition to enhancing the visibility of our high-end premium wines, we plan to broaden our market appeal by releasing a new product in fiscal year 2001...our Villa Fiore Red Table Wine. Last year, we began purchasing bulk wine from independent growers in Napa Valley that we blended with some of our own wine to create a unique red table wine. Because purchasing bulk wine was less expensive than growing and harvesting it ourselves, we will be able to release our Villa Fiore Red Table Wine with a suggested retail price of approximately $10 per bottle. We hope to increase the strength of the Villa Fiore brand name by attracting new consumers with preferences for high-quality, lower-priced wines. To ensure the success of our new product, we are negotiating with several potential strategic partners that specialize in the national marketing of popular-priced premium wines.

In summary, we expect that the targeted marketing of our existing high-end premium wines, complemented by the introduction of our popular-priced premium red table wine, will allow us to capture a wider range of the growing consumer wine market, increase our sales substantially, and as a result, increase our profits."

***Panel B: List Condition*[28]**

Villa Fiore Winery, Inc. hosted a conference call on July 19, 2000 following its earnings announcement for the fiscal year ended June 30, 2000. The following statements highlight certain remarks made by Villa Fiore's Chief Executive Officer (CEO) during the conference call to provide conference call participants with information about the industry, the winery, and management's plans and expectations for the upcoming fiscal year (year ending June 30, 2001).

- Will begin to advertise high-end premium wines in important industry publications
- Industry-wide sales of red wines have more than doubled in past few years
- High-end premium wines will debut in several gourmet restaurants
- Will release new product—red table wine
- Hope to increase brand-name strength
- Expect substantial increase in sales
- Suggested retail price approximately $10 per bottle for red table wine
- Working to expand distribution channels nationally
- Strong economy
- Negotiating with potential strategic partners that market popular-priced premium wines
- Red table wine is a premium blend made with some purchased bulk wine
- Expect increase in profits

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[28] To control for order effects, four different orderings were used in the list conditions and the last bulleted item was identical in all orderings.

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Insider Trading, Earnings Quality, and Accrual Mispricing

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ABSTRACT: This paper investigates whether insider trading is informative about earnings quality and the valuation implications of accruals. We show that (1) the one-year-ahead persistence of income-increasing accruals is significantly lower when accompanied by abnormal insider selling and greater when accompanied by abnormal insider buying; (2) the accrual mispricing phenomenon observed in previous work (e.g., Sloan 1996) is due to the mispricing of income-increasing accruals; (3) one-year-ahead hedge returns to trading strategies based on the direction of accruals and insider trading significantly exceed those based on accruals alone; and (4) the lower persistence of income-increasing accruals accompanied by abnormal insider selling appears to be at least partly attributable to opportunistic earnings management. Our evidence suggests that market participants and researchers can use managers' contemporaneous trading in *ex ante* assessing the likelihood that the firms' accruals are of high or low quality, and in assessing the likelihood of earnings management. Our evidence suggesting that insiders trade on their knowledge of factors associated with accrual persistence is also relevant to policymakers charged with regulating insider trading.

Keywords: *accrual mispricing; insider trading; earnings quality; earnings management.*

Data Availability: *Data used in this study are available from public sources identified in the study.*

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I. INTRODUCTION

This paper investigates whether insider trading is informative about earnings quality (defined as the likelihood that a firm can sustain current earnings in the future) and the valuation implications of accruals. Recent evidence suggests that even sophisticated market participants such as analysts and auditors do not fully understand the persistence and valuation implications of accounting accruals.[1] Two possible explanations for these findings include (1) the relation between the firm's accrual-generating process and future earnings is sufficiently complex that investors fail to identify the transitory nature of the accruals (Thomas and Zhang 2002), or (2) managers opportunistically manage earnings and investors fail to recognize until later periods that accruals are less persistent than operating cash flows (Bradshaw et al. 2001; DeFond and Park 2001; Xie 2001; Thomas and Zhang 2002; Hribar 2000; Tarpley 2000). Whether they manage earnings or not, a firm's top executives likely possess private information regarding the underlying economic factors associated with the nature and persistence of accounting accruals, and we propose that their trading is informative *ex ante* about their firm's earnings quality. Therefore, we examine (1) whether firm-periods in which the direction of the accruals contradicts the direction of the insider trading have lower earnings quality, (2) whether firm-periods in which the direction of the accruals confirms the direction of the insider trading have higher earnings quality, and (3) whether the market differentially misprices high vs. low quality accruals.

Our evidence is based on a sample of 21,678 firm-years from 3,906 firms with December fiscal year-ends over the period 1985–1996. Our analysis proceeds as follows. First, we evaluate whether a signal based on insider trading predicts one-year-ahead earnings persistence and can thereby help identify firms with high and low earnings quality. Second, following Sloan (1996), we use market pricing tests (Mishkin 1983) and hedge portfolio tests to examine whether the extent of accrual mispricing differs between firms with high vs. low earnings quality. Third, we examine the extent to which low earnings quality results from earnings management.

Our earnings persistence tests reveal that income-increasing accruals are significantly more persistent for firms with abnormal insider buying and significantly less persistent for firms with abnormal insider selling, relative to firms for which there is no abnormal insider trading.[2] In contrast, we find that insider trading provides little indication of the persistence of income-decreasing accruals. Our results suggest that insider trading information is useful in *ex ante* identifying whether firms' income-increasing accruals are high or low quality.

Our pricing tests reveal that (1) accrual mispricing is primarily due to the mispricing of income-increasing accruals, (2) income-increasing accruals appear to be overpriced when managers engage in abnormal selling and rationally priced when managers engage in abnormal buying, (3) market participants also overprice income-increasing accruals when a firm's top executives do not trade, but the overpricing is less severe than when managers engage in abnormal selling, and (4) market participants appear to price all income-increasing accruals as though they are of high quality. Our findings suggest that investors fail to interpret insider selling information correctly, perhaps because it is difficult to distinguish information-motivated from liquidity-motivated selling, or because investors discount insider selling signals that are contrary to their beliefs about a firm's earnings.[3]

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[1] In his seminal study, Sloan (1996) shows that the cash flow component of earnings is more persistent than the accrual component of earnings, and that investors do not understand the differential persistence and overweight the accrual component relative to the cash flow component. Several studies following Sloan (1996) provide evidence consistent with accrual mispricing. For example, Bradshaw et al. (2001), DeFond and Park (2001), Xie (2001), and Hribar (2000) document that investors do not fully understand the valuation implications of accruals, unexpected accruals, and accrual components; Collins and Hribar (2000) show that accrual mispricing appears to be distinct from post-earnings announcement drift; Ali et al. (2000) document that the accrual phenomenon is not related to firm size, analyst coverage, institutional ownership, volume, or transaction costs. Barth and Hutton (2001) find that analysts revise their earnings forecasts more frequently for firms with low earnings persistence, but that market participants nonetheless misprice accruals.

[2] When we find similar results for total accruals, current accruals, and their unexpected counterparts estimated using various models of accrual expectations, we use the term *accruals* to designate all of these constructs.

Our hedge-portfolio tests reveal that hedge strategies based on a short position in firms with the highest accruals accompanied by abnormal insider selling, and three alternative long positions, earn significant one-year-ahead returns ranging from 16 to 25 percent. We find that hedge returns on strategies that combine accruals and insider selling are systematically higher than those based on accruals alone. Further analysis suggests these hedge returns are not simply manifestations of market-to-book, earnings yield, size, price momentum, beta, or earnings surprise effects. Taken together, these results confirm that insider trading is useful in *ex ante* assessing the persistence of income-increasing accruals, and suggest that insiders trade on their knowledge of factors associated with the persistence of accruals.

Our evidence of accrual mispricing suggests that investors fail to understand the lower persistence of certain income-increasing accruals. This lower persistence can be attributed to either (1) changes in the firm's economic environment that render accruals less informative about one-year-ahead earnings, or (2) managers engaging in opportunistic earnings management. It is difficult to distinguish between these two possibilities because accruals are *a priori* not estimates of earnings management, and unexpected accruals measure managerial discretion with error (e.g., see McNichols 2000; Beneish 2001). We conduct further tests that focus on firms with income-increasing accruals accompanied by abnormal insider selling because our results suggest these firms' accruals are the most overpriced by the market. Specifically, we show that firms with income-increasing accruals accompanied by abnormal insider selling have (1) performance-matched abnormal accruals (Kothari et al. 2001) suggestive of upward earnings management, (2) a higher propensity to make income-increasing accounting choices, and (3) a higher propensity to report profits and year-to-year increases in earnings. The evidence suggests that the lower persistence of income-increasing accruals accompanied by abnormal insider selling is at least partly attributable to opportunistic earnings management.

The study provides some of the first insight into the specific information that insiders appear to trade on. The results suggest that some portion of insider trading is associated with insiders' advance knowledge of the economic factors underlying the lack of persistence of income-increasing accruals. Investors can use abnormal insider selling as a signal of lower quality of certain income-increasing accruals, if they can disentangle information-motivated selling from insider selling for liquidity or portfolio-rebalancing purposes.

Our results suggest that investors would benefit from increased proxy statement or Form 10-K disclosures about trading by firms' top executives during the fiscal year. Of course, our analysis provides insights only into potential benefits to investors of increased disclosure—we provide no evidence on the costs of expanding insider trading disclosure. If policymakers determine that the benefits to investors outweigh the costs to executives and firms, then they may consider expanded disclosures of top executives' trading, similar to existing compensation disclosures. For example, such disclosures could include (1) the aggregate amount of open market transactions, (2) the corresponding amount of trading for a group of comparable firms, and (3) an indication of the profit forgone or loss avoided from the time of the executives' trading to the fiscal year-end.

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[3] For evidence of confirmatory bias in which investors discount contradictory information, see Gilovich (1991) and Hirshleifer (2001).

Our findings have two potential implications for future research on the persistence and valuation of accruals. First, although prior work suggests that the accrual mispricing phenomenon is due to current accruals (Bradshaw et al. 2001; Thomas and Zhang 2002), or to the unexpected component of accruals (Xie 2001), our results suggest that it may be fruitful to focus on the income-increasing component of those accruals. Second, our evidence suggests that opportunistic earnings management partially accounts for investors' failure to understand the low persistence of income-increasing accruals that are accompanied by abnormal insider selling. Although this evidence only pertains to the abnormal insider selling subsample, the accumulation of evidence in Beneish (1999), Beneish et al. (2002), and in this study, is consistent with some managers inflating earnings so they can sell their firms' equity at higher prices. Thus, future research could investigate the extent to which insider trading opportunities provide an incentive for earnings management.

We present the remainder of the paper in five parts. The next section discusses our hypotheses. Section III describes the data and method, and Section IV presents the empirical results. Section V reports several robustness checks, and Section VI concludes.

II. HYPOTHESES

Managers who make strategic operating decisions likely possess private information regarding economic factors underlying the likelihood that accounting accruals will result in future earnings. For example, an increase in receivables could mean that sales are increasing and point to solid future sales growth. On the other hand, increasing receivables could be the result of actions, such as relaxing credit checks or granting easier credit terms, taken to avoid reporting lower sales growth. Hence, we expect that managers have private information about the likelihood that income-increasing accruals will result in higher future earnings, and the likelihood that income-decreasing accruals will result in lower future earnings.

Income-Increasing Accruals

If managers expect higher reported earnings to persist and lead to higher future stock prices, then they have incentives to purchase their firm's stock. Indeed, prior research confirms that insiders buy before stock price increases.[4] Investors may interpret such insider buying as a signal of high earnings quality because insiders make relatively few open market purchases, usually acquiring shares through stock option and long-term performance plans. Thus, we expect managers privy to positive information about the effect of income-increasing accruals on their firm's prospects either to strategically increase their stake in the firms' equity or to abstain from selling.

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[4] Prior research suggests that insiders buy before stock price increases, and sell before stock price declines (see e.g., Jaffe 1974; Finnerty 1976; Baesel and Stein 1979; Givoly and Palmon 1985; Seyhun 1986; Rozeff and Zaman 1988; Seyhun 1998). Further, these studies show that it takes time for prices to reflect the information contained in insider trading. For example, Finnerty (1976), Baesel and Stein (1979), Givoly and Palmon (1985), Rozeff and Zaman (1988), and Seyhun (1998) find insignificant returns associated with insider trading from three to six months following the transaction. Indeed, these studies suggest that outside investors seeking to mimic insider trading patterns ought to hold their positions for longer periods in order to generate returns that exceed transaction costs. Because this body of research suggests that insider trading is a leading rather than a contemporaneous indicator of firm performance, we examine the relation between accrual persistence conditional on insider trading and *future* rather than contemporaneous firm performance.

Alternatively, if the income-increasing accruals arise because managers manipulate earnings to hide deteriorating firm performance, then we expect managers to act on their knowledge that the accruals are unlikely to persist and sell their firm's stock. Prior research concludes that managers charged with GAAP violation overstate earnings to delay stock price declines so they can sell their equity-contingent wealth at higher prices (Beneish 1999), and more generally, that insiders sell before price declines. Although managers attempting to mislead investors may restrict their selling to avoid attracting the attention of the SEC's insider trading monitors, two forces work against such self-restriction. First, if managers successfully delay the decline in stock prices, then the time gap between any insider selling and the subsequent release of bad news lengthens, which in turn reduces the likelihood of SEC scrutiny. Second, and more importantly, the SEC rarely imposes trading sanctions, even on insiders who sold stock and were charged with fraudulently overstating earnings (Beneish 1999).[5] Thus, we expect managers privy to negative information about the effect of income-increasing accruals on their firms' prospects, to sell their firms' equity. The preceding discussion leads to the following hypotheses:

H1: Income-increasing accruals or income-increasing unexpected accruals are more persistent when accompanied by abnormal insider buying.

H2: Income-increasing accruals or income-increasing unexpected accruals are less persistent when accompanied by abnormal insider selling.

Our third hypothesis posits that the mispricing of accruals is associated with the extent of insider trading. As in Sloan (1996), we investigate whether investors appear to fixate on earnings and fail to understand the differential persistence of accrual and cash flow components of earnings. Specifically, we evaluate whether investors distinguish that income-increasing accruals associated with abnormal insider buying are more persistent (and that accruals associated with abnormal insider selling are less persistent) than accruals associated with no abnormal insider trading. Our hypothesis, patterned after Sloan's (1996, 292), is as follows:

H3: The earnings expectations embedded in stock prices fail to fully reflect the higher persistence of income-increasing accruals or income-increasing unexpected accruals associated with abnormal insider buying, and the lower persistence of income-increasing accruals or income-increasing unexpected accruals associated with abnormal insider selling.

Income-Decreasing Accruals

Our predictions for income-decreasing accruals parallel those for income-increasing accruals. If managers expect income-decreasing accruals to persist and subsequently lead to lower stock prices, then they have incentives to sell their firms' stock before prices decline. Therefore, we expect income-decreasing accruals accompanied by abnormal selling to be associated with higher earnings persistence and lower future returns.

On the other hand, if managers expect income-decreasing accruals to reverse and lead to higher earnings and higher stock prices, then they have incentives either to increase their stake in their firms' equity or to abstain from selling. Therefore, we expect income-decreasing accruals accompanied by abnormal buying to be associated with lower earnings persistence and higher future returns. We describe our expectations in the following alternative-form hypotheses:

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[5] In the event of detection, managers may also be able to justify their selling for personal liquidity reasons. Evidence that firms cut managers' compensation in times of financial distress (Gilson and Vestuypens 1993) could also justify managers' sales of shares to maintain current consumption.

H4: Income-decreasing accruals or income-decreasing unexpected accruals are less persistent when accompanied by abnormal insider buying.

H5: Income-decreasing accruals or income-decreasing unexpected accruals are more persistent when accompanied by abnormal insider selling.

H6: The earnings expectations embedded in stock prices fail to fully reflect the lower persistence of income-decreasing accruals or income-decreasing unexpected accruals associated with abnormal insider buying, and the higher persistence of income-decreasing accruals or income-decreasing unexpected accruals associated with abnormal insider selling.

III. METHOD

Sample Selection

The initial sample included all firms on the Compustat Industrial, Research, and Full Coverage 1998 tapes for the period 1985 to 1997. We then eliminated: (1) financial services firms (SIC codes 6000–6700), (2) non-December fiscal year-end firms,[6] (3) firms with insufficient data to compute accruals, and (4) firms with discontinued operations (Compustat #66) exceeding 5 percent of total assets.[7]

To ensure sufficient data for the pricing and returns tests, we eliminated: (1) firms with missing return data on CRSP, (2) firms delisted prior to the May 1 portfolio formation date, (3) firms that begin trading after May 1 in any given year because we could not trade these firms at the time of portfolio formation, and (4) American Depository Receipts (ADRs), because CRSP does not compute a market-capitalization-size decile for ADRs and thus size-adjusted returns are not available. Our final sample consists of 21,678 firm-years from 3,906 firms over the period 1985–1996.

Insider Trading, Abnormal Buying, and Abnormal Selling

Section 16(a) of the Securities Exchange Act of 1934 requires all insiders (officers, directors, and 10 percent beneficial owners) to disclose transactions in their firm's equity securities on SEC Form 4 on or before the 10th day of the month after the event. We obtain insider trading data from the National Archives Center for Electronic Records and from Thomson Financial. Similar to prior research: (1) we focus on open market (public) transactions because reputation effects likely preclude insiders from opportunistic trading where both parties are known, and (2) we omit transactions of fewer than 100 shares because these are unlikely to represent information-motivated trading (Penman 1982, 1985; Givoly and Palmon 1985).

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[6] We focus on December fiscal year-end firms so that our subsequent portfolio assignments in the pricing tests yield tradable investment strategies. That is, we base our hedge return tests on a trading strategy that requires an annual ranking of firms according to accruals and insider trading activity. Our annual ranking occurs on April 30 (the day before portfolio formation), when financial data and insider trading data for the previous fiscal year are likely available for all sample firms.

[7] We remove firms with significant reclassified operations because their prior year's financial data in Compustat are not adjusted, so accrual computations relying on lagged data would be distorted.

Prior insider trading research has documented that higher-level insiders possess more valuable information and earn larger abnormal returns when trading (Baesel and Stein 1979; Nunn et al. 1983; Seyhun 1986, 1998; Lin and Howe 1990). Therefore, our measure of insider trading uses the transactions of the top five executives (CEO, CFO, COO, president, and chairman of the board). Following prior insider trading research, we compute a firm-specific, insider trading metric Net Shares Traded (NST) as:

$$\text{NST} = \Sigma\ (\text{Shares Purchased}_t/\text{SHARES\_OUT}_t) - \Sigma\ (\text{Shares Sold}_t/\text{SHARES\_OUT}_t)$$

where the sum is over each firm's senior executives over all days t in the calendar year, and where SHARES_OUT is the number of shares outstanding on the date of the insider transaction.[8] We define net shares traded in terms of the proportion of the firm the insider traded to control for cross-sectional variation in the level of shares outstanding. For subsequent sensitivity analysis we also compute a second metric, Insider Consensus, which uses a simple voting scheme to compare the number of insiders selling shares to the number of insiders buying shares.[9]

Table 1 presents descriptive statistics on the aggregate level of insider trading and our two insider trading measures. Panel A shows that the frequency and volume of insider selling exceed those of insider buying. The number of sales is twice the number of purchases, and the number of shares sold (1.02 billion) outnumbers those purchased (0.35 billion) by almost 3:1. Prior insider trading research systematically documents more frequent insider selling (e.g., among others, Jaffe 1974; Finnerty 1976; Givoly and Palmon 1985; Seyhun 1986; Rozeff and Zaman 1988; Lin and Howe 1990; Seyhun 1998), and attributes such dominance to the fact that insiders acquiring shares have many alternatives to open market purchases (e.g., options, restricted stock awards, performance plans awards, dividend unit awards) while share disposals occur mostly through open market sales. Panel B shows that net selling firm-years (18.6 percent) are more frequent than net buying firm-years (12.1 percent), and that senior executives do not trade in nearly 70 percent of the firm-years. Panel B also shows that a firm's senior executives generally trade in the same direction; their trading is not unanimous in only 2 percent of the sample.

To isolate "abnormal" insider trading, we incorporate prior findings that small firms have relatively more insider purchases than sales, whereas larger firms have relatively more insider sales than purchases (Seyhun 1986; Rozeff and Zaman 1988; Pope et al. 1990; Lin

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[8] Because we evaluate whether investors can exploit insider trading information to make refined assessments of accrual persistence, our measures of insider trading and accruals are contemporaneous. That is, we base our measure of insider trading in any given year on the level of trading from January 1 to December 31, the same period in which our sample firms generated earnings. Our choice of period length is motivated by the facts that (1) we require a firm-specific measure of insider trading activity, and (2) we focus on a small subset of insiders who are more likely to have information on accrual persistence. We believe that aggregating over a calendar year increases the likelihood of observing these executives' information-motivated trading. By choosing December 31 as the period-end, we increase the likelihood that insider trading information is publicly available at the time we form portfolios (May 1 of the following year). Because our choice of when to begin our insider trading measurement period is arbitrary, we also repeated our analysis using insider trading activity in just the last six months of the calendar year. This shorter insider trading window leads to similar results.

[9] Most insider trading research uses variations on the net shares (Penman 1982, 1985; Elliot et al. 1984; Seyhun 1986) and insider consensus metrics (Lorie and Niederhoffer 1968; Jaffe 1974; Finnerty 1976; Madden 1979). Insider consensus is no longer widely used because it ignores the magnitude of shares traded. For example, a firm would not be a consensus-selling firm if the CEO sold 20 million shares while the CFO purchased 200 shares.

TABLE 1
Descriptive Statistics for Insider Trading by Senior Executives of 3,906 Firms with December Fiscal Year-Ends between 1985 and 1996
(21,678 firm-years)[a]

Panel A: Aggregate Insider Trading Statistics

Number of firms	3,906	
Number of unique insiders with one or more transaction[b]	6,735	
	Purchases	*Sales*
Open market transactions	9,376	20,792
Firm-years	2,940	4,162
Shares traded (in millions)	353	1,020

Panel B: Insider Trading Metrics

	Firm-Years	*Percent of Sample*
Net Shares Traded (NST)[c]		
Net selling firms	4,020	18.6
Net buying firms	2,617	12.1
No trading	15,041	69.4
Insider Consensus[d]		
Unanimous selling	3,711	17.1
Majority selling, not unanimous	50	0.2
Equal (nonzero) sellers and buyers	366	1.7
Unanimous buying	2,489	11.5
Majority buying, not unanimous	35	0.2
No trading	15,027	69.3

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[a] Senior Executives are the firm's top five executives: CEO, CFO, COO, President, and Chairman of the Board.

[b] Transactions consist of open market purchases and sales (codes "P" and "S" on SEC Form 4). We exclude transactions of less than 100 shares, warrants, and convertible securities.

[c] Net Shares Traded (NST) = Σ (Shares $\text{Purchased}_t/\text{SHARES\_OUT}_t$) $-$ Σ (Shares $\text{Sold}_t/\text{SHARES\_OUT}_t$) summed over the firm's top five executives for each day t, and then summed over all days t in the calendar year; and SHARES_OUT_t = the number of shares outstanding on the date of the insider transaction.

Net selling firms are those where NST < 0. Similarly, net buying firms are those where NST > 0.

[d] Insider Consensus counts the number of insiders purchasing shares relative to the number of insiders selling shares. Unanimous selling occurs when one or more of the firms' insiders sell shares while no insiders purchase shares in a given year. Majority selling means that more insiders sold than bought shares during the year. There are only 15,027 observations in "No Trading" because in 14 cases insiders omit the number of shares in their filing.

and Howe 1990).[10] Thus, we first compute the annual NST for net selling and net buying firms using all firms in the available CRSP population for each size decile. We then define a size-adjusted insider trading measure for each net selling (buying) firm-year by comparing its level of insider trading to the median level of NST for all similar-sized net selling (buying) firms measured over the same year. Our computation of abnormal insider trading helps control for the effect of differential stock-compensation intensity to the extent that such intensity is a function of size (Smith and Watts 1992; Gaver and Gaver 1993), and is more likely to isolate information-motivated trading.[11] We categorize firm-years as:

Abnormal Selling = 1 if $NST < 0$ and NST is lower than the median of all net selling firms in the same size (market value) decile, and 0 otherwise; and

Abnormal Buying = 1 if $NST > 0$ and NST is greater than the median of all net buying firms in the same size (market value) decile, and 0 otherwise.

Because insider selling by top executives is least likely in the smallest size deciles and insider buying is far less frequent in the largest size deciles, we alter the above definition in the extreme deciles. That is, we categorize all net selling firms in size deciles 1 and 2 as abnormal selling firms and all net buying firms in size deciles 9 and 10 as abnormal buying firms.

Sample Characteristics

Table 2 presents the characteristics of our sample by accrual decile, as in Sloan (1996), to allow comparisons across studies and because some of our hedge returns tests exploit such partitioning. Panel A describes the components of earnings and, consistent with Sloan (1996), reveals evidence of a negative correlation between accruals and cash flows (untabulated Pearson correlation –0.316, p-value < 0.001). The mean (median) value of cash flow falls from 0.116 (0.178) in the lowest accrual decile to –0.106 (–0.045) in the highest accrual decile. Panel A also reveals evidence of a positive correlation between accruals and unexpected accruals estimated using the Dechow et al. (1995) modification of the Jones (1991) model. The (untabulated) correlation between total accruals and unexpected total accruals is 0.845 (p-value < 0.001), and the correlation between current accruals and unexpected current accruals is 0.873 (p-value < 0.001).

Panel B of Table 2 compares the financial characteristics of firms in the lowest and highest accrual deciles. Relying on median tests, we find that firms in the lowest accrual decile are smaller, more levered, have lower returns in the prior year, lower earnings-to-price ratios, lower market-to-book ratios, and lower betas. These differences lead our subsequent analysis to evaluate whether the returns to our hedge strategies result from taking positions in firms that have other characteristics that are correlated with subsequent returns.

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[10] Prior research, which used *all* insider trades, documents a negative correlation between net insider trading and firm size. We find a similar result for the insider trades of the firms' top five executives. For example, in the smallest two deciles of market capitalization, 66.8 percent of firms trading are net buying firms, 33.2 percent are net selling firms, and the average Net Shares Traded is 1.03 percent. In contrast, we find that, in the highest two deciles of market capitalization, 30.8 percent of firms trading are net buying firms, 69.2 percent are net selling firms, and the average NST is –4.80 percent. The average NST for the two groups is significantly different ($t = 10.54$, $p = 0.001$).

[11] Alternatively, one could compare a firm's NST to the median NST of all firms in the same CRSP size decile. However, because trading by the top five executives is relatively infrequent, the median NST for all firm size deciles is zero. Such a measure would therefore classify all firm-years with non-zero NST as abnormal traders and increase the likelihood of treating small, non-information-related trading as abnormal. Because in 98 percent of our sample the direction of top executives' trading is unanimous, our results based on *Insider Consensus* are nearly equivalent to this alternative computation. As we discuss in Section V, although the results of analyses based on *Insider Consensus* are somewhat weaker than those based on NST, the study's inferences are unaffected.

TABLE 2

Mean (Median) Values of Selected Characteristics for Ten Portfolios of Firms Formed Annually by Assigning Firms to Deciles Based on the Magnitude of Total Accruals[a]

Sample Consists of 21,678 Firm-Years between 1985 and 1996

	Lowest Accruals	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*Highest Accruals*
Panel A: Component of Earnings										
Total Accruals	−0.250	−0.118	−0.084	−0.063	−0.047	−0.033	−0.017	0.005	0.042	0.173
	(−0.206)	(−0.116)	(−0.084)	(−0.064)	(−0.047)	(−0.033)	(−0.017)	(0.005)	(0.041)	(0.136)
Current Accruals	−0.162	−0.043	−0.020	−0.008	0.002	0.010	0.023	0.043	0.082	0.209
	(−0.132)	(−0.045)	(−0.022)	(−0.008)	(−0.001)	(0.007)	(0.020)	(0.041)	(0.080)	(0.174)
CFO	0.116	0.125	0.126	0.121	0.110	0.097	0.080	0.057	0.022	−0.106
	(0.178)	(0.165)	(0.154)	(0.143)	(0.131)	(0.118)	(0.105)	(0.083)	(0.051)	(−0.045)
EARNINGS	−0.133	0.007	0.042	0.058	0.063	0.064	0.063	0.062	0.064	0.067
	(−0.044)	(0.047)	(0.070)	(0.080)	(0.086)	(0.085)	(0.086)	(0.085)	(0.092)	(0.104)
Unexpected Total Accruals[b]	−0.187	−0.062	−0.033	−0.019	−0.007	0.001	0.012	0.028	0.057	0.162
	(−0.158)	(−0.064)	(−0.035)	(−0.019)	(−0.008)	(0.003)	(0.012)	(0.027)	(0.057)	(0.139)
Unexpected Current Accruals[b]	−0.153	−0.046	−0.023	−0.013	−0.003	0.003	0.014	0.031	0.063	0.168
	(−0.129)	(−0.044)	(−0.022)	(−0.011)	(−0.003)	(0.003)	(0.013)	(0.031)	(0.064)	(0.144)

(Continued on next page)

TABLE 2 (Continued)

	Lowest Accruals	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*Highest Accruals*
Panel B: Financial and Market Characteristics										
LMVE	3.260	4.413	5.068	5.389	5.577	5.550	5.196	4.831	4.399	4.021
	(3.095)	(4.257)	(4.912)	(5.431)	(5.675)	(5.662)	(5.132)	(4.781)	(4.347)	(3.952)*
LEVERAGE	0.527	0.473	0.473	0.467	0.448	0.444	0.450	0.442	0.460	0.508
	(0.490)	(0.454)	(0.450)	(0.446)	(0.437)	(0.443)	(0.453)	(0.444)	(0.450)	(0.479)*
RET(t–1)	−0.055	0.024	0.009	0.021	0.024	0.023	0.033	0.050	0.060	0.152
	(−0.193)	(−0.057)	(−0.057)	(−0.036)	(−0.023)	(−0.020)	(−0.016)	(−0.025)	(−0.035)	(−0.050)*
EPRATIO	−0.564	−0.085	−0.072	−0.007	−0.003	0.016	0.015	0.008	−0.002	−0.030
	(−0.130)	(0.018)	(0.040)	(0.050)	(0.056)	(0.059)	(0.060)	(0.053)	(0.048)	(0.042)*
MTB	3.395	2.614	2.442	2.309	2.218	2.390	1.887	2.751	2.191	3.110
	(1.531)	(1.527)	(1.613)	(1.616)	(1.620)	(1.622)	(1.657)	(1.804)	(2.010)	(2.221)*
BETA	1.105	1.081	1.026	1.041	1.001	0.931	0.990	1.092	1.203	1.318
	(1.031)	(0.986)	(0.930)	(0.956)	(0.905)	(0.799)	(0.879)	(0.988)	(1.123)	(1.266)*
Panel C: Abnormal Insider Trading and Accrual Deciles[c]										
Proportion of Abnormal Selling Firms	7.63%	7.39%	7.94%	7.14%	7.73%	5.78%	7.27%	10.45%	12.82%	15.59%*
Proportion of Abnormal Buying Firms	8.02%	8.08%	7.57%	7.05%	8.01%	8.12%	7.36%	6.86%	6.43%	5.71%*

(*Continued on next page*)

TABLE 2 (Continued)

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* Indicates rejection of the null hypothesis of equality in medians (Panel B), and the null hypothesis of equality in proportions (Panel C) for the highest and lowest accrual deciles at the 5 percent level or lower (two tailed-test).

[a] Variable definitions:

Total accruals = the change in current assets (Compustat item #4), minus the change in cash (#1), minus changes in current liabilities (#5), plus the change in the current portion of long term debt (#34), minus depreciation and amortization expense (#14), plus change in taxes payable (#71). If items #34 or #71 are missing, then we set the item equal to 0 in calculating total accruals. If any of the other required items is missing in Compustat, then we delete the observation;

CFO = operating cash flow, computed as (Operating Income after depreciation (#178) − Total accruals) ÷ Average total assets (#6);

EARNINGS = operating Income after depreciation (#178) ÷ Average total assets (#6);

LMVE = natural logarithm of the market value (in millions) of common equity at year end = Price (#24) × Common shares outstanding (#25);

LEVERAGE = [Current liabilities (#5) + Long term debt (#9)] ÷ Average total assets;

RET(t − 1) = prior year return, the size-adjusted return (with dividends) over the prior fiscal year (CRSP);

EPRATIO = earnings-to-price ratio, computed as income before extraordinary items #18 ÷ Market value of equity (#24 × #25);

MTB = market-to-book ratio, computed as the market value of equity (#24 × #25) ÷ Book value of equity (#60); and

BETA = the slope coefficient of the regression of the firm's return on the return to the equally weighted CRSP index, estimated using daily returns over calendar year t (approximately 250 trading days).

[b] We estimate unexpected total accruals using the Jones (1991) model as modified by Dechow, et al. (1995). The Jones (1991) model relates. Total accruals (see note a) to the change in sales (#12) and the level of gross property, plant, and equipment (PPE #8) and is written as follows (all variables deflated by average total assets (#6)): Total $\text{accruals}_{it} = a_i + b_i\,\Delta\text{Sales}_{it} + c_i\,\text{PPE}_{it} + u_{it}$. We estimate the model cross-sectionally using all firms in a given two-digit SIC code industry and year. We use each yearly estimation to make one-year-ahead forecasts of expected accruals as follows; Expected $\text{accruals}_{it+1} = \alpha_i + \beta_i\,(\Delta\text{Sales}_{it+1} - \Delta\text{Receivables}_{it+1}) + \gamma_i\,\text{PPE}_{it+1}$, where the change in receivables (#2) reflects the Dechow et al. (1995) modification of the Jones model, and the Greek letters reflect estimates of the model's coefficients in the prior year. We estimate unexpected total accruals as the difference between Total accruals and Expected accruals. We estimate unexpected current accruals similarly from a model that drops the PPE variable.

[c] We measure abnormal insider trading relative to the level of insider trading in the same year by firms of similar size (CRSP-market capitalization decile) and we condition on the sign of the sample firm's net shares traded (NST) as defined in Table 1. Abnormal Buying = 1 if (1) NST > 0 and the firm is in size decile 9 or 10, or (2) NST >0 and NST is greater than the median of all buying firms of similar size, and 0 otherwise; Abnormal Selling = 1 if (1) NST < 0 and the firm is in size decile 1 or 2, or (2) NST < 0 and NST is less than the median of all net selling firms of similar size, and 0 otherwise.

Panel C of Table 2 reports the proportion of abnormal buying and selling observations by accrual decile. In the first seven accrual deciles, the proportions of abnormal buying and selling observations are similar and fairly uniform across deciles, ranging between 7 and 8 percent. However, in the three largest accrual deciles, abnormal-selling firm-years are more frequent than abnormal-buying firm-years, suggesting that large income-increasing accruals are associated with abnormal insider selling.

IV. EMPIRICAL RESULTS

Persistence and Pricing Tests

Following Sloan (1996), Xie (2001), and Hribar (2000), we use the framework proposed by Mishkin (1983), to test our hypotheses on the one-year-ahead persistence of components of current earnings, and the rational pricing of components of current earnings. Specifically, to assess whether the market rationally prices the implications of *total accruals* for one-year-ahead earnings, we estimate the following system:[12]

$$\begin{aligned}\mathrm{EARNINGS}_{t+1} = {} & a_0 + a_1\mathrm{CFO}_t + a_2\mathrm{NTPOSACC}_t + a_3\mathrm{BPOSACC}_t \\ & + a_4\mathrm{SPOSACC}_t + a_5\mathrm{NTNEGACC}_t + a_6\mathrm{BNEGACC}_t + a_7\mathrm{SNEGACC}_t \\ & + a_8\mathrm{SELL}_t + a_9\mathrm{BUY}_t + a_{10}\mathrm{MTB}_t + \varepsilon_{t+1}\end{aligned} \tag{1a}$$

$$\begin{aligned}\mathrm{SIZEADJR}_{t+1} = {} & \lambda + \beta[\mathrm{EARNINGS}_{t+1} - \alpha_0 - \alpha_1\mathrm{CFO}_t - \alpha_2\mathrm{NTPOSACC}_t - \alpha_3\mathrm{BPOSACC}_t \\ & - \alpha_4\mathrm{SPOSACC}_t - \alpha_5\mathrm{NTNEGACC}_t - \alpha_6\mathrm{BNEGACC}_t - \alpha_7\mathrm{SNEGACC}_t \\ & - \alpha_8\mathrm{SELL}_t - \alpha_9\mathrm{BUY}_t - \alpha_{10}\mathrm{MTB}_t] + e_{t+1}\end{aligned} \tag{1b}$$

where:

EARNINGS = operating income after depreciation (Compustat #178), deflated by average total assets (#6);
CFO = operating cash flow is the difference between earnings and total accruals;[13]
NTPOSACC = total accruals if total accruals are positive and there is no abnormal insider trading, and 0 otherwise;
BPOSACC = total accruals if total accruals are positive and there is abnormal insider buying, and 0 otherwise;
SPOSACC = total accruals if total accruals are positive and there is abnormal insider selling, and 0 otherwise;
NTNEGACC = total accruals if total accruals are negative and there is no abnormal insider trading, and 0 otherwise;

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[12] To conduct tests of persistence and rational pricing on *current* accruals, we modify the equations in the system by defining the accrual-based variables in terms of current accruals, and by adding Depreciation (Compustat #14) as an explanatory variable. We also conduct tests of persistence and rational pricing for unexpected total and unexpected current accruals, estimated using Dechow et al.'s (1995) modification of the Jones (1991) model. Thus, for unexpected current accruals, we further modify the system by defining the accrual-based variables in terms of unexpected current accruals and expected current accruals.

[13] We calculate total accruals as the change in current assets (Compustat #4), minus the change in cash (#1) and current liabilities (#5), plus the change in the current portion of long-term debt (#34), minus depreciation and amortization expense (#14), plus the change in taxes payable (#71), deflated by average total assets (#6). If items #34 or #71 are missing, then we set the item equal to zero in calculating total accruals because those numbers are likely immaterial for the firm in question. If any of the other required items is missing in Compustat, we drop the observation.

BNEGACC = total accruals if total accruals are negative and there is abnormal insider buying, and 0 otherwise;
SNEGACC = total accruals if total accruals are negative and there is abnormal insider selling, and 0 otherwise;
SELL = dummy variable indicating abnormal insider selling;
BUY = dummy variable indicating abnormal insider buying;
MTB = market value of equity (#24 × #25) ÷ book value of equity (#60); and
SIZEADJR = one-year-ahead size-adjusted return beginning four months after the fiscal year-end in which we measure total accruals.[14]

We include insider trading main effect variables (BUY, SELL) to control for possible effects of insider trading on the level of one-year-ahead earnings. We include the market-to-book ratio (MTB) to control for the effect of growth on one-year-ahead earnings and because prior research suggests there is a positive correlation between insider selling volume and growth (Rozeff and Zaman 1988; Pope et al. 1990; Lin and Howe 1990; Seyhun 1998).[15]

Equation (1a) estimates the *forecasting* coefficients of accruals and other earnings components (a_i) for predicting one-year-ahead earnings. Equation (1b) estimates the *valuation* coefficients (α_i) that the market assigns to accruals and other earnings components. The Mishkin framework provides a statistical comparison between the forecasting coefficients (measures of the predictive ability of current earnings components for one-year-ahead earnings) and the valuation coefficients (measures of the market's pricing of components of current earnings).

We implement the tests of rational pricing by estimating Equations (1a) and (1b) jointly using a two-stage iterative generalized nonlinear least squares procedure. We first estimate the unconstrained system, Equations (1a) and (1b), without imposing any constraints on the coefficients. In the second stage, we test whether the valuation coefficients differ from the forecasting coefficients obtained in the first stage by imposing rational pricing constraints ($\alpha_q = a_q$, for all q). Under the null hypothesis that the market rationally prices one or more earnings components with respect to their associations with one-year-ahead earnings, Mishkin (1983) shows that the following likelihood ratio statistic is asymptotically $\chi^2(q)$ distributed:

---

[14] Our one-year-ahead returns computation follows Sloan (1996). If a security delists in a particular year, we include the CRSP delisting return in our one-year ahead return computation and reinvest the proceeds in the CRSP size-matched decile for the remainder of the year. Further, as in Sloan (1996), we assume a −100 percent delisting return for securities delisted as a result of liquidation, or because firms are forced into delisting by the Stock Exchange or the SEC and CRSP codes the delisting return as missing. The results of our persistence, pricing, and hedge return tests are robust to eliminating delisted securities for which we assume a −100 percent return.

[15] Because evidence in prior research is based on *all* insiders, we examine whether a similar relation exists between insider selling by the firm's top five executives and firm growth. We use the market-to-book ratio (MTB) as a proxy for firm growth and annually rank all firms with available data on Compustat over the period 1985–1996. For the lowest MTB decile, we find that only 37.8 percent of the firms trading are net insider selling firms and their average Net Shares Traded is −1.10 percent. For the highest MTB decile, we find that 75.6 percent of the firms trading are net insider selling firms, and their average NST is −1.57 percent. The average NST does not significantly differ across groups (t = 1.36, p-value = 0.173). The evidence of a relation between insider selling by top executives and growth is thus mixed. On one hand, it suggests that an insider trading metric based on the frequency of trading would tend to classify more firms with high MTB as abnormal sellers. On the other, the evidence suggests that our classification of firms into abnormal trading groups based on the level of insider trading relative to shares outstanding (NST) would not bias our abnormal selling group toward high MTB firms. Nonetheless, we also estimate the system after including interactions of MTB with the six accrual-based variables in Equations (1a) and (1b). As discussed in Section V, the study's inferences continue to hold even after controlling for growth (MTB).

$$2 \times N \times Ln(SSR^c/SSR^u),$$

where:

q = the number of rational pricing constraints imposed;
N = the number of observations in the sample;
SSR^c = the sum of squared residuals from the constrained regressions in the second stage; and
SSR^u = the sum of squared residuals from the unconstrained regressions in the first stage.

To test our hypotheses relating to the one-year-ahead persistence of accruals, conditional on the direction of insider trading, we use F-tests to compare coefficients in the forecasting Equation (1a). (The forecasting equation coefficients estimated from the unconstrained system are identical to those obtained from estimating Equation (1a) alone using ordinary least squares.) We test H_1 and H_2 by assessing whether income-increasing accruals are more persistent when accompanied by abnormal insider buying ($a_3 > a_2$), and less persistent when accompanied by abnormal insider selling ($a_4 < a_2$). Similarly, we test H_4 and H_5 by assessing whether income-decreasing accruals are less persistent when accompanied by abnormal insider buying ($a_6 < a_5$), and more persistent when accompanied by abnormal insider selling ($a_7 > a_5$). To test the mispricing hypotheses (H_3 and H_6), we evaluate whether the persistence implicit in the pricing regression equals the historical persistence implied by the forecasting equation. For H_3, we test whether $\alpha_3 \neq a_3$ and whether $\alpha_4 \neq a_4$, and for H_6 we test whether $\alpha_6 \neq a_6$ and whether $\alpha_7 \neq a_7$.

In Table 3, we present tests that examine the one-year-ahead persistence and pricing of accruals and unexpected accruals, after conditioning on the direction of insider trading. In Panel A, we replicate Sloan (1996), and obtain similar results: our coefficient estimates of 0.860 and 0.752 for the persistence of operating cash flow and total accruals, respectively, are similar to his (0.855 and 0.765). As in Sloan (1996), an F-test rejects the null hypothesis that the coefficients are equal at the 0.001 level, and our results are thus consistent with the cash component of earnings being more persistent than the accrual component. Moreover, as in Sloan (1996), tests of equality of the forecasting and pricing coefficients suggest that the market appears to overprice accruals and underprice cash flows, as if investors overestimate the persistence of accruals while underestimating that of cash flows.

In Panel B of Table 3, we extend Sloan's (1996) test by partitioning accruals according to whether they are income-increasing or income-decreasing. Similar to Panel A, we find that income-increasing and income-decreasing accruals are less persistent than cash flows. We also find no significant difference between the persistence of income-increasing and income-decreasing accruals (F-test = 0.57, p-value = 0.452). However, the market appears to overprice income-increasing accruals; the market's assessment of persistence implicit in the pricing regression (1.002) significantly exceeds the historical persistence implied by the forecasting coefficient (0.743) (likelihood ratio = 15.31, p-value = 0.001). In contrast, we find no evidence that the market misprices income-decreasing accruals; and the market's perception of persistence implicit in the pricing regression (0.790) is not significantly different from the persistence implied by the earnings forecasting regression (0.757) (likelihood ratio = 0.42, p-value = 0.517). Because we find similar one-year-ahead persistence for income-increasing and income-decreasing accruals, we also evaluate whether the mispricing of income-increasing accruals exceeds that of income-decreasing accruals. The likelihood ratio statistic we obtain (6.22) rejects the null hypothesis that the relative mispricing is equal ($\alpha_2 - a_2 = \alpha_3 - a_3$).

TABLE 3

Tests of the Persistence and the Rational Pricing of Earnings Components Conditional on the Direction of Insider Trading[a]

Panel A: Cash Flow and Accruals

$$EARNINGS_{t+1} = a_0 + a_1 CFO_t + a_2 ACCRUAL_t + \varepsilon_{t+1}$$

$$SIZEADJR_{t+1} = \beta(EARNINGS_{t+1} - \alpha_0 - \alpha_1 CFO_t - \alpha_2 ACCRUAL_t) + e_{t+1}$$

	Parameter Estimate		*Test of Equality of Forecasting and Pricing Coefficients*[b]	
Independent Variable	*Earnings Forecasting Regression*	*Pricing Regression*	*Likelihood Ratio*	*p-value*
CFO	0.860	0.742	38.57	0.001
ACCRUAL	0.752	0.872	11.31	0.001

Hypotheses Tests	*Test Statistic*[c]	*p-value*
CFO, ACCRUAL: $a_1 = a_2$	224.9	0.001

Panel B: Cash Flow, Income-Increasing (Positive) Accruals, and Income-Decreasing (Negative) Accruals

$$EARNINGS_{t+1} = a_0 + a_1 CFO_t + a_2 POSACC_t + a_3 NEGACC_t + \varepsilon_{t+1}$$

$$SIZEADJR_{t+1} = \beta(EARNINGS_{t+1} - \alpha_0 - \alpha_1 CFO_t - \alpha_2 POSACC_t - \alpha_3 NEGACC_t) + e_{t+1}$$

	Parameter Estimate		*Test of Equality of Forecasting and Pricing Coefficients*[b]	
Independent Variable	*Earnings Forecasting Regression*	*Pricing Regression*	*Likelihood Ratio*	*p-value*
CFO	0.860	0.751	31.50	0.001
POSACC	0.743	1.002	15.31	0.001
NEGACC	0.757	0.790	0.42	0.517

Hypotheses Tests	*Test Statistic*	*p-value*
POSACC, NEGACC: $a_2 = a_3$	0.57	0.452
POSACC, NEGACC: $\alpha_2 - a_2 = \alpha_3 - a_3$	6.22	0.012

(*Continued on next page*)

TABLE 3 (Continued)

Panel C: Cash Flow, Accruals, and Insider Trading

$$EARNINGS_{t+1} = a_0 + a_1 CFO_t + a_2 NTPOSACC_t + a_3 BPOSACC_t + a_4 SPOSACC_t + a_5 NTNEGACC_t + a_6 BNEGACC_t + a_7 SNEGACC_t + a_8 SELL_t + a_9 BUY_t + a_{10} MTB_t + \varepsilon_{t+1}$$

$$SIZEADJR_{t+1} = \beta(EARNINGS_{t+1} - \alpha_0 - \alpha_1 CFO_t - \alpha_2 NTPOSACC_t - \alpha_3 BPOSACC_t - \alpha_4 SPOSACC_t - \alpha_5 NTNEGACC_t - \alpha_6 BNEGACC_t - \alpha_7 SNEGACC_t - \alpha_8 SELL_t - \alpha_9 BUY_t - \alpha_{10} MTB_t) + e_{t+1}$$

	Parameter Estimate		*Test of Equality of Forecasting and Pricing Coefficients*[b]	
Independent Variable	*Earnings Forecasting Regression*	*Pricing Regression*	*Likelihood Ratio*	*p-value*
CFO	0.862	0.745	36.36	0.001
NTPOSACC	0.765	0.954	6.72	0.010
BPOSACC	0.907	0.992	0.10	0.752
SPOSACC	0.561	1.171	11.91	0.001
NTNEGACC	0.750	0.771	0.15	0.696
BNEGACC	0.749	0.829	0.18	0.672
SNEGACC	0.863	0.891	0.03	0.874
SELL	0.007	0.039	2.71	0.100
BUY	0.001	0.003	0.01	0.908
MTB	0.001	−0.001	4.15	0.042

Hypotheses Tests	*Test Statistic*	*p-value*
H_1: NTPOSACC, BPOSACC: $a_2 = a_3$	6.22	0.006
H_2: NTPOSACC, SPOSACC: $a_2 = a_4$	27.36	0.001
H_4: NTNEGACC, BNEGACC: $a_5 = a_6$	0.01	0.489
H_5: NTNEGACC, SNEGACC: $a_5 = a_7$	8.28	0.002
NTPOSACC, BPOSACC, SPOSACC: $\alpha_2 = \alpha_3 = \alpha_4$	1.36	0.507
NTPOSACC, SPOSACC: $\alpha_2 - a_2 = \alpha_4 - a_4$	4.94	0.013

(*Continued on next page*)

TABLE 3 (Continued)

Panel D: Cash Flow, Current Accruals, and Insider Trading

$$
\begin{aligned}
EARNINGS_{t+1} = {} & a_0 + a_1CFO_t + a_2NTPOSCA_t + a_3BPOSCA_t + a_4SPOSCA_t \\
& + a_5NTNEGCA_t + a_6BNEGCA_t + a_7SNEGCA_t + a_8SELL_t + a_9BUY_t \\
& + a_{10}MTB_t + a_{11}DEP_t + \varepsilon_{t+1} \\
SIZEADJR_{t+1} = {} & \beta(EARNINGS_{t+1} - \alpha_0 - \alpha_1CFO_t - \alpha_2NTPOSCA_t - \alpha_3BPOSCA_t - \alpha_4SPOSCA_t \\
& - \alpha_5NTNEGCA_t - \alpha_6BNEGCA_t - \alpha_7SNEGCA_t - \alpha_8SELL_t - \alpha_9BUY_t \\
& - \alpha_{10}MTB_t - \alpha_{11}DEP_t) + e_{t+1}
\end{aligned}
$$

	Parameter Estimate		*Test of Equality of Forecasting and Pricing Coefficients*[b]	
Independent Variable	*Earnings Forecasting Regression*	*Pricing Regression*	*Likelihood Ratio*	*p-value*
CFO	0.850	0.740	30.13	0.001
NTPOSCA	0.753	0.906	5.71	0.017
BPOSCA	0.846	1.048	0.77	0.381
SPOSCA	0.602	1.220	14.80	0.001
NTNEGCA	0.751	0.829	1.32	0.251
BNEGCA	0.786	0.434	2.42	0.120
SNEGCA	0.811	0.782	0.01	0.909
SELL	0.003	0.019	0.75	0.387
BUY	0.001	−0.015	0.67	0.412
MTB	0.001	−0.001	4.30	0.038
DEP	−0.467	−0.560	1.06	0.302

Hypotheses Tests	*Test Statistic*	*p-value*
H_1: NTPOSCA, BPOSCA: $a_2 = a_3$	3.57	0.029
H_2: NTPOSCA, SPOSCA: $a_2 = a_4$	18.06	0.001
H_4: NTNEGCA, BNEGCA: $a_5 = a_6$	0.52	0.472
H_5: NTNEGCA, SNEGCA: $a_5 = a_7$	1.23	0.134
NTPOSCA, BPOSCA, SPOSCA: $\alpha_2 = \alpha_3 = \alpha_4$	3.73	0.155
NTPOSCA, SPOSCA: $\alpha_2 - a_2 = \alpha_4 - a_4$	7.41	0.003

(Continued on next page)

TABLE 3 (Continued)

Panel E: Cash Flow, Unexpected Current Accruals, and Insider Trading

$$\begin{aligned}EARNINGS_{t+1} = {} & a_0 + a_1 CFO_t + a_2 NTPOSUCA_t + a_3 BPOSUCA_t + a_4 SPOSUCA_t \\ & + a_5 NTNEGUCA_t + a_6 BNEGUCA_t + a_7 SNEGUCA_t + a_8 NTECA_t + a_9 BECA_t \\ & + a_{10} SECA_t + a_{11} SELL_t + a_{12} BUY_t + a_{13} MTB_t + a_{14} DEP_t + \varepsilon_{t+1}\end{aligned}$$

$$\begin{aligned}SIZEADJR_{t+1} = {} & \beta(EARNINGS_{t+1} - \alpha_0 - \alpha_1 CFO_t - \alpha_2 NTPOSUCA_t - \alpha_3 BPOSUCA_t \\ & - \alpha_4 SPOSUCA_t - \alpha_5 NTNEGUCA_t - \alpha_6 BNEGUCA_t - \alpha_7 SNEGUCA_t - \alpha_8 NTECA_t \\ & - \alpha_9 BECA_t - \alpha_{10} SECA_t - \alpha_{11} SELL_t - \alpha_{12} BUY_t - \alpha_{13} MTB_t - \alpha_{14} DEP_t) + e_{t+1}\end{aligned}$$

	Parameter Estimate		*Test of Equality of Forecasting and Pricing Coefficients*[b]	
Independent Variable	*Earnings Forecasting Regression*	*Pricing Regression*	*Likelihood Ratio*	*p-value*
CFO	0.851	0.741	27.81	0.001
NTPOSUCA	0.747	0.899	4.65	0.031
BPOSUCA	0.921	1.160	1.01	0.314
SPOSUCA	0.548	1.287	14.99	0.001
NTNEGUCA	0.744	0.863	3.10	0.080
BNEGUCA	0.786	0.185	7.24	0.007
SNEGUCA	0.800	0.776	0.01	0.919
NTECA	0.733	0.754	0.06	0.812
BECA	0.661	0.602	2.21	0.137
SECA	0.707	1.117	2.76	0.097
SELL	0.003	0.011	0.18	0.675
BUY	−0.001	−0.036	3.18	0.075
MTB	0.001	−0.556	4.55	0.033
DEP	−0.467	−0.557	0.94	0.332

Hypotheses Tests	*Test Statistic*	*p-value*
H_1: NTPOSUCA, BPOSUCA: $a_2 = a_3$	11.76	0.001
H_2: NTPOSUCA, SPOSUCA: $a_2 = a_4$	23.33	0.001
H_4: NTNEGUCA, BNEGUCA: $a_5 = a_6$	0.77	0.377
H_5: NTNEGUCA, SNEGUCA: $a_5 = a_7$	1.28	0.130
NTPOSUCA, BPOSUCA, SPOSUCA: $\alpha_2 = \alpha_3 = \alpha_4$	4.76	0.093
NTPOSUCA, SPOSUCA: $\alpha_2 - a_2 = \alpha_4 - a_4$	8.62	0.002

(*Continued on next page*)

TABLE 3 (Continued)

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[a] Sample Consists of 21,678 firm-years from 1985–1996.
EARNINGS, CFO, MTB, accruals and unexpected accruals are defined as in Table 2.
SIZEADJR = the difference between the 12-month buy-and-hold return starting four months after the fiscal year-end in which accruals are measured and the 12-month buy-and-hold return on a value-weighted portfolio of similar-sized (market value) firms;
DEP = depreciation and amortization scaled by average total assets;
SELL = 1 if abnormal insider selling, and 0 otherwise;
BUY = 1 if abnormal insider buying, and 0 otherwise;
POSACC (NEGACC) = total accruals if total accruals are positive (negative), and 0 otherwise;
NTPOSACC (NTNEGACC) = total accruals if total accruals are positive (negative), and there is no abnormal insider trading, and 0 otherwise;
NTPOSCA (NTNEGCA) = current accruals if current accruals are positive (negative), and there is no abnormal insider trading, and 0 otherwise;
NTPOSUCA (NTNEGUCA) = unexpected current accruals if unexpected current accruals are positive (negative), and there is no abnormal insider trading, and 0 otherwise;
NTECA = expected current accruals when there is no insider trading, and 0 otherwise;
BPOSACC (BNEGACC) = total accruals if total accruals are positive (negative), and BUY = 1, and 0 otherwise;
BPOSCA (BNEGCA) = current accruals if current accruals are positive (negative), and BUY = 1, and 0 otherwise;
BPOSUCA (BNEGUCA) = unexpected current accruals if unexpected current accruals are positive (negative), and BUY = 1, and 0 otherwise;
BECA = expected current accruals when BUY = 1, and 0 otherwise;
SPOSACC (SNEGACC) = total accruals if total accruals are positive (negative), and SELL = 1, and 0 otherwise;
SPOSCA (SNEGCA) = current accruals if current accruals are positive (negative), and SELL = 1, and 0 otherwise;
SPOSUCA (SNEGUCA) = unexpected current accruals if unexpected current accruals are positive (negative), and SELL = 1, and 0 otherwise;
SECA = expected current accruals when SELL = 1, and 0 otherwise.

[b] Mishkin (1983) shows that the test statistic $2 \times N \times \log(SSR^c/SSR^u)$ is asymptotically $\chi^2(q)$ distributed where:
q = the number of rational pricing constraints imposed;
N = the number of observations in the sample;
SSR^c = the sum of squared residuals from the constrained regressions in the second stage;
SSR^u = the sum of squared residuals from the unconstrained regressions in the first stage;

[c] Test statistic is based on an F-test for Panel A, Panel B, and H_1–H_4. The Likelihood Ratio Test is used for all other hypotheses.

The preceding results suggest that accrual mispricing is largely attributable to income-increasing accruals. Indeed, we obtain similar results when we define accruals as current accruals, unexpected total accruals, and unexpected current accruals. Moreover, this result is not attributable to measuring earnings before special items and potentially excluding income-decreasing earnings management that occurs in the form of one-time charges, because we obtain similar results using Income before Extraordinary Items (Compustat #18). Prior work has suggested that the accrual-mispricing phenomenon is due to current accruals (Bradshaw et al. 2001; Thomas and Zhang 2002), and to the unexpected component of accruals (Xie 2001), so our results suggest that future research focus on the income-increasing component of those accruals.

In Panel C of Table 3, we investigate the persistence and pricing of total accruals conditional on the direction of insider trading. Our earnings persistence tests reveal the following. In support of H_1, we find that income-increasing accruals are significantly more persistent for firms with abnormal insider buying (0.907) relative to firms for which there is no abnormal trading (0.765). In contrast, we find that income-increasing accruals are significantly less persistent for firms with abnormal insider selling (0.561) relative to firms for which there is no abnormal trading (0.765), consistent with H_2. We find no support for H_4: the estimate of persistence on income-decreasing accruals accompanied by abnormal insider buying (0.749) is not significantly different from the coefficient on income-decreasing accruals when there is no abnormal trading (0.750). However, in support of H_5, we find that income-decreasing total accruals are significantly more persistent (0.863 vs. 0.750) when accompanied by abnormal insider selling.

The pricing tests of total accruals conditional on the direction of insider trading yield the following results. Our tests of H_3 yield mixed results. On one hand, we find no evidence that income-increasing total accruals accompanied by abnormal insider buying are mispriced. The forecasting and valuation coefficients on BPOSACC equal 0.907 and 0.992, respectively, and the likelihood ratio test fails to reject the null hypothesis of rational pricing (likelihood ratio = 0.10, p-value = 0.752). On the other, in support of H_3, we find that the market appears to overprice income-increasing total accruals accompanied by abnormal insider selling. The forecasting coefficient on SPOSACC (0.561) is significantly smaller than its valuation counterpart (1.171), suggesting that investors overestimate the persistence of income-increasing accruals accompanied by abnormal insider selling (likelihood ratio = 11.91, p-value = 0.001) relative to their ability to predict one-year-ahead earnings. When there is no abnormal insider trading, income-increasing accruals are also overpriced. The coefficient on NTPOSACC inferred from the pricing equation (0.954) significantly exceeds the forecasting equation counterpart (0.765). Consistent with our results in Panel B, we find no evidence that the market misprices income-decreasing accruals, even after conditioning on the direction of insider trading (H_6).

To gain further insight about the mispricing of income-increasing total accruals, we conduct two other comparisons. First, we evaluate whether the market prices income-increasing total accruals the same when there is abnormal insider selling (1.171), no abnormal insider trading (0.954), and abnormal insider buying (0.992). We cannot reject the hypothesis of joint equality for the three valuation coefficients, obtaining a likelihood ratio of 1.36 (p-value = 0.507). Because our analysis suggests that income-increasing total accruals accompanied by abnormal buying are rationally priced with respect to their implications for one-year-ahead earnings, our results suggest that the market prices all income-increasing total accruals as if they were of high quality.

Second, we compare the mispricing of income-increasing total accruals accompanied by abnormal selling to the mispricing of income-increasing total accruals when there is no abnormal trading. Panel C of Table 3 shows that the valuation coefficient for income-increasing accruals accompanied by abnormal selling ($\alpha_3 = 1.171$) is approximately twice as large as its forecasting counterpart ($a_3 = 0.561$). In contrast, the valuation coefficient for income-increasing accruals when there is no trading ($\alpha_2 = 0.954$) is approximately 25 percent larger than its forecasting counterpart ($a_2 = 0.765$). The overpricing of the abnormal-selling income-increasing accruals is greater: the likelihood ratio statistic (4.94) rejects the null hypothesis that the relative mispricing is equal ($\alpha_2 - a_2 = \alpha_4 - a_4$) with a p-value of 0.013. We later evaluate whether the lower earnings persistence and the greater relative overpricing of income-increasing accruals accompanied by abnormal insider selling likely result from opportunistic earnings management.

With two exceptions that relate to income-decreasing accruals, we obtain virtually identical results for our tests based on current accruals and unexpected current accruals.[16] The first exception is that, as shown in Panels D and E of Table 3, we find no evidence of a relation between abnormal insider selling and the persistence of income-decreasing current accruals or income-decreasing unexpected accruals. The second exception is that Panel E shows that the market appears to misprice income-decreasing unexpected current accruals accompanied by abnormal insider buying. In sum, collective evidence for income-decreasing accruals is weak because: (1) we find no support for H_4 that income-decreasing accruals accompanied by abnormal insider buying are less persistent, (2) we find only limited support for H_5 that income-decreasing accruals accompanied by abnormal insider selling are more persistent only for *total* accruals, and (3) we observe evidence that income-decreasing accruals are mispriced as hypothesized in H_6 only when abnormal insider *buying* accompanies income-decreasing *unexpected current* accruals.

In sum, we interpret the results in Table 3 as evidence that insider trading information is useful in *ex ante* identifying the likely persistence (i.e., quality) of firms' income-increasing accruals. This is important because our evidence suggests that accrual mispricing is primarily driven by income-increasing accruals. In addition, the results suggest that while investors appear to understand the signal contained in insider buying, they fail to understand the signal contained in insider selling. We believe this arises because insider buying provides a more informative signal of future firm performance than insider selling. Firms' top executives are typically underdiversified, as much of their wealth (salary, bonus, retirement benefits, stock, and option holdings) and human capital are tied to their firm's performance. Thus, when these executives make open-market purchases and willingly assume additional firm-specific equity risk, their buying is a credible positive signal about future firm performance. In contrast, the motivations for insider selling are more numerous: portfolio rebalancing, liquidity needs, tax issues, or an anticipated decline in firm performance can all trigger managers to sell their firms' shares. For this reason, investors may have difficulty isolating information-motivated insider selling.

Hedge Returns Tests

The pricing tests reported in Table 3 are subject to several limitations. The Mishkin (1983) test is a joint test of market rationality and the ability of our model to correctly capture the market's expectation of earnings. Thus, if the test rejects market rationality, one interpretation is that our earnings expectation model is misspecified. Our expectation model is likely misspecified because it implicitly assumes that the ability to forecast one-year-ahead earnings is the only value-relevant dimension of current earnings components. To the extent that our earnings expectation model is incomplete, the Mishkin (1983) framework assumes that stock prices are efficient with respect to all omitted variables that are correlated with earnings or with our insider trading constructs. To ensure that our market mispricing results are not due to errors in model specification, we next investigate whether investment strategies based on our results (i.e., portfolios formed on the basis of firms' accrual and insider trading characteristics) earn economically significant returns.

As in Sloan (1996), we compute mean size-adjusted returns for deciles formed on total accruals. Table 4, Panel A shows that the return to the hedge portfolio formed by taking a long position in the lowest accrual decile and a short position in the highest accrual decile earns a hedge return of 11.5 percent, which is similar to Sloan's (1996, 307) 10.4 percent.

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[16] We also obtain similar results when we estimate unexpected accruals using the Jones (1991) model, and Beneish's (1998) modification of the Jones model, instead of the Dechow et al. (1995) modification of the Jones model.

TABLE 4

Insider Trading and Subsequent Annual Size-Adjusted Hedge Portfolio Stock Returns Sample Consists of 21,678 Firm-Years between 1985 and 1996

(p-values in parentheses)

Panel A: Average Returns for Portfolio Assignment Based on Total Accruals

		Accruals and Insider Trading		
Portfolio[a]	*Accruals Only*	*Abnormal Selling*	*Abnormal Buying*	*No Abnormal Trading*
Lowest	0.055	0.025	0.045	0.059
	(0.011)	(0.687)	(0.460)	(0.016)
Highest	−0.060	−0.181	−0.025	−0.039
	(0.001)	(0.001)	(0.594)	(0.023)

Hedge Portfolios	*Hedge Returns*
(1) Short: Highest total accrual decile [−0.060] Long: Lowest total accrual decile [0.055]	11.5%
(2) Short: Highest total accruals and Abnormal Selling [−0.181] Long: Similar sized firms [0.000]	18.1%
(3) Short: Highest total accruals and Abnormal Selling [−0.181] Long: Lowest total accrual decile [0.055]	23.6%
(4) Short: Highest total accruals and Abnormal Selling [−0.181] Long: Highest cash flow decile [0.075][b]	25.6%

Panel B: Average Returns for Portfolio Assignment Based on Current Accruals

		Accruals and Insider Trading		
Portfolio[a]	*Accruals Only*	*Abnormal Selling*	*Abnormal Buying*	*No Abnormal Trading*
Lowest	0.068	0.023	0.040	0.075
	(0.002)	(0.702)	(0.488)	(0.003)
Highest	−0.061	−0.164	−0.024	−0.044
	(0.001)	(0.002)	(0.636)	(0.010)

Hedge Portfolios	*Hedge Returns*
(1) Short: Highest current accrual decile [−0.061] Long: Lowest current accrual decile [0.068]	12.9%
(2) Short: Highest current accruals and Abnormal Selling [−0.164] Long: Similar sized firms [0.000]	16.4%
(3) Short: Highest current accruals and Abnormal Selling [−0.164] Long: Lowest current accrual decile [0.068]	23.2%
(4) Short: Highest current accruals and Abnormal Selling [−0.164] Long: Highest cash flow decile [0.075][b]	23.9%

(Continued on next page)

TABLE 4 (Continued)

Panel C: Average Returns for Portfolio Assignment Based on Unexpected Current Accruals

		Accruals and Insider Trading		
Portfolio[a]	*Accruals Only*	*Abnormal Selling*	*Abnormal Buying*	*No Abnormal Trading*
Lowest	0.063 (0.004)	0.034 (0.560)	−0.045 (0.354)	0.077 (0.003)
Highest	−0.064 (0.001)	−0.184 (0.001)	−0.013 (0.784)	−0.049 (0.005)

Hedge Portfolios	*Hedge Returns*
(1) Short: Highest unexpected current accrual decile [−0.064] Long: Lowest unexpected current accrual decile [0.063]	12.7%
(2) Short: Highest unexpected current accruals and Abnormal Selling [−0.184] Long: Similar sized firms [0.000]	18.4%
(3) Short: Highest unexpected current accruals and Abnormal Selling [−0.184] Long: Lowest unexpected current accrual decile [0.063]	24.7%
(4) Short: Highest unexpected current accruals and Abnormal Selling [−0.184] Long: Highest cash flow decile [0.075][b]	25.9%

[a] We rank firms on the magnitude of their accruals and assign them in equal numbers to ten portfolios each year. The highest accrual decile corresponds to the most income-increasing accruals.

[b] The average size-adjusted return to the high cash flow decile was 7.48 percent (t = 5.42)

See Table 2 for definitions of insider trading variables and accrual metrics.

Incorporating insider trading information in the formation of the hedge portfolio increases hedge returns. We obtain a hedge return of 18.1 percent by shorting abnormal insider selling firms in the highest accrual decile, and investing the proceeds in a portfolio of similar-sized firms. Indeed, by decomposing the average size-adjusted return of −6.0 percent for the highest accrual decile, we obtain −18.1 percent in firms where insiders are abnormal sellers, −2.5 percent in firms where insiders are abnormal buyers, and −3.9 percent in firms where there is no abnormal trading. Further, consistent with our finding that the market does not misprice income-increasing accruals accompanied by abnormal insider buying, the return to the insider buying partition is insignificant. Our insider trading measure is not simply identifying those firms with the most extreme accruals in the highest accrual decile because the mean and median accrual levels for the abnormal selling firms and non-abnormal-selling firms are not statistically different.[17] The decomposition suggests that managers use their knowledge of factors associated with the low persistence of the firm's income-increasing accruals to their benefit by selling their equity-contingent wealth at higher prices.

We consider two alternative ways to invest the proceeds from short-selling firms with high accruals and abnormal selling. First, even though our evidence suggests that the market does not misprice income-decreasing accruals, we take a long position in firms with the lowest (i.e., most income-decreasing) accruals to provide a comparison to prior work. Second, consistent with our evidence that investors underweight operating cash flows, we also take a long position in firms with the highest cash flows. The mean size-adjusted return is 5.50 percent for firms in the lowest accrual decile and 7.48 percent for firms in the highest cash flow decile. These alternative long positions yield hedge portfolio returns of 23.6 and 25.6 percent, respectively. The hedge returns for current and unexpected current accruals in Panels B and C of Table 4 are similar, which is not surprising as our accrual and unexpected accrual proxies are highly correlated.[18]

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[17] In the highest accrual decile, the mean total accrual is 0.168 for the abnormal selling sample, and 0.174 for the sample complement (t = 0.92). Moreover, the mean current accrual is 0.208 for the abnormal selling sample and 0.214 for the sample complement (t = 0.89). We obtain similar results using unexpected accruals.

In summary, the ability of insider selling information to identify low-earnings-quality firms in the highest accrual decile is robust across various accrual and unexpected accrual measures. The use of insider trading information in conjunction with accruals yields hedge returns that significantly exceed those obtained using accruals alone. The difference in hedge returns between the accrual-only and accrual-insider trading strategies is not attributable to the return to a strategy based on insider trading alone, because such a strategy yields a one-year-ahead return of only 3.3 percent.[19] We interpret the higher hedge returns on the combined high accrual-insider selling strategy as evidence that a firms' top executives trade on their knowledge of the economic factors underlying the low persistence of their firm's income-increasing accruals. Our findings thus contribute to insider trading research because, although prior research has shown that insider trading is profitable, past studies have failed to identify the specific information on which managers trade in anticipation of future price changes (e.g., Elliott et al. 1984; Sivakumar and Waymire 1994; Givoly and Palmon 1985).

Our analysis assumes that the top five executives have the most accurate information regarding the quality of their firms' earnings and that they trade based on this knowledge. Untabulated analyses reveal that trading strategies based on the top five executives' trading earn significantly larger hedge returns than do strategies based on abnormal buying and selling by directors, officers, and large shareholders. Further, the hedge returns based on the abnormal trading of directors, officers, and large shareholders are not significantly different from those of the accrual-only hedges. Thus, these other "insiders" trading provides little incremental information. We also repeat our analysis without distinguishing among insiders, but instead pooling all insider transactions for each firm. We find that aggregate insider trading is not informative and does not improve upon hedge returns based on accruals alone. These results suggest that the top five executives' trades signal the most valuable information about accrual persistence, and validate our design choice.

We measure hedge returns before transaction costs, yet a large portion of the hedge return arises from the short side of the hedge. While collateral transaction costs seem unlikely to explain the large returns to our short position, we are not able to estimate them, and we thus do not know whether these returns are sufficient to compensate investors for the costs and risks associated with short sales. We do, however, assess the reasonableness of these seemingly large hedge returns in three ways. First, we repeat the analysis after eliminating delisted firms for which, as in Sloan (1996), we assume a −100 percent delisting return. The results are robust to the elimination of these delisted securities; for example, we obtain a hedge return of 22.6 percent for the trading strategy that short sells firms with high accruals and abnormal selling and takes a long position in firms with low accruals, while the hedge return is 23.6 percent when we use the whole sample.

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[18] We also examine the annual returns to the various hedge portfolios on a year-by-year basis. Similar to Sloan (1996), we find that the annual hedge returns based on accruals are positive in 10 of 12 years. The annual hedge returns are positive for strategies that combine insider selling and total accruals (12 out of 12 years), current accruals (11 of 12 years), and unexpected current accruals (11 of 12 years); and hedges based on shorting these high accrual decile firms that also have abnormal insider selling yield higher returns than the accrual-only hedge in 10 of 12 years. In addition, we find no indication that the annual hedge returns decrease over the period 1985–1996. This suggests that the relation between insider trading and accrual persistence appears stable over time.

[19] Our return estimate of 3.3 percent likely understates the profitability of trading strategies based solely on insider trading, because we do not measure returns immediately following the public reporting of insider trades. However, our conclusions would not be affected if the returns to the strategy based on insider trading alone were, in line with prior work, in the range of 5 to 6 percent. Estimates of one-year-ahead size-adjusted returns to strategies based on insider trading equal 5 percent in Rozeff and Zaman (1988), 6 percent in Seyhun (1998), and 4.8 percent in Lakonishok and Lee (2001) who also adjust for book-to-market effects.

Second, collateral transaction costs are higher for smaller, lower priced firms, and short sellers typically focus on firms with market capitalization greater than $500 million to reduce the risk that a lender demands the return of a stock (Staley 1997). Therefore, we reformulate our hedge portfolios subject to the restriction that firms in the long and short positions must be in CRSP-size-deciles 6 through 10. We obtain smaller but still substantial hedge returns: the returns to the trading strategy that short sells firms with high accruals and abnormal selling and takes a long position in firms with low accruals is 18.7 percent, whereas when we use the whole sample, the corresponding hedge return is 23.6 percent.

Third, we investigate whether the hedge returns can be explained by omitted variables associated with future returns. We consider six such characteristics: (1) the market to-book ratio (MTB), (2) returns in the prior year (RET_{t-1}), (3) earnings yield (EPRATIO), (4) firm size (LMVE), (5) earnings surprise (SURP), and (6) beta (BETA), and we estimate the following regression:[20]

$$\begin{aligned} SIZEADJR_{t+1} = {} & a_0 + a_1HEDGE_t + a_2MTB_t + a_3RET_{t-1} + a_4EPRATIO_t \\ & + a_5LMVE_t + a_6SURP_t + a_7BETA_t \end{aligned} \tag{2}$$

where $SIZEADJR_{t+1}$ is the one-year ahead size-adjusted return to each of our firms in the hedge portfolios, and HEDGE is an indicator variable equal to 1 when we take a long position in the firms' stock and 0 otherwise.

We mean-adjust all independent variables except HEDGE. As a result, we interpret the slope coefficient on HEDGE, a_1, as the return to the hedge portfolio after controlling for the effect of the other variables. Similarly, we interpret the intercept (a_0) as the return to the short position in the hedge portfolio and the sum of a_0 and a_1 as the return to the long position in the portfolio after controlling for the effect of the other variables. We estimate Equation (2) year-by-year as well as in a pooled cross-sectional regression. Because we obtain similar results in both analyses, we present only the pooled results.

Table 5 reports the size-adjusted returns to various hedge portfolios after controlling for these six possible alternative determinants of future returns. Panel A presents the hedge returns to the three possible accrual-only hedges, and Panel B presents the hedge returns to the same three accrual measures after taking a short position in the insider selling firms in the highest accrual decile. Thus, the hedge return differences between Panels A and B are attributable solely to the incremental effect of using insider selling information.

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[20] Prior research has shown these characteristics are correlated with subsequent returns: (1) the market-to-book ratio, following evidence in Chan et al. (1996), Davis (1994), and Haugen and Baker (1996), who document that firms with high market-to-book ratios subsequently earn lower returns; (2) returns in the prior year, following evidence in Jegadeesh (1990), and Jegadeesh and Titman (1993) that short-run returns tend to continue in the subsequent year; (3) earnings yield, following evidence that firms with low P/E ratios outperform firms with high P/E ratios on a risk-adjusted basis (among others, Haugen and Baker 1996); (4) firm size and beta, following evidence in, among others, Fama and French (1992), that size and beta explain future returns; and (5) earnings surprise, to capture the post-announcement drift documented by (among others) Freeman and Tse (1989) and Bernard and Thomas (1989).

In Panel A of Table 5, the dependent variables are returns for firms in the total accruals, current accruals, and unexpected current accruals hedge portfolios. Panel A indicates hedge returns of 12 to 13 percent after controlling for these other factors. These hedge returns are similar to those reported in Table 4 that exclude the six control variables. In Panel B of Table 5, returns to hedges that combine accruals and abnormal insider selling systematically exceed those to the accrual-only hedges in Panel A. The hedge returns range between 22 and 23 percent, and are only slightly lower than those reported in Table 4.

In sum, controlling for market-to-book ratio, prior year returns, earnings yield, firm size, earnings surprise, and beta does not diminish the profitability of our hedge strategies. This suggests that our hedge portfolio returns are not simply manifestations of market-to-book, price momentum, P/E, size, or post-earnings announcement drift effects.

TABLE 5
Hedge Portfolio Performance after Controlling for Various Factors Associated with Future Returns
(p-level in parentheses)

$$SIZEADJR_{t+1} = a_0 + a_1 HEDGE_t + a_2 MTB_t + a_3 RET_{t-1} + a_4 EPRATIO_t + a_5 LMVE_t + a_6 SURP_t + a_7 BETA_t{}^{a}$$

Panel A: Accrual and Unexpected Accrual Hedges

Long Portfolio: All firms in decile 1 of the specified accrual metric (i.e., most income-decreasing accruals).
Short Portfolio: All firms in decile 10 of the specified accrual metric (i.e., most income-increasing accruals).

	Total Accruals	*Current Accruals*	*Unexpected Current Accruals*
Intercept	−0.069 (0.001)	−0.068 (0.001)	−0.069 (0.001)
HEDGE	0.121 (0.001)	0.129 (0.001)	0.125 (0.001)
MTB	−0.001 (0.567)	−0.001 (0.715)	−0.001 (0.741)
RET(t−1)	0.018 (0.200)	0.004 (0.751)	0.001 (0.960)
EPRATIO	0.021 (0.005)	0.024 (0.015)	0.018 (0.071)
LMVE	−0.026 (0.001)	−0.024 (0.002)	−0.025 (0.001)
SURP	0.001 (0.881)	0.006 (0.467)	0.006 (0.447)
BETA	0.048 (0.001)	0.043 (0.001)	0.043 (0.002)
Observations	4,251	4,257	4,117

(Continued on next page)

TABLE 5 (Continued)

Panel B: Accrual and Insider Selling Hedges

Long Portfolio: All firms in decile 1 of the specified accrual metric (i.e., most income-decreasing accruals).
Short Portfolio: Abnormal selling firms in decile 10 of the specified accrual metric (i.e., most income-increasing accruals).

	Abnormal Selling and Total Accruals	*Abnormal Selling and Current Accruals*	*Abnormal Selling and Unexpected Current Accruals*
Intercept	−0.187	−0.164	−0.172
	(0.001)	(0.003)	(0.004)
HEDGE	0.238	0.227	0.227
	(0.001)	(0.001)	(0.001)
MTB	−0.001	−0.001	0.001
	(0.709)	(0.829)	(0.869)
RET(t − 1)	0.021	−0.002	−0.003
	(0.326)	(0.909)	(0.882)
EPRATIO	0.022	0.028	0.021
	(0.019)	(0.051)	(0.180)
LMVE	−0.026	−0.021	−0.035
	(0.016)	(0.063)	(0.003)
SURP	0.001	0.007	0.006
	(0.934)	(0.506)	(0.548)
BETA	0.038	0.029	0.036
	(0.038)	(0.121)	(0.072)
Observations	2,388	2,395	2,301

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[a] Variable definitions:

SIZEADJR = the one-year ahead size-adjusted return to each of our firms in the hedge portfolios;
HEDGE = an indicator variable equal to 1 when we take a long position in the firms' stock and 0 otherwise; and
SURP = the difference between income before extraordinary items in the fourth quarter in the most recent year and the same quarter in the previous year, deflated by market value of equity.

Variables MTB, RET(t − 1), EPRATIO, LMVE, BETA, insider trading variables and accrual metrics are defined in Table 2. All variables (excluding HEDGE) are mean-adjusted. We rank firms on the magnitude of their accruals and assign them in equal numbers to ten portfolios each year.

Notwithstanding the above evidence, the possibility remains that our findings are due to other unknown omitted risk factors rather than to investors failing to understand the valuation implications of large income-increasing accruals accompanied by abnormal selling. If the returns to our trading strategy stem from isolating firms with low earnings quality, then the hedge returns should diminish after the subsequent earnings fail to materialize. In untabulated analyses, we find that returns to various accrual and abnormal insider selling hedges are statistically insignificant in the second year after portfolio formation. This suggests that our inferences are unlikely to be an artifact of omitted risk factors.

Distinguishing Opportunistic Earnings Management from Changes in Firm Performance

Our tests of earnings persistence require that managers act as informed traders, not that they opportunistically manage earnings. Thus, our evidence that income-increasing accruals accompanied by abnormal insider selling are associated with less persistent earnings suggests either: (1) that managers artificially inflate earnings, or (2) that changes in the firm's economic environment lead to transitory income-increasing accruals. We assess the extent to which earnings management is a likely explanation for the lower persistence of income-increasing accruals accompanied by abnormal insider selling, by investigating whether these firm-years are associated with three signs of earnings management: (1) significantly positive performance-matched abnormal accruals, (2) a greater propensity to make income-increasing accounting choices, and (3) a higher incidence of small positive reported earnings and small increases in reported earnings. We conduct our analyses for total accruals, current accruals, and their unexpected counterparts, but because we obtain similar results across accrual metrics, we report in Table 6 only the results for current accruals.

Performance-Matched Abnormal Accruals

Firms with income-increasing accruals accompanied by abnormal insider selling have mean accruals (and unexpected accruals) that significantly exceed zero. However, this result is not conclusive of earnings management because accruals are *a priori* not estimates of earnings management, and unexpected accruals measure managerial discretion with error (see discussions in McNichols 2000 and Beneish 2001). Following Kothari et al. (2001), we identify for each income-increasing accrual firm a performance-matched firm based on industry membership (two-digit SIC code), year, and lagged return on assets (ROA). We evaluate whether our firms' current accruals significantly exceed those of their matched counterparts. The test assumes that the expected or normal level of accruals for a given firm in a given industry and year, is the level of accruals for the firm's performance-match and is equivalent to estimating Kothari et al.'s (2001) performance-matched abnormal accruals.[21]

Table 6, Panel A, reveals that firms with income-increasing current accruals accompanied by abnormal insider selling still have more positive accruals than their performance-matched counterparts. The mean performance-matched current accruals are 0.029, significantly positive at $p = 0.001$. We also find that performance-matched current accruals for abnormal insider selling firms significantly exceed the corresponding measures for firms in which there is no abnormal trading (p 0.02), and for abnormal insider buying firms ($p = 0.01$).[22] To the extent that performance matching isolates accruals stemming from managerial discretion, the evidence suggests that our income-increasing accrual, abnormal insider selling firms on average engage in more upward earnings management relative to both their performance-matched counterparts and to other income-increasing accrual firms.

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[21] Kothari et al. (2001) show that performance-matched accruals lead to estimates of unexpected accruals that are smaller in magnitude and to better-specified tests relative to Jones-like models of aggregate accruals. However, the results of our test should be interpreted with caution, because the external validity of performance-matched accruals remains to be established, and because, like aggregate accrual models, performance matching is silent about the form and extent of managerial discretion.

[22] Firms with income-increasing current accruals and no abnormal insider trading also have significantly greater income-increasing accruals than their performance-matched counterparts ($p = 0.001$). In contrast, the results for firms with income-increasing current accruals that occur concurrently with abnormal insider buying are mixed. Mean and median performance-matched current accruals equal 0.006 (p-value = 0.376) and 0.012 (p-value = 0.001), respectively, and only the median is distinguishable from zero.

TABLE 6
Tests of Earnings Management

Panel A: Performance-Matched Current Accruals[a]

Insider Trading and Accrual Partition	*Performance-Matched Current Accruals*			*Current Accruals*	
	Mean	*Median*	*Percent Positive*	*Mean*	*Median*
Income-Increasing Current Accruals and Abnormal Insider Selling (1)	0.029 (0.001)	0.035 (0.001)	64.1	0.087	0.059
Income-Increasing Current Accruals and No Abnormal Trading (2)	0.016 (0.001)	0.021 (0.001)	60.1	0.066	0.038
(1) vs. (2) Mean t-test and Median Wilcoxon-Z [p-value]	2.17 [0.02]	2.48 [0.01]			
Income-Increasing Current Accruals and Abnormal Insider Buying (3)	0.006 (0.376)	0.012 (0.001)	57.3	0.062	0.036
(1) vs. (3) Mean t-test and Median Wilcoxon−Z [p-value]	2.53 [0.01]	3.31 [0.01]			

Panel B: Depreciation Methods of Sample Firms Relative to Performance-Matched Firms[b]

Insider Trading and Accrual Partition	*Less Income-Increasing*	*Same Method*	*More Income-Increasing*	*p-level*[c]
Income-Increasing Current Accruals and Abnormal Insider Selling	0.128	0.696	0.176	0.004
Income-Increasing Current Accruals and No Abnormal Trading	0.173	0.666	0.161	0.981
Income-Increasing Current Accruals and Abnormal Insider Buying	0.160	0.674	0.166	0.843

Panel C: Bad Debt Allowance for Sample Firms vs. Performance-Matched Firms[d]

Insider Trading and Accrual Partition	*Mean (median)*		*Difference [p-level]*
	Sample Firms	*Matched Firms*	
Income-Increasing Current Accruals and Abnormal Insider Selling	0.049 (0.031)	0.058 (0.033)	−0.008 [0.028]
Income-Increasing Current Accruals and No Abnormal Trading	0.048 (0.030)	0.053 (0.032)	−0.005 [0.001]
Income-Increasing Current Accruals and Abnormal Insider Buying	0.045 (0.031)	0.048 (0.033)	−0.003 [0.376]

(*Continued on next page*)

TABLE 6 (Continued)

Panel D: Earnings Per Share (EPS) for Accrual and Insider Trading Partitions[e]

	Percent of Firms Reporting EPS from			
Insider Trading and Accrual Partition	*−0.05 to 0.00*	*0.00 to 0.05*	*t-statistic (p-value)*[f]	*Percent of Firms Reporting Profits*
Income-Increasing Current Accruals and Abnormal Selling	1.25	3.53	10.82 (0.01)	82.6
All other firm-years	1.77	4.28	2.40 (0.02)	69.9

Panel E: Change in Earnings Per Share (EPS) for Accrual and Insider Trading Partitions[e]

	Percent of Firms Reporting Changes in EPS from			
Insider Trading and Accrual Partition	*−0.05 to 0.00*	*0.00 to 0.05*	*t-statistic (p-value)*[f]	*Percent of Firms Reporting Increases*
Income-Increasing Current Accruals and Abnormal Selling	1.73	6.44	6.22 (0.01)	76.8
All other firm-years	2.17	5.78	5.26 (0.01)	64.8

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[a] For each insider trading and positive-accrual firm, we identify a performance-matched firm based on industry membership (two-digit SIC), year, and lagged return on assets (ROA). Following Kothari et al. (2001), we estimate performance-matched abnormal accruals as the difference between each accrual metric for our sample and performance-matched control firms.

[b] Following Teoh et al. (1998), we rank the various depreciation methods (Compustat annual footnote 15) according to their effect on income: (1) straight line, (2) combination of methods, and (3) accelerated. If, for example, a firm uses straight-line depreciation while its match uses a combination of methods, then we classify our sample firm's method as more income-increasing than that of its match.

[c] The test focuses on observations with dissimilar depreciation methods and evaluates whether the frequency of income-increasing and income-decreasing choices are equal.

[d] Dependent Variable = Bad Debt Allowance ÷ Gross Accounts Receivable, where Bad Debt Allowance is Compustat item #67 and Gross Accounts Receivable is Compustat item #2.

[e] EPS is computed as Net Income Before Extraordinary Items divided by the Number of Shares Outstanding. Change in EPS refers to the change in EPS in two consecutive years.

[f] We follow Degeorge et al. (1999) and compute the following statistic:

$$\tau_n = (\Delta\log(p(x_n)) - \text{mean}_{i\in R,\, i\neq n}\{\Delta\log(p(x_i))\}) \div \text{s.d}_{i\in R,\, i\neq n}\{\Delta\log(p(x_i))\}$$

where:

$p(x_n)$ = the proportion of observations in bin n of the distribution, and

R = a small symmetric region around bin n (the bin of interest).

We compute the change in the logarithm of the proportion of observations falling in the bin containing 0 to the bin immediately to the left of 0. We compare this difference to the mean difference in adjacent bins in a small symmetric region of 10 cells around, but excluding, the threshold of interest. We also compute the sample standard deviation of the differences. Following Degeorge et al. (1999), the resulting statistic follows a t-distribution with 9 degrees of freedom and the p-values we report are based on one-tailed tests.

Accounting Choices

Following Teoh et al. (1998), we investigate the extent to which our sample firms' depreciation methods and provision for bad debts are more income-increasing than those of their performance-matched counterparts. Table 6, Panel B reports that income-increasing accruals, insider-selling firms are significantly more likely to use income-increasing depreciation methods than their performance-matched counterparts ($p = 0.004$). In contrast, income-increasing accrual firms with no abnormal trading or with abnormal buying are not more likely to use income-increasing depreciation methods than their performance-matched controls.

Panel C of Table 6 reports that firms with income-increasing accruals and either abnormal insider selling or no abnormal trading have lower bad debt allowances than their performance-matched counterparts ($p = 0.028$ and 0.001, respectively). In contrast, the bad debt allowance of income-increasing accrual, insider-buying firms is not distinguishable from that of their performance-matched counterparts. Collectively, these results suggest that our income-increasing accrual, abnormal insider selling firms tend to be more aggressive in their accounting choices: they tend to report lower depreciation and lower bad-debt expense than their performance-matched controls.

Frequency of Reported Earnings around Zero or Zero Earnings Changes

We also follow Burgstahler and Dichev (1997) and Degeorge et al. (1999) and examine the distribution of reported earnings around two thresholds: (1) zero earnings, and (2) last year's earnings. In Table 6, Panel D, we find that there are 2.28 percent (3.53 − 1.25) more firms just to the right (vs. the left) of the zero earnings threshold for income-increasing current accruals accompanied by abnormal insider selling. This is not significantly different from the corresponding percentage for all other firm-years in the sample (2.51 percent). We find similar results in Panel E; there are 4.71 percent more firms to the right (vs. the left) of the zero earnings change threshold for income-increasing current accruals accompanied by abnormal insider selling, but this is not significantly different from the corresponding percentage for all other firm-years in the sample (3.61 percent). There are thus discontinuities in both subsamples, but the discontinuities are not larger for income-increasing accruals accompanied by abnormal insider selling than for other firm-years.

Because investigating such discontinuities speaks only to whether earnings management likely occurs in the vicinity of the threshold, Panels D and E also display the proportions of firms that report profits and increases in earnings. We find that firms with income-increasing current accruals accompanied by abnormal selling are significantly more likely to report profits (82.6 percent) and higher earnings relative to the prior year (76.8 percent) than all other firm-years in the sample (69.9 and 64.8 percent, respectively).[23] Absent earnings management, these results are surprising because they show that firms in which we observe contemporaneous insider *selling* are more likely to report profits and annual earnings increases.

Overall, we interpret the evidence in these additional analyses as supporting the proposition that firms with income-increasing accruals accompanied by abnormal insider selling likely engaged in upward earnings management. However, income-increasing accruals are also mispriced when there is no abnormal insider trading. Thus, we conclude that opportunistic earnings management is only a partial explanation for accrual mispricing.

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[23] Binomial tests reject the null hypothesis of equality in the proportions of firms reporting profits ($Z = 10.25$, p-value = 0.001), and the null hypothesis of equality in the proportions of firms reporting increases in earnings ($Z = 9.28$, p-value = 0.001).

V. ROBUSTNESS CHECKS

Our results depend critically on the validity of our insider trading measure. In the absence of a model to determine the equilibrium amount of information-motivated insider trading, we assess the sensitivity of our results by (1) controlling for factors associated with the intensity with which firms award stock-based compensation, and (2) using Insider Consensus as an alternative means of classifying firm-years into different abnormal buying/selling groups. Our results are robust to these additional checks.

Although we suspect that insider selling increases with the intensity of stock-based compensation, we are unaware of any empirical evidence documenting such a relation. First, data for such a study are not readily available. Second, most stock-compensation awards offer executives the option to either acquire the firms' stock *or* to receive in cash the option's intrinsic value by exercising Stock Appreciation Rights (SARs). This option attenuates the potential positive correlation between stock-compensation intensity and insider selling intensity, because executives who exercise SARs are not required to file insider trading reports (SEC Form 4) as their stock ownership does not change. Nonetheless, we conduct several tests to ensure that our inferences are not attributable to insider trading acting as an instrument for firm characteristics that are associated with differential accrual persistence.

Prior work has shown that the use of stock compensation awards increases with firm size and with firms' growth opportunities (e.g., Lewellen et al. 1987; Smith and Watts 1992; Gaver and Gaver 1993).[24] Because we measure abnormal insider trading relative to other similarly sized firms, our insider trading measures control for variation in stock-based compensation intensity that is associated with firm size; however, our measures do not control for growth, and Tarpley (2000) suggests that firms with higher growth have less persistent accruals.

To control for the possibility that our measure of abnormal insider selling is merely a proxy for the variation in stock-compensation intensity that is associated with the firm's growth, we repeat our analyses after controlling for growth. Specifically, we modify our model by adding interactions of market-to-book ratio (MTB) with the six accrual-based variables in Equations (1a) and (1b). Untabulated analyses reveal that after controlling for the effect of growth on the persistence and pricing of accruals, our results are unchanged: income-increasing total accruals are significantly less persistent for firms with abnormal insider selling (0.637) relative to firms for which there is no abnormal trading (0.774) (F-test = 9.06, p-level = 0.001). Although income-increasing total accruals are significantly less persistent for firms with higher growth, further analysis reveals that insider trading dominates growth in terms of its effect on accrual persistence.[25] Similarly, including the market-to-book interactions has little effect on our tests of rational pricing. After controlling for growth, we still find that the market overprices income-increasing total accruals accompanied by abnormal insider selling, relative to these accruals' implications for one-year-ahead earnings (likelihood ratio = 4.92, p-level = 0.013), but we find no evidence that the market misprices the interaction between MTB and SPOSACC (likelihood ratio = 1.65, p-level = 0.199). This suggests that the mispricing of income-increasing total accruals accompanied by abnormal insider selling is not an artifact of insider selling proxying for growth. We obtain similar results for models based on current accruals and unexpected-current accruals and conclude that our findings are robust to variation in firms' growth opportunities.

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[24] Prior work has investigated other potential determinants of the intensity of stock compensation awards, such as managerial ownership, the agency costs of debt, and tax costs, but the evidence has been mixed. For example, Lewellen et al. (1987) and Matsunaga (1995) conclude that managerial ownership is unrelated to stock option awards, but Mehran (1995) documents a negative relation. Similarly, Matsunaga (1995) and Mehran (1995) find that leverage is unrelated to stock option awards, but Lewellen et al. (1987) find a positive relation.

[25] For example, the estimated coefficient on the interaction between MTB and SPOSACC (income-increasing total accruals accompanied by abnormal insider selling) is significantly negative (–0.014, p-level = 0.001). Using the median value of MTB for abnormal insider selling firms (2.6) to evaluate the effect of this interaction, the estimated persistence of income-increasing accruals would further decline by –0.036 (–0.014 × 2.6), or about one quarter of the decline due to abnormal insider selling –0.137 (0.774 – 0.637).

Other determinants of stock-compensation intensity may explain cross-sectional differences in accrual persistence, but data on these determinants are not available for large samples over the period we study. To the extent that other determinants of stock-compensation intensity are stable over time for a given firm, we can further probe this issue by examining whether our insider trading measure repeatedly classifies observations from the same firm as abnormal insider selling. Our 1,952 abnormal insider selling firm-years are based on 1,101 firms. Of these, 635 firms (58 percent) appear only once (out of 12 possible years) in our abnormal selling group, 263 firms (24 percent) appear twice (out of 12 possible years) and the remaining 203 firms (18 percent) appear in three or more years. Thus, our abnormal insider trading classification is not merely a proxy for firm-specific characteristics. Moreover, we find that persistence and pricing tests confirm our prior results when we repeat our analyses after deleting firms that have more than two abnormal insider selling firm-years during the 12-year sample period.

We also repeated our analyses using an alternative measure of insider trading—Insider Consensus. Because top executives' trading is unanimous in 98 percent of our sample firm-years, classifying firms into abnormal trading groups using Insider Consensus is nearly equivalent to treating all firm-years with positive net shares traded as abnormal buyers and firm-years with negative net shares traded as abnormal sellers. The results are somewhat weaker, but, with one exception, our inferences are unchanged; the single exception is that we find no evidence that the persistence of income-increasing accruals differs between the abnormal insider buying and no abnormal trading groups.

VI. CONCLUSION

This paper provides evidence that insider trading is an informative signal about earnings quality and the valuation implications of accruals. Specifically, we show that (1) the one-year-ahead persistence of income-increasing accruals is significantly lower when accompanied by abnormal insider selling and greater when accompanied by abnormal insider buying; (2) the accrual mispricing phenomenon observed in previous work (e.g., Sloan 1996) is due to the mispricing of income-increasing accruals; (3) investors appear to price all income-increasing accruals as though they are of high quality; (4) one-year-ahead hedge returns to trading strategies based on accruals and insider trading significantly exceed those based on accruals alone; and (5) the lower persistence of income-increasing accruals accompanied by abnormal insider selling appears at least partly attributable to opportunistic earnings management.

This paper extends Sloan (1996) by suggesting that accrual mispricing is largely due to the mispricing of income-increasing accruals. Thus, although prior work suggests that the accrual-mispricing phenomenon is due to current accruals or to the unexpected component of accruals, our results suggest that researchers focus on the income-increasing component of those accruals. Second, our evidence suggests that opportunistic earnings management partially accounts for investors' failure to understand the low persistence of income-increasing accruals that are accompanied by abnormal insider selling. Although this evidence only pertains to the abnormal insider selling subsample, the accumulation of evidence in Beneish (1999), Beneish et al. (2002), and in this study is consistent with some managers inflating earnings so they can sell their firms' equity at higher prices. Thus, future research could investigate the extent to which insider trading opportunities provide an incentive for earnings management.

Our findings also have implications for insider trading research. Although prior research has documented that insider trading is profitable (e.g., Jaffe 1974; Finnerty 1976; Givoly and Palmon 1985; Rozeff and Zaman 1988; Seyhun 1998), this research has largely been unable to identify precisely what type of information gives insiders the edge. Our findings suggest that insiders exploit their superior knowledge of the economic factors underlying the persistence of their firms' income-increasing accruals.

Finally, the results have implications for accounting and investing professionals. In particular, the evidence that contemporaneous insider trading is informative about the quality of the noncash components of earnings suggests that members of audit committees and auditors who are now required to report to audit committees on the quality of their clients' earnings (AICPA 1999) could benefit from examining executives' trading.

Three limitations of this study raise issues for future research. One limitation is the assumption that our insider trading measures capture managers' information-motivated rather than diversification-, liquidity-, or tax-motivated trading. Extant insider trading research provides no model of the equilibrium amount of insider trading; rather, it describes empirical regularities that we exploit in developing our measures of abnormal trading and in conducting robustness tests. Despite our efforts, we cannot rule out the possibility that our insider trading measures reflect trading that is not information-motivated.

A second limitation is that our measures of accruals capture managerial discretion with error. We attempt to minimize the measurement error, and we conclude that earnings management is only a partial explanation for the lower persistence of income-increasing accruals accompanied by abnormal selling, and the resultant mispricing. However, we cannot completely rule out the possibility that the lower persistence of these accruals arises because of adverse changes in the firm's economics. The isolation of the discretionary component of accruals is worthy of further research.

A final limitation is that we do not provide a theory for our asymmetric findings that market participants appear to misprice only income-increasing accruals. One possibility is to explore whether investors are overly optimistic about firm growth prospects (e.g., see DeAngelo et al. 1996), or whether they simply appear overly optimistic about firm prospects because they have a preference for holding firms with positively skewed returns.

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Executive Target Bonuses and What They Imply about Performance Standards

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ABSTRACT: We provide evidence that CEOs' and lower-level business unit executives' *target bonuses* are negatively associated with a proxy for measurement noise in accounting-based performance measures, and positively associated with proxies for firms' growth opportunities and the extent of executives' decision-making authority. Non-CEO executives' target bonuses are also positively associated with their CEO's target bonus.

In addition, we compare executives' actual and target bonuses over two consecutive periods to draw inferences about how firms revise executives' *performance standards*. If firms adjust performance standards to fully reflect executives' past performance, then we expect an executive's chances of earning an above-target bonus to be independent of his past performance. We find evidence to the contrary; an executive is more likely to receive an above-target bonus if he received an above-target bonus in the prior year than if he did not. This suggests that firms do not adjust standards to fully reflect executives' past performance, consistent with agency-theoretic arguments that a firm can better motivate its executives if it discounts executives' past performance in setting their future compensation.

Keywords: *bonus plans; incentive compensation; performance standards; targets.*

Data Availability: *The data used in this study are from private sources listed in the text.*

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I. INTRODUCTION

Large U.S. corporations usually pay their senior executives a salary, an annual bonus, and long-term incentive compensation. Although the annual bonus is not usually the largest component of executive pay, it plays an integral role in corporate management control systems. The annual bonus motivates all executives to be more productive, and often represents the main component of incentive pay for mid-level executives.[1] The effect of bonus plans on executives' incentives is also important for practitioners and researchers who design and evaluate alternative performance measures. For example, accounting-based annual bonus plans motivate executives to be more productive in the short-term (e.g., Holthausen et al. 1995b), but the manner in which firms design and revise accounting-based bonus plans may also encourage executives to misrepresent their reported accounting performance (e.g., Leone and Rock 2002).

Bonus plans' effects on executives' incentives (and hence firm value) are important, but detailed descriptions of the salient features of bonus plans are rarely available, especially for mid-level executives. A typical executive bonus plan specifies the compensation earned by an executive as a function of the performance measure used to evaluate him (Murphy 2001). Specifically, a firm pays an executive a prespecified *target bonus* if the executive's actual performance equals a prespecified *performance standard* and pays more or less than the target bonus if the executive's performance is greater or less than the performance standard.[2]

Our objective is to provide evidence about the determinants of executives' target bonuses, and what target bonuses imply about an important component of management control systems—performance standards. We focus on executives' target bonuses (rather than *ex post* actual bonuses) because, by definition, target bonuses reflect firms' *ex ante* incentive design decisions, and are unaffected by unsystematic *ex post* events that affect executives' actual bonuses. Consistent with the implications of agency theory, we show that executives' target bonuses reflect firms' *ex ante* intentions to motivate their executives optimally, thereby increasing firm value. In particular, we find that executives' target bonuses (measured relative to executives' base salaries) are negatively associated with a proxy for measurement noise in accounting-based performance metrics, and positively associated with proxies for firms' growth opportunities and the extent of executives' decision-making authority. Non-CEO executives' target bonuses are also positively associated with their CEO's target bonus. This evidence confirms that related findings in prior studies of executives' *ex post* actual bonuses (e.g., Fisher and Govindarajan 1993; Baiman et al. 1995; Nagar 2002) also apply to executives' target bonuses that more accurately reflect firms' *ex ante* incentive design decisions. Our evidence also demonstrates that economic determinants of CEO compensation, documented in studies such as Smith and Watts (1992) and Gaver and Gaver (1993), also extend deeper in the organization to group, division, and plant managers.

Because target bonuses reflect firms' *ex ante* intentions to motivate their executives optimally, target bonuses provide a unique window through which we can draw inferences about the otherwise unobservable *performance standards* that underlie executives' target bonuses. This is important because performance standards are a key component of management control systems; the way firms set performance standards, the way they revise them over time, and the ease with which executives achieve those standards affect executives' incentives to increase firm value. Our study provides the first large sample evidence that provides insight into the evolution of executives' performance standards.

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[1] In our sample, the median non-CEO executive's annual bonus is $48,000, representing approximately 16 percent of pay and 52 percent of incentive pay. For CEOs, the median annual bonus is approximately $344,000, representing 21 percent of pay and 41 percent of incentive pay.

[2] This definition of *target bonus* is consistent with the language in corporate proxy statements and the academic literature. For example, Holthausen et al. (1995a) describe budget-based plans in which firms specify target bonuses for each executive but actual bonuses vary depending on the degree to which executives achieve pre-established performance budgets.

Multiperiod agency models often predict that firm value is higher if a principal can commit in advance to adopt standards that do not incorporate information about an agent's past performance (Laffont and Tirole 1993, 16). However, some of these same models also suggest that a sequentially rational principal can benefit *ex post* if he reneges on this agreement and uses information about an agent's past performance in setting the agent's future compensation (e.g., Milgrom and Roberts 1992, 232). Of course, if firms use executives' superior past performance as a standard for evaluating their future performance, then executives may reduce their productivity in early periods (which in turn likely reduces firm value) to avoid being held to higher (i.e., ratcheted) standards in the future. Still other research suggests that firms adjust performance standards differently depending on whether executives' past performance was above or below expected performance (Weitzman 1980; Leone and Rock 2002).[3]

Since the prior theoretical literature offers different predictions about how firms set standards, the question of whether firms fully or partially adjust performance standards for executives' past performance is largely an empirical issue. Unfortunately, researchers rarely have access to performance measure and performance standard data on a large scale, because firms almost never disclose such proprietary details. Consequently, we use bonus data and compare executives' actual bonuses with their target bonuses over two consecutive periods to infer the manner in which firms revise executives' performance standards. Our inference relies on the definition of target bonus—an executive earns more than his target bonus if he performs above his preestablished performance standard, and he earns less than his target bonus if he performs below standard. Hence, the probability an executive earns more than his target bonus corresponds to the probability that he performs better than his standard. It follows that if firms revise performance standards in a manner that fully incorporates executives' past performance, then both the probability that an executive earns more than his target bonus and the amount of this excess bonus should be independent of the executive's past performance. In contrast, if firms do not fully incorporate executives' past performance in setting their performance standards (e.g., because of an implicit or explicit commitment to discount past performance information), then both the probability that an executive earns more than his target bonus and the amount of this abnormal bonus depends on the executive's past performance.

Our evidence is consistent with the view that firms do not fully adjust performance standards for executives' past performance. In particular, we show that if an executive previously earned more than his target bonus, then he has a 72 percent chance of earning more than his target bonus again in the current year. In contrast, if an executive previously earned less than his target bonus, then he has only a 42 percent chance of earning more than his target bonus in the current year. Likewise, we find that an executive's expected abnormal bonus (defined as the difference between his actual bonus and target bonus) in any period is significantly positively associated with his abnormal bonus in the prior period. These findings hold for both CEOs and lower-level business unit executives, suggesting a firm-wide reluctance to fully adjust performance standards for executives' past performance.

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[3] Basing executive compensation on performance standards creates other incentive problems. For example, if firms set performance standards as part of their participative budgeting process, then executives may game their participation to ensure that standards are set below their expected performance (e.g., Chow et al. 1991). In addition, the manner in which firms set and revise executives' performance standards may encourage executives to smooth reported performance (e.g., Holthausen et al. 1995a).

In a single-firm study of budget targets, Leone and Rock (2002) conclude that firms *ratchet* managers' budget targets upward following superior past performance, thus motivating high-performing managers to consciously reduce their reported performance. However, they do not examine the extent to which firms revise managers' targets upward, or whether these ratcheted targets fully reflect managers' past performance. In contrast, our large sample results suggest that firms discount executives' past performance in setting their future compensation, potentially mitigating executives' incentives to reduce their reported performance.

Since our evidence concerning the way performance standards are revised is indirect, we investigate alternative interpretations for our results. First, we consider whether our results could be explained by the fact that some firms pay their executives discretionary or subjective bonuses for dimensions of performance that are not easily quantifiable (e.g., Bushman et al. 1996; Ittner et al. 1997; Murphy and Oyer 2002). If these dimensions of performance persist through time but are not explicitly incorporated in executives' performance standards, then executives' abnormal bonuses may be positively serially correlated. Supplementary analyses do not support this alternative interpretation. Specifically, firms' propensity to pay their executives subjective bonuses is unrelated to the serial correlation in their executives' abnormal bonuses.

Our results are also susceptible to the interpretation that executives smooth their reported performance. That is, if executives adjust performance downward when they are likely to exceed their standard and inflate performance when they are likely to miss their standard, then a positive serial correlation in executives' abnormal bonuses may be attributable to performance smoothing rather than to firms' standard-setting practices. Evidence from supplementary analysis for a sample of CEOs is inconsistent with this explanation. Specifically, we find that the serial correlation in a CEO's abnormal bonuses is unrelated to our proxy for a CEO's opportunity to adjust reported performance—the magnitude of his firm's accounting accruals.

In the next section, we discuss the role of executive target bonuses, provide a framework that links executive bonuses with performance, and develop a set of research hypotheses. In Section III we describe our sample and empirical methods, including the way we measure our compensation and explanatory variables. Section IV presents our empirical results. In Section V, we address alternative interpretations for our results and discuss robustness tests. Finally, in Section VI, we offer some concluding remarks.

II. INCENTIVE BONUSES, TARGETS, AND PERFORMANCE STANDARDS

Executive bonus plans include three basic components: performance measures, performance standards, and the relation between pay and performance.[4] Figure 1 depicts a typical executive bonus plan that shows the link among these three components (Murphy 2001, 251). If an executive's performance equals the standard, then he earns his *target bonus*. Bonus plans also often specify a minimum bonus (corresponding to a threshold performance level) and a maximum bonus, both usually expressed as a percentage of the

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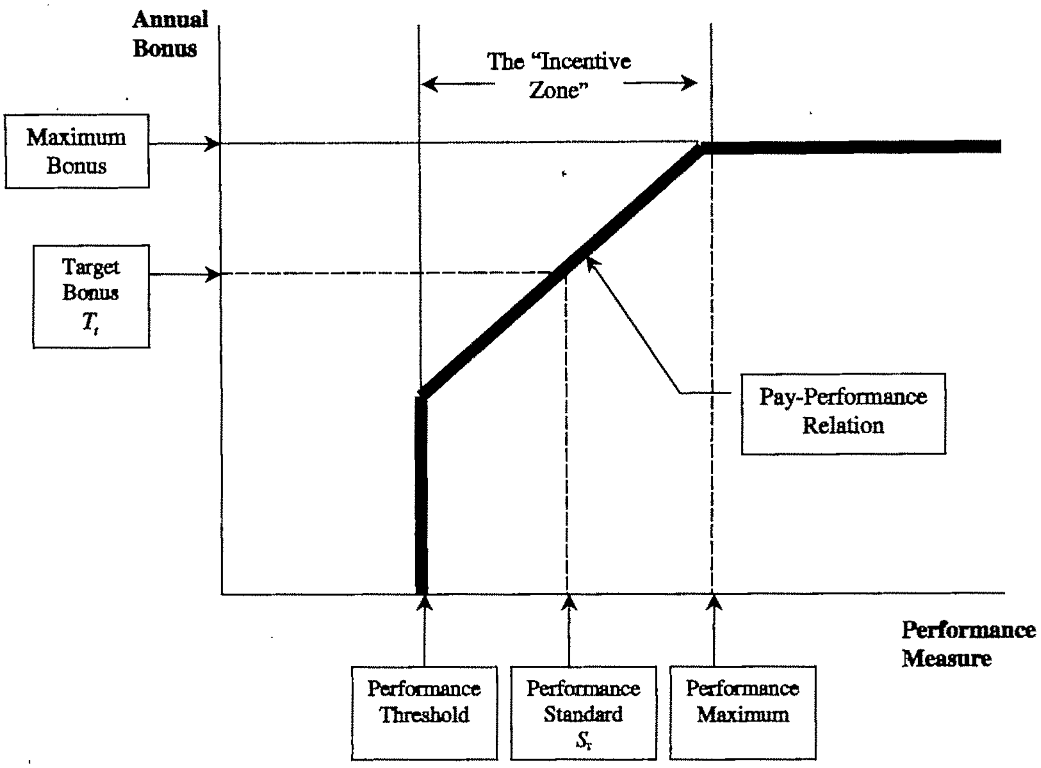
[4] Firms jointly design (1) compensation practices (bonuses, stock options, and other short-term and long-term compensation), (2) performance measurement systems, and (3) policies for delegating decision-making authority. Nonetheless, we focus exclusively on executive annual bonus plans, and defer a comprehensive general equilibrium analysis for future research.

FIGURE 1
A Typical Executive Annual Bonus Plan



Adapted from Murphy (2001, 251).

executive's target bonus. Finally, the pay-for-performance relation in the "incentive zone" in Figure 1 is often linear, but may also be convex or concave.

Figure 1 notwithstanding, research on the incentive effects of bonus plans face two difficulties: (1) there is little theoretical guidance about how the combination of various bonus plan features contributes to managerial incentives, and (2) firms rarely publicly disclose detailed information about bonus plan components, especially for mid-level executives. Prior literature has used either the existence of bonus plans or the amount of executives' actual bonuses as proxies for the "incentive power" of bonus plans (e.g., Gaver and Gaver 1993; Holthausen et al. 1995a). In contrast to this prior research, we focus on executives' target bonuses (rather than *ex post* actual bonuses) because, by definition, target bonuses reflect firms' *ex ante* incentive design decisions, and are unaffected by unsystematic *ex post* events that affect executives' actual bonuses. More importantly, since executives' target bonuses explicitly reflect executives' performance standards, focusing on target bonuses provides insights about otherwise unobservable performance standards.

Determinants of Executives' Target Bonuses

We begin by examining the economic determinants of executives' target bonuses in order to establish that firms design target bonuses to motivate their executives optimally.

Such evidence is necessary to support our maintained proposition that the performance standards that underlie executives' target bonuses affect executives' incentives.

Agency theory suggests that measurement noise affects the way firms use performance measures to compensate executives (e.g., Holmstrom 1979; Lambert and Larcker 1987). Since annual bonus plans explicitly depend on accounting-based performance criteria (Holthausen et al. 1995a; Bushman et al. 1996; Murphy 1999), we expect executives' target bonuses to be negatively associated with the measurement noise in accounting-based metrics.

Agency theory suggests that firms with greater investment opportunities reward their executives (whether CEOs or business unit managers) with greater incentive pay to motivate them to be more productive (Smith and Watts 1992; Gaver and Gaver 1993). Prendergast (2001) and Nagar (2002) further suggest that firms provide more incentive pay to those executives who exercise greater decision-making authority in order to align executives' interests with shareholders' interests. Thus, we predict that an executive's target bonus is positively associated with both his firm's investment opportunities and the extent of his decision-making authority.

Finally, prior literature that relies on social comparison theory (e.g., O'Reilly et al. 1988) suggests that management control systems at lower levels in an organization tend to parallel or complement control systems at higher levels in an organization. In addition, Holmstrom and Tirole (1989) intimate that providing complementary incentives throughout a hierarchical organization can enhance firm value. We interpret this literature as suggesting that a firm's bonus plans tend to have similar incentive attributes regardless of executives' rank in the organization. Thus, we predict that lower-level business unit executives' target bonuses are positively associated with their CEO's target bonus.

> **H1:** An executive's target bonus is negatively associated with measurement noise in his firm's accounting-based performance metrics. An executive's target bonus is positively associated with his firm's growth opportunities, the extent of his decision-making authority, and his CEO's target bonus.

Hypothesis 1 may appear similar to predictions in the prior literature (e.g., Smith and Watts 1992; Gaver and Gaver 1993; Fisher and Govindarajan 1993; Bushman et al. 1994; Baiman et al. 1995; Nagar 2002). However, in contrast to most prior research, we use panel data on target bonuses (rather than actual bonuses) for a broad cross-section of firms, and we include non-CEO business unit executives as well as CEOs. More importantly, H1 is a necessary precondition for our study of executive performance standards. Indeed, our inferences about executives' performance standards are important only if target bonuses reflect firms' *ex ante* intentions to motivate their executives optimally and increase firm value.

Executives' Performance Standards and Target Bonuses

In dynamic environments, the way firms update and revise an executive's performance standards to reflect his past performance affects the executive's current and future incentives. For instance, if a firm revises an executive's performance standard upward because of the executive's superior past performance, then the executive may reduce his productivity in early periods to avoid being held to higher, or ratcheted, standards in the future. Firms can benefit if they commit not to use executives' superior past performance against them. For example, firms can agree in advance to long-term compensation arrangements that keep standards partly or completely fixed through time. Unfortunately, such commitments may not be credible because a sequentially rational firm benefits (*ex post*) if it uses past performance information in revising the executive's compensation arrangement (Meyers 1995;

Meyers and Vickers 1997; Indjejikian and Nanda 1999). Moreover, a firm's incentive to renege and reset standards based on available information is likely to be stronger for bonus plans than for other forms of pay because, by definition, firms can renegotiate bonus plans annually. The following example (drawn from Johnson & Johnson's 2001 proxy statement, p. 11) aptly illustrates a firm's predicament in deciding how to use executives' past performance in setting their future performance standards.

> The annual amount of expected improvement in EVA is fixed. To ensure that the EVA element provides strong incentives for management to annually increase shareholder value and does not reward poor performance by reducing performance standards or penalize superior performance by raising performance standards, the Compensation Committee allowed no recalibration of the expected EVA improvement for the three fiscal years 1997–1999. However, due to the significant changes in the Company's business since EVA was adopted, such recalibration was performed for fiscal year 2000.

To develop testable hypotheses about the extent to which firms adjust performance standards for an executive's past performance, we develop a simple framework that links an executive's compensation to his performance. We assume that a firm's executive works for two periods, $t = 1,2$, and the firm rewards the executive based on a measure of his performance, X_t, evaluated relative to a performance standard, S_t. We also assume that S_t depends only past standards and past performance, as in $S_t = k_t(S_{t-1}, X_{t-1})$. Of course in reality, a performance standard, S_t, may also reflect new information unrelated to S_{t-1} and X_{t-1}. Our framework can accommodate such new information, but our results are unaffected as long as the new information is orthogonal to the executive's past performance. Finally, if the firm does not adjust the performance standard for the executive's past performance, then S_t is timeless and does not depend on X_{t-1}.

Given this framework, we let p be the probability that the executive's performance achieves (i.e., equals or exceeds) his standard in the second period, conditional on the executive achieving his standard in the first period. Similarly, we define q as the probability that the executive achieves his standard in the second period, conditional on the executive missing his standard in the first period. That is, $p = \Pr(X_2 \geq S_2|X_1 \geq S_1)$, and $q = \Pr(X_2 \geq S_2|X_1 < S_1)$.

If $p = q$, then the probability that the executive achieves his standard in the second period is independent of whether the executive's performance exceeded or fell short of his standard in the first period. In contrast, if $p \neq q$, then the likelihood that the executive will achieve his standard in the second period depends on whether the executive achieved his performance standard in the first period. For instance, if $p > q$, then an executive is more likely to achieve his performance standard in the second period if he achieved his performance standard in the first period than if he did not. Technically, $p \neq q$ if (and only if) the probability that the executive achieves his performance standard given his past performance, $\Pr(X_2 \geq S_2|X_1)$, depends on the realization of X_1—if $\Pr(X_2 \geq S_2|X_1)$ is an increasing function of X_1, then $p > q$, whereas if $\Pr(X_2 \geq S_2|X_1)$ is a decreasing function of X_1, then $p < q$. $\Pr(X_2 \geq S_2|X_1)$ depends on the combined effect of (1) the information reflected in $S_2 = k_2(S_1, X_1)$, and (2) the characteristics of the conditional distribution of X_2 given X_1.[5] To focus on the information reflected in S_2, we make the following Observation.

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[5] A simple illustration of this combined effect is a setting in which X_1 and X_2 are statistically independent so that the conditional distribution of X_2 given X_1 does not depend on X_1. If S_2 is timeless (i.e., does not depend on X_1), then $\Pr(X_2 \geq S_2|X_1)$ is independent of X_1 and thus $p = q$. In contrast, if S_2 depends on X_1, then $p \neq q$ even though X_1 and X_2 and are statistically independent.

Observation: If the joint probability distribution of X_1 and X_2 is normal or lognormal, then $S_2 = E(X_2|X_1)$ implies $p = q$.
Proof: See Appendix.

Intuitively, this observation suggests that if an executive's performance standard fully reflects the information in his past performance, in the sense that $S_2 = E(X_2|X_1)$, then the probability that the executive will achieve his standard this period is independent of his past performance (i.e., $p = q$). Although this observation does not hold for all potential joint distributions of performance, it aptly characterizes the implication of arguments in the prior literature that sequentially rational firms use all available information in setting executives' performance standards.[6] In H2a and H2b below, we rely on this observation and state the null hypothesis assuming that an executive's performance standard fully reflects information about his past performance. This null hypothesis provides a specific point estimate that we can test against the alternative hypothesis that $S_2 \neq E(X_2|X_1)$.

As stated earlier, we lack executive performance standard data to test $S_2 = E(X_2|X_1)$ or $p = q$ directly. Instead, we compare executives' actual bonuses with their target bonuses over two consecutive periods to infer whether performance standards reflect executives' past performance. Since an executive earns more than his target bonus if he performs above his performance standard, and he earns less than his target bonus if he performs below standard, the probability that the executive performs above his standard corresponds to the probability that he earns more than his target bonus.[7] This link between the probability that the executive achieves his performance standard and the probability that he earns more than his target bonus suggests that, if $S_2 = E(X_2|X_1)$, then the following hypothesis should hold.

H2a: The probability that an executive receives a bonus greater than or equal to his target bonus is independent of whether, in the prior period, the executive received a bonus that was greater than or less than his target bonus.

The preceding Observation (and H2a) focuses on the *probability* that the executive achieves his performance standard, given that he did (or did not) achieve his standard in the prior year. To develop a test that incorporates the *magnitude* by which the executive's performance exceeds his performance standard, we focus on the term $E(X_2 - S_2|X_1 - S_1)$. Regardless of the joint distribution of X_1 and X_2, if $S_2 = E(X_2|X_1)$ (plus or minus a constant), then $E(X_2 - S_2|X_1 - S_1)$ is independent of X_1. That is, if the executive's performance standard fully reflects the information in his past performance, then we expect abnormal performance, defined as $X_t - S_t$, to be serially uncorrelated.

To assess whether executives' abnormal performance is serially correlated, we analyze whether executives' abnormal bonuses (defined as actual bonuses less target bonuses) are serially correlated. The magnitude of an executive's abnormal bonus is a reasonable proxy for the (unobservable) magnitude of his abnormal performance within the "incentive zone" in Figure 1, because an executive's bonus is an increasing function of his performance. Moreover, if this pay-for-performance relation is linear, or if the joint distribution of X_1 and X_2 is normal, then $S_2 = E(X_2|X_1)$ implies that the executive's expected second-period abnormal bonus is independent of his prior period abnormal bonus (see the Appendix for proof). Hence, for circumstances where executives' abnormal bonuses reflect their abnormal performance, we state a different version of our second hypothesis as follows.

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[6] Sufficiency of $S_2 = E(X_2|X_1)$ for $p = q$ also holds for rectangular distributions of the form $f(x_1, x_2) = b$, $0 < x_1 < x_2 \leq 1/\sqrt{b}$ (where b is positive), but it does not hold for exponential distributions.

[7] The probability that an executive earns more than his target bonus may not correspond to the probability that he achieves the performance standard if firms consistently pay their executives discretionary bonuses for dimensions of performance that are not explicitly reflected in executives' performance standards. We address this issue in Section V.

H2b: Executives' abnormal bonuses are serially uncorrelated.

H2a and H2b together address whether performance standards fully impound all relevant information about executives' past performance; that is $S_2 = E(X_2|X_1)$. The alternative hypothesis ($S_2 \neq E(X_2|X_1)$) is that firms incorporate information about executives' past performance in setting current period performance standards, but less completely than proposed under H2a and H2b. Indeed, as discussed earlier, firms may benefit *ex ante* (but not *ex post*) if they ignore executives' past performance, or at least use past performance information less aggressively, in establishing future performance standards (e.g., Milgrom and Roberts 1992, 232; Laffont and Tirole 1993, 16). Prior literature also suggests that firms may revise performance standards asymmetrically; for example, firms may increase standards following above-standard performance, but not reduce standards following below-standard performance (Weitzman 1980; Holthausen et al. 1995a; Leone and Rock 2002). Our tests explicitly allow for the possibility that firms revise executives' performance standards asymmetrically.

Finally, H2a and H2b are joint hypotheses about the information reflected in executives' standards and the probability distributions of managerial performance, but H2a applies to a broader set of circumstances than H2b. If managerial performance through time is joint normally distributed, then $S_2 = E(X_2|X_2)$ is sufficient for both hypotheses. On the other hand, if managerial performance is joint log normally distributed, then $S_2 = E(X_2|X_1)$ is sufficient for H2a but not for H2b unless the pay-for-performance relation is also linear.

III. EMPIRICAL METHODS

Sample and Data

Our sample consists of 397 firms comprising 4,576 managers and 11,383 manager-years, from Hewitt Associates' compensation surveys of public domestic companies for eight years, 1988 to 1995.[8] To be included in our sample, a firm must be in the Hewitt Associates' survey for at least two consecutive years and must report base salaries, target bonuses, and actual bonuses for at least one of the following executives: CEO, group manager, division manager, or plant manager. A group manager has responsibility for multiple divisional profit centers, whereas a division manager typically has profit-center responsibility for a single division within a group. The 397 public companies satisfying these criteria are relatively large, multidivisional companies representing a variety of industries.

The Hewitt Associates' annual compensation survey reports executive salaries and target bonuses for the current fiscal year and the bonus paid in the current year for prior fiscal year's performance. Hence, in comparing executives' actual and target bonuses, we use two consecutive compensation surveys and match the reported target bonus in one year with the actual bonus paid in that year as reported in the next year's compensation survey. This matching of executives' target and actual bonuses limits our analysis to fiscal years 1988 to 1994. Also, because the Hewitt survey compiles compensation information for managerial positions rather than for individual executives, we cannot ensure that an individual in a position in one year is the same individual who was in the survey in the prior year. Hence, if executive turnover is rampant in our sample period, then comparing executives' actual and target bonuses (especially through time) may lead to incorrect conclusions about executives' performance. That said, we believe that the mismatching of actual and target bonuses due to managerial turnover is unlikely to be a serious problem. Based on publicly available information, the average tenure of CEOs in our sample is more than six years (the median is five years), and Bushman et al. (1994) report that division managers in a comparable set of firms from the same compensation surveys average 3.7 years on the job.

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[8] Lambert et al. (1993), Holthausen et al. (1995b), Murphy (1999, 2001), and Murphy and Oyer (2002) use similar proprietary compensation data. Hewitt Associates collects these executive compensation data annually through a mail survey with follow-up telephone conversations to verify its accuracy.

Compensation Variables

We measure an executive's target bonus, and his corresponding actual bonus, relative to the executive's base salary. This controls for scale effects and also reflects the fact that firms commonly specify executives' target bonuses as a fraction of their base salaries. Thus, for t = 1988,...,1994, we have:

$$A_t = \frac{\text{Actual bonus an executive earns for fiscal year t performance}}{\text{Executive's base salary for fiscal year t}} \times 100$$

$$T_t = \frac{\text{Target bonus an executive expects to earn for fiscal year t performance}}{\text{Executive's base salary for fiscal year t}} \times 100$$

and our measure of an executive's *abnormal bonus* is given by:

$$A_t - T_t = \frac{\text{[Actual bonus} - \text{Target bonus] for fiscal year t performance}}{\text{Executive's base salary for fiscal year t}} \times 100.$$

Table 1 provides descriptive statistics on CEO and non-CEO executive salaries, bonus payments, A_t, T_t, $T_t - T_{t-1}$, $A_t - T_t$, and abnormal bonus as a percentage of target bonus, $(A_t - T_t)/T_t$. The CEO and business unit executive salaries average \$602,090 and \$169,350 respectively, and the corresponding bonus payments average \$412,110 and \$74,800 respectively.

Table 1 shows that CEOs' target and actual bonuses are higher—in both absolute terms and as a percentage of base salary—than non-CEOs' target and actual bonuses. T_t, an executive's target bonus as a percentage of his base salary, varies in the cross-section but exhibits little change from year to year. Although the distributions of the compensation variables in Table 1 differ between the CEO and non-CEO executives, we have no *a priori* reason to expect our hypotheses to vary between the CEO and non-CEO subsamples. Nevertheless, we conduct most of our tests separately for CEOs and non-CEO executives.

Table 1 also shows that executives' mean and median abnormal bonus percentage, $(A_t - T_t)/T_t$, is positive for both CEOs and non-CEO executives, suggesting that executives in our sample period tended to perform above the standard that defined the target bonus. Table 2 shows that executives' mean and median abnormal bonus percentage is positive for all but one year and that there is considerable cross-sectional dispersion in this abnormal bonus percentage in all years. More than 6 percent of the $(A_t - T_t)/T_t$ observations are -1, indicating that the executives performed below the threshold at which they earned bonuses (i.e., $A_t = 0$). The time pattern in the abnormal bonus percentage appears to correspond to general economic trends. Although we have no *a priori* reason to predict

TABLE 1
Descriptive Statistics for Executive Compensation Variables[a]

	CEOs						Non-CEOs					
	n	*Mean*	*Median*	*1st Quartile*	*3rd Quartile*	*Std. Dev.*	*n*	*Mean*	*Median*	*1st Quartile*	*3rd Quartile*	*Std. Dev.*
Base Salary ($ thousands)	1,380	602.09	588.15	450.75	715.00	206.05	10,003	169.35	145.03	98.30	211.00	94.89
Actual Bonus ($ thousands)	1,380	412.11	344.30	184.55	575.00	342.37	10,003	74.80	48.00	20.00	101.00	83.07
Actual Bonus (A_t)	1,380	78.01	62.50	38.47	86.61	337.13	10,003	38.52	34.36	19.75	52.80	26.56
Target bonus (T_t)	1,380	66.75	60.00	50.00	65.00	156.22	10,003	35.34	35.00	25.00	45.00	15.23
Change in target bonus ($T_t - T_{t-1}$)	954	1.29	0.00	0.00	0.00	10.58	5,721	0.44	0.00	0.00	0.00	8.74
Abnormal bonus ($A_t - T_t$)	1,380	11.26	5.28	−15.02	24.15	186.82	10,003	3.18	2.30	−9.22	14.55	23.12
Abnormal bonus percentage $100 \times (A_t - T_t)/T_t$	1,380	8.31	10.04	−26.46	42.31	61.64	10,003	11.57	7.53	−29.29	43.99	64.99

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[a] The compenstion data are for 366 CEOs and 4,210 non-CEO (group, division, and plant) executives of 397 firms over the years 1988–1994. Hence, the descriptive statistics are for 1,380 CEO-year and 10,003 non-CEO manager-year observations.

A_t = 100 × the executive's actual bonus for fiscal year t performance, scaled by the executive's year t base salary.

T_t = 100 × the executive's target bonus for fiscal year t performance, scaled by the executive's year t base salary.

TABLE 2
Distribution of Executives' Abnormal Bonus as a Percentage of Target Bonus, $(A_t - T_t)/t_t$, by Year (1988–1994)[a]

Year	*n*	*Mean*	*Std. Dev.*	*1st Quartile*	*Median*	*3rd Quartile*	*% > 0*	*% = −1*
1988	1,743	0.21	0.64	−0.18	0.15	0.50	62.48	2.58
1989	994	0.34	0.88	−0.24	0.15	0.67	60.06	2.92
1990	1,483	0.05	0.59	−0.36	0.03	0.39	52.66	5.80
1991	1,702	−0.05	0.56	−0.41	−0.03	0.26	47.12	9.81
1992	1,769	0.01	0.61	−0.41	0.04	0.37	53.25	11.76
1993	1,840	0.07	0.61	−0.32	0.05	0.40	54.13	7.39
1994	1,852	0.23	0.61	−0.14	0.21	0.56	66.04	3.24
Full Sample	11,383	0.11	0.65	−0.29	0.08	0.44	56.49	6.42

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[a] The compensation data are for 366 CEOs and 4,210 non-CEO (group, division, and plant) executives of 397 firms over the years 1988–1994. Hence, the descriptive statistics are for 1,380 CEO-year and 10,003 non-CEO manager-year observations.

A_t = 100 × the executive's actual bonus for fiscal year t performance, scaled by the executive's year t base salary.

T_t = 100 × the executive's target bonus for fiscal year t performance, scaled by the executive's year t base salary.

that our hypotheses are time-sensitive, we include year-dummy variables in our multiple regressions.

Explanatory Variables

Hypothesis 1 predicts that an executive's target bonus is negatively associated with measurement noise in accounting-based performance metrics, and is positively associated with a firm's growth opportunities, the extent of the executive's decision-making authority, and a firm's propensity to offer incentive compensation to the CEO.

Measurement Noise in Accounting-Based Metrics

Prior compensation research often uses the variance of an accounting rate of return as a proxy for the unobservable measurement noise in accounting-based performance metrics (e.g., Lambert and Larcker 1987; Holthausen et al. 1995b). Lacking accounting rate of return data for individual business units, we use the variance of a firm's annual return on equity to proxy for performance-measure noise for all of the firm's managerial positions. We compute a firm's accounting return on equity as its income before extraordinary items divided by its average common equity, as reported in the 2001 annual Compustat files. For each executive, we compute the time-series variance of his firm's accounting return-on-equity over the ten-year period prior to the executive's first appearance in our sample. We predict an executive's target bonus is negatively associated with the variance of his firm's annual return on equity (VarRoe).

Growth Opportunities

Following Smith and Watts (1992), we use a firm's market-to-book ratio as a proxy for its growth opportunities. Lacking data on the growth opportunities of individual business units, we use a firm's market-to-book ratio as a proxy for growth opportunities for all its business units. We predict an executive's target bonus is positively associated with his firm's market-to-book ratio, measured as the ratio of market value of equity to book value of equity, as reported in the 2001 annual Compustat files. Our empirical proxy is the firm's average market-to-book ratio (MTB) computed over a ten-year period prior to the first year that an executive appears in our sample. We include only firms with a positive book value of equity.

Extent of Executives' Decision-Making Authority

We measure the extent of an executive's decision-making authority using three variables: size of the executive's organizational unit, the uncertainty in the firm's product/market environment, and the executive's reporting level in the firm.

Prior research suggests that large firms require more expert managers (e.g., Rosen 1982; Smith and Watts 1992), large firms have more investment opportunities (e.g., Gaver and Gaver 1993; Smith and Watts 1992), and that the cost of directly monitoring an agent is greater in large firms (Eaton and Rosen 1983). *Ceteris paribus*, this implies that executives of large firms have more decision-making authority and greater impact on firm value than executives of small firms. Consequently, agency theory predicts that executives of larger firms receive more base pay and, in particular, more incentive pay than executives of smaller firms. We expect this relation between firm size and incentive pay to also apply to a firm's business units (groups, division, and plants). Thus, we predict an executive's target bonus is positively associated with the size of the unit for which he is responsible. For CEOs, we expect their target bonus to be positively associated with firm-wide sales revenues. We expect non-CEOs' target bonuses to be positively associated with their business units' sales revenues. Because the distribution of sales is skewed, we use the natural logarithm of sales (LnSales) as the independent variable in all our tests. Sales revenue data are from Hewitt Associates' compensation surveys, which report sales revenues for the firm and for each surveyed business unit. For plants, Hewitt Associates estimates sales revenue based on the plant's production volume.

Our second proxy for the extent of an executive's decision-making authority is the uncertainty of his firm's product-market environment, measured by the variance of annual firm stock returns. Since firms facing greater uncertainty in their product/market environments likely delegate more responsibility to lower-level executives (Prendergast 2001), we predict an executive's target bonus is positively associated with the variance of his firm's annual stock returns.[9] We compute the time-series variance of annual stock returns over a ten-year period prior to the first year that an executive appears in our sample. Because the distribution of the variance of stock returns is skewed, we use the natural logarithm of the variance of stock returns (VarRet) as our empirical proxy. We obtain annual return data from the 2001 annual Compustat files.

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[9] Prior studies also use the variance of annual stock returns as proxies for a firm's growth opportunities (Smith and Watts 1992; Gaver and Gaver 1993), and measurement noise in market-based performance measures (Lambert and Larcker 1987), rather than as a proxy for an executive's decision-making authority. Under all interpretations, we expect an executive's target bonus to be positively associated with the variance of the firm's annual stock returns.

Our last proxy for the extent of an executive's decision-making authority is an executive's reporting level (Level). Hewitt Associates' provides executive reporting-level data where executives lower in a firm's hierarchy are assigned higher numerical values (e.g., Level 0 is the board of directors, Level 1 is the CEO, and so on). *Ceteris paribus*, executives lower in a firm's hierarchy (i.e., higher values of Level) likely wield less decision-making authority, and are more easily monitored by their superiors (e.g., Gerhart and Milkovich 1990; Baiman et al. 1995). This suggests that firms provide less incentive compensation to lower-level executives. Thus, we predict an executive's target bonus is negatively associated with his reporting level.

Firm Propensity to Offer Incentive Compensation

Finally, we measure a firm's propensity to offer incentive compensation to its CEO by including the CEO's target bonus as an explanatory variable in predicting target bonuses for non-CEO executives. Fisher and Govindarajan (1993) and Baiman et al. (1995) document a positive relation between a CEO's actual short-term bonus ratio and the corresponding ratio for (lower-level) profit center managers. Thus, we predict that a business unit executive's target bonus is positively associated with his CEO's target bonus.

IV. EMPIRICAL RESULTS

Descriptive Statistics

Table 3 presents descriptive statistics for the explanatory variables we use to test our first hypothesis. The statistics are for 238 CEOs and 2,743 distinct non-CEOs of 262 (of the original 397) firms that have complete information for all the explanatory variables. We average all variables with multiple manager-year observations (Firm sales, Business unit sales, Level, CEOs' T, and non-CEOs' T) over the sample period.

Panel A of Table 3 shows that the mean and median firm sales are $5.7 billion and $2.8 billion, respectively, whereas the corresponding sales for business units are $1 billion and $295 million. The mean and median variance of annual return-on-equity are 0.018 and 0.003, whereas the mean and median variance of annual stock return are 0.042 and 0.036, respectively. The mean and median market-to-book ratios are 2.33 and 1.84, respectively. These statistics are similar in magnitude to those documented in prior research (e.g., Gaver and Gaver 1993). Finally, the mean and median reporting levels of business unit executives are 3.5 and 3, respectively, which implies that the median business unit executive is two reporting levels below the CEO, and three reporting levels below the board of directors.

Panel B of Table 3 presents the correlation matrix among the variables used to test H1 for the non-CEO sub-sample only. Non-CEO executives' target bonuses are significantly negatively correlated with measurement error in accounting-based performance measures (variance of return on equity), positively correlated with business unit size (logarithm of sales), growth opportunities (market-to-book ratio), and their CEO's target bonus (CEOs' T), and negatively correlated with their reporting level in the organization. These findings provide preliminary univariate support for our first hypothesis.

Test of H1

Table 4 reports tests of H1 based on cross-sectional ordinary-least-squares regressions.[10] As in Table 3, we use one observation for each CEO and non-CEO executive by averaging

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[10] Multicollinearity in Table 4 is not a concern since the variance inflation factors for the regressions are all below 2, and the condition numbers are all below 5.

TABLE 3
Descriptive Statistics for Determinants of Executives' Target Bonuses[a]

Panel A: Descriptive Statistics

Variables	*n*	*Mean*	*Median*	*1st Quartile*	*3rd Quartile*	*Std. Dev.*
Variance of return on equity	262	0.018	0.003	0.001	0.009	0.066
Market-to-book ratio	262	2.325	1.844	1.371	2.954	1.386
Firm sales ($ millions)	262	5,715	2,781	1,343	6,750	8,047
Business unit sales ($ millions)	2,743	1,033	295	93	885	3,661
Variance of stock returns	262	0.042	0.036	0.028	0.048	0.027
Level (only non-CEOs)	2,743	3.5	3	3	4	1.02
CEOs' average target bonus (T)	238	0.613	0.582	0.500	0.694	0.230
Non-CEOs' average target bonus (T)	2,743	0.375	0.356	0.288	0.450	0.144

Panel B: Correlation Table for the Sample of Non-CEOs[b]

	Market-to-Book Ratio	*Logarithm of Sales*	*Logarithm of the Variance of Stock Returns*	*Level*	*Non-CEO's T*
Variance of return on equity	0.044	−0.020	**0.526**	0.029	**−0.041**
Market-to-book ratio		0.034	**−0.079**	−0.022	**0.098**
Logarithm of sales			**−0.151**	**−0.323**	**0.407**
Logarithm of the variance of stock returns				**−0.093**	0.028
Level					**−0.458**
CEO's T					**0.315**

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[a] The descriptive statistics are for 238 CEOs and 2,743 distinct non-CEOs of 262 firms. We compute a firm's variance of return on equity over the 10-year period prior to the first year that a firm's CEO or non-CEO executive appears in our sample. Market-to-book ratio is a firm's average ratio of market value of equity to book value of equity, computed over the 10-year period prior to the first year that a firm's CEO or non-CEO executive appears in our sample. Firm sales is a firm's average reported revenue for the years the firm is in our sample. Business unit sales is a business unit's average reported revenue for the years the business unit is in our sample. A firm's variance of stock returns is the time-series variance of annual stock returns over the 10-year period prior to the first year that a firm's CEO or non-CEO executive appears in our sample. Level is a non-CEO executive's average reporting level for the years the executive is in our sample. The CEO and non-CEO executives' T is their average target bonus scaled by their base salary.

[b] Bold numbers indicate significance at the 1% two-tailed level.

TABLE 4
Test of H1: OLS Estimates of the Determinants of Executives' Target Bonuses[a]

Model for CEOs: $T = \alpha + \beta_1 VarRoe + \beta_2 MTB + \beta_3 LnSales + \beta_4 VarRet$

Model for non-CEOs:

$$T = \alpha + \beta_1 VarRoe + \beta_2 MTB + \beta_3 LnSales + \beta_4 VarRet + \beta_5 Level + \beta_6 CEOsT$$

Independent Variables	*Predicted Sign*	*(1) CEOs*	*(2) Non-CEOs*	*(3) Non-CEOs*
Intercept		0.825** (3.58)	0.347** (6.72)	0.365** (7.39)
VarRoe	–	−0.005 (−0.59)	−0.005** (−3.01)	−0.005** (−3.18)
MTB	+	0.020* (2.29)	0.009** (4.91)	0.004* (2.09)
LnSales	+	0.083** (7.37)	0.027** (17.57)	0.022** (14.34)
VarRet	+	0.125** (4.01)	0.024** (3.40)	0.027** (4.03)
Level	–	—	−0.052** (−21.57)	−0.055** (−23.70)
CEOs *T*	+	—	—	0.150** (16.64)
Adjusted R^2		26.13%	33.87%	40.10%
F-statistic		6.24	83.62	99.76
n		238	2,743	2,657

*,** Significant at the 5%, and 1% two-tailed level respectively.

[a] The sample is 238 CEOs and 2,743 distinct non-CEOs of 262 firms. The CEO and non-CEO executives' T is their averaged target bonus scaled by their base salary. VarRoe is a firm's variance of return on equity over the 10-year prior to the first year that a firm's CEO or non-CEO executive appears in our sample. MTB is a firm's average ratio of market value of equity to book value of equity, computed over the 10-year period perior to the first year that a firm's CEO or non-CEO executive appears in our sample. LnSales is the natural logarithm of average firm sales and average business unit sales for the CEO and non-CEO regressions respectively. VarRet is the natural logarithm of the time-series variance of annual stock returns over the 10-year period prior to the first year that a firm's CEO or non-CEO executive appears in our sample. Level is a non-CEO executive's average reporting level for the years the executive is in our sample. We report coefficient estimates from OLS regressions and asymptotic t-statics based on White (1980) standard errors in parantheses.

multiple manager-year observations over the sample period.[11] Column (1) regresses the CEOs' average target bonuses on proxies for measurement noise in accounting-based performance metrics, firm growth opportunities, and the extent of CEOs' decision-making authority. Column (2) reports the corresponding results for non-CEO executives, including the executives' reporting level as an additional proxy for the extent of their decision-making authority. Finally, Column (3) reports results for non-CEO executives after including their CEO's average target bonus as a proxy for a firm's propensity to offer incentive compensation to its CEO. All columns also include industry dummy variables to control for industry differences in compensation practices.[12] For parsimony, we do not report these dummy-variable coefficients in Table 4.

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[11] We obtain qualitatively similar results if we treat each manager-year observation as independent but include year-dummy variables in the OLS regressions.

The results in Table 4 are generally consistent with our predictions. An executive's target bonus is significantly negatively related to VarRoe, our proxy for noise in accounting-based performance measures, but only for non-CEOs. An executive's target bonus is significantly positively related to his firm's market-to-book ratio consistent with incentive pay increasing in firm growth opportunities. The three proxies for the extent of an executive's decision-making authority have significant coefficients, all consistent with our predictions: target bonus increases with sales (LnSales) and the variance of stock returns (VarRet) and decreases with a non-CEO executive's reporting level (Level). Finally, a non-CEO executive's target bonus is significantly positively associated with his CEO's target bonus, our measure of the firm's propensity to offer incentive compensation to its CEO.

The evidence in Table 4 confirms that determinants of *ex post* actual bonuses identified in prior research (e.g., Fisher and Govindarajan 1993; Baiman et al. 1995; Nagar 2002) also explain firms' *ex ante* incentive design decisions, as reflected in executives' target bonuses. Our evidence also demonstrates that determinants of CEO incentive compensation, documented in studies such as Smith and Watts (1992) and Gaver and Gaver (1993), apply deeper in the organization to group, division, and plant managers. More importantly, the evidence that target bonuses reflect a firm's *ex ante* intentions to motivate its managers optimally provides a unique window through which we can draw inferences about the performance standards that underlie executives' target bonuses.

Test of H2a

Table 5 presents tests of H2a. As discussed earlier, if performance standards fully reflect an executive's past performance, then the probability that an executive receives at least his target bonus should be independent of whether he received an above-target or below-target bonus in the prior year. Table 5 shows that if a CEO received *at least* his target bonus last year, then he has a 72 percent chance of receiving *at least* his target bonus again this year. In contrast, if a CEO's bonus was less than his target bonus last year, then he has only a 42 percent chance of receiving at least his target bonus this year. A non-parametric test of differences in proportions shows that this difference is statistically significant with a z-statistic of 9.48.

Table 5 also shows that if a non-CEO executive received *at least* his target bonus last year, then he has a 69 percent chance of receiving at least his target bonus again this year. In contrast, if a non-CEO executive's bonus was less than his target bonus last year, then he has only a 42 percent chance of receiving at least his target bonus this year. Again, a non-parametric tests of differences in proportions shows that this difference is statistically significant with a z-statistic of 20.45.

The evidence in Table 5 rejects the null hypothesis that the probability that an executive will achieve his performance standard is independent of his past performance, suggesting instead that firms do not fully adjust standards to reflect executives' past performance.

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[12] We do not include firm fixed effects in the non-CEO regressions in Table 4 because they would subsume firm-level economic determinants of non-CEO target bonuses that we explicitly identify. Moreover, including the CEO's target bonus as an explanatory variable for the non-CEO regression in Model 3 controls for (unmodeled) firm-specific factors that affect non-CEO target bonuses.

TABLE 5
Test of H2a: Frequency Distribution of Executives' Actual vs. Target Bonuses in Consecutive Periods[a]

		CEOs (n = 970)		Non-CEO Managers (n = 5,722)	
		$A_t < T_t$	$A_t \geq T_t$	$A_t < T_t$	$A_t \geq T_t$
$A_{t-1} < T_{t-1}$	% of Total Observations	23.7	16.8	26.3	18.9
	% of Row Observations	58.5	**41.5**	58.2	**41.8**
$A_{t-1} \geq T_{t-1}$	% of Total Observations	16.7	42.8	17.2	37.6
	% of Row Observations	28.1	**71.9**	31.4	**68.6**

Tests: To test whether $\Pr(A_t \geq T_t | A_{t-1} \geq T_{t-1}) = \Pr(A_t \geq T_t | A_{t-1} < T_{t-1})$ we test for differences in proportions (indicated in bold), for CEOs and for non-CEOs separately. For CEOs, the z-statistic is 9.48, and for non-CEOs, the z-statistic is 20.45. Both are significant at the 1% level, rejecting the null.

[a] The sample for the test of H2a is 970 CEO-year and 5,722 non-CEO-year observations. A_t is the executive's actual bonus for fiscal year-t performance, scaled by his year-t base salary. T_t is the executive's target bonus for fiscal year-t performance, scaled by his year-t base salary. The rows in the table correspond to outcomes for which the executive's actual bonus was less than ($A_{t-1} < T_{t-1}$), or greater than or equal to ($A_{t-1} \geq T_{t-1}$) his target bonus in the prior year. The columns correspond to the same outcomes for the current year, t. For CEOs and non-CEOs separately, the upper number in each cell is the percentage of the total CEO or non-CEO observations that are classified in that cell. The lower number in each cell is the conditional percentage—the percentage of row observations corresponding to that cell.

Test of H2b

Table 6 presents tests of H2b. We regress CEO and non-CEO executives' abnormal bonus percentage, $(A_t - T_t)/T_t$, on its lagged value. We scale $(A_t - T_t)$ by T_t to adjust for cross-sectional variation in executives' target bonus percentages.[13] To allow for the possibility that firms revise standards asymmetrically depending on whether executives' past performance was above or below standard, we include a dummy variable and an interaction variable that measure the incremental response if an executive's actual bonus was less than his target bonus in the prior year. The OLS regression model that tests H2b is:

$$\frac{A_t - T}{T_t} = \alpha + \beta_1 \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right) + \beta_2 BELOW_{t-1} + \beta_3 BELOW_{t-1} \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right) + \text{year dummies} + \text{industry effects} + \varepsilon \quad (1)$$

where $BELOW_{t-1} = 1$ if $A_{t-1} < T_{t-1}$ and 0 otherwise.

If performance standards fully reflect executives' past performance, then executives' abnormal bonuses should be serially uncorrelated, and both β_1 and β_3 should not differ

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[13] We obtain qualitatively similar results when we test H2b using $A_t - T_t$, rather than $(A_t - T_t)/T_t$, as the dependent variable, and control for scale effects by including $1/T_t$ as an additional independent variable.

TABLE 6
Test of H2b: OLS Regressions of Persistence in Executives' Abnormal Bonus Percentages[a]

Model: $\frac{A_t - T_t}{T_t} = \alpha + \beta_1 \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right) + \beta_2 BELOW_{t-1} + \beta_3 BELOW_{t-1} \cdot \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right)$

	CEOs		*Non-CEOs*	
Independent Variables	*(1)* *Full Sample*	*(2)* *Zeros Excluded*	*(3)* *Full Sample*	*(4)* *Zeros Excluded*
Intercept	0.015 (0.29)	0.079 (1.68)	−0.005 (−0.21)	0.045* (2.04)
$\frac{A_{t-1} - T_{t-1}}{T_{t-1}}$	0.460** (8.09)	0.423** (8.55)	0.424** (17.28)	0.431** (18.75)
$BELOW_{t-1}$	−0.035 (−0.63)	−0.041 (−0.75)	−0.011 (−0.46)	0.005 (0.20)
$BELOW_{t-1} \cdot \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right)$	−0.169 (−1.82)	−0.274* (−2.18)	−0.148** (−3.55)	−0.247** (−4.74)
Adjusted R^2	22.00%	19.18%	17.35%	15.17%
F-statistic	14.50	10.40	59.33	44.86
F-test: $\beta_1 + \beta_3 = 0$	15.58**	1.73	67.63**	15.51**
n	957	792	5,555	4,907

*,**Significant at 5%, and 1% two-tailed level, respectively.

[a] Columns (1) and (2) estimate the regression model for CEOs, and columns (3) and (4) estimate the regression model for non-CEO executives, A_t is an executive's actual bonus for fiscal year t performance, scaled by his year-t base salary. T_t is an executive's target bonus for fiscal year t, scaled by his year-t base salary. $BELOW_{t-1}$ is a dummy variable equaling 1 if an executive's actual bonus was less than his target bonus in year t−1. The regressions also include year and industry dummy variables, but Table 6 suppresses the coefficients. All regressions include only executive-years with $A_t < 3T_t$, and $A_{t-1} < 3T_{t-1}$. In addition, columns (2) and (4) restrict the sample to only those executive-years where an executive earned a bonus in both years t and t−1 (A_t and $A_{t-1} > 0$). Asymptotic t-statistics based on White (1980) standard errors appear in parentheses.

significantly from zero—assuming, of course, that executives' abnormal bonuses are monotonically linked to their abnormal performance. The assumption that executives' abnormal bonuses mirror their abnormal performance clearly holds in the incentive zone depicted in Figure 1. However, this assumption does not hold if an executive performs below the minimum performance threshold where he earns no bonus or performs better than the performance maximum, since in these regions an executive's bonus is not monotonically linked to his performance. To control for executives' performance below the threshold, we test H2b based on a subsample that excludes observations where executives earn no bonus (Column (2) for CEOs and Column (4) for non-CEOs) as well as on the full sample (Column (1) for CEOs and Column (3) for non-CEOs). We lack data to directly identify cases where executives earn the maximum bonus. However, when executives earn the maximum bonus, their abnormal bonus is likely to be large. To help control for performance exceeding the performance maximum, and to mitigate the effects of extreme values, we exclude very large abnormal bonuses where an executive's bonus is more than three times his target bonus. Thus, the regressions in Columns (1) and (3) of Table 6 are based on 13 fewer CEO and 167 fewer non-CEO manager-years relative to Table 5.

To control for variation in average bonuses across years, detected in Table 2, all regressions in Table 6 include dummy variables for the year in which the executives earned their bonus. To control for (unmodeled) industry-wide factors that may give rise to persistent abnormal bonuses over time, all regressions in Table 6 also include industry fixed effects.[14] For parsimony, Table 6 suppresses the industry and year dummy-variable coefficients.

The regressions in Table 6 are statistically significant with adjusted R^2 ranging from 15 to 22 percent. The coefficient estimates on executives' past abnormal bonus percentage, $(A_{t-1} - T_{t-1})/T_{t-1}$, are positive and significant in all regressions, leading us to reject the null hypothesis (H2b) that executives' abnormal bonuses are serially uncorrelated.

In Column (1), an insignificant intercept implies that, after controlling for industry and year effects, CEOs receive their target bonuses, on average. However, if in the prior year a CEO's bonus exceeded his target bonus by 10.0 percent, then his current bonus exceeds the target bonus by 4.6 percent, on average.

Column (2), which is based on the reduced sample of CEO bonuses that excludes observations where the CEO failed to earn a bonus, suggests a similar result, except that the coefficient on the interaction term between the prior period abnormal bonus percentage and BELOW is significantly negative. Thus, if in the prior year a CEO's bonus exceeded his target bonus by 10.0 percent, then his current bonus on average exceeds his target bonus by 4.2 percent. However, if in the prior year a CEO missed his target bonus by 10.0 percent, then his current bonus misses the target bonus by only 1.5 percent (0.423 − 0.274 in Table 6) on average, which is insignificant based on an F-test of the sum of coefficients.

Columns (3) and (4) in Table 6 for non-CEO executives yield similar results. For example, Column (3) suggests that if in the prior year a business unit executive's bonus exceeded his target bonus by 10.0 percent, then his current bonus exceeds his target bonus by 4.2 percent. On the other hand, if a non-CEO executive missed his target bonus by 10.0 percent in the prior year, then his current bonus misses the target bonus by 2.8 percent (0.424 − 0.148 in Table 6) on average, which is statistically significant based on an F-test of the sum of coefficients.

The results in Table 6 suggest that firms do not fully adjust performance standards for executives' past performance. Moreover, we find mixed evidence of an asymmetric standard-setting process where firms revise future standards downward to reflect the bad times more aggressively than they revise them upward to reflect the good times.

V. SENSITIVITY ANALYSIS

Taken together, Tables 5 and 6 provide evidence rejecting H2a and H2b in favor of a standard-setting process that does not fully reflect executives' past performance. However, because our evidence concerning executives' performance standards is indirect, we investigate two alternative interpretations for our results.

Discretionary or Subjective Executive Bonuses

The frequency patterns in Table 5 and the serial correlations in Table 6 could arise if firms consistently pay executives discretionary or subjective bonuses for dimensions of executive performance that are not explicitly incorporated in performance standards. If such dimensions of performance persist through time, then executives' abnormal bonuses could be serially correlated even if executives' performance standards fully reflect all other dimensions of their past performance.

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[14] We exclude firm fixed effects in the Table 6 regressions because of data limitations in the CEO sample (i.e., most firms have only four observations during our sample period 1988–1994). For consistency, we also exclude firm fixed effects in the non-CEO regressions. However, in unreported tests, we find qualitatively similar results when we include firm fixed effects in the non-CEO regressions.

Bushman et al. (1995, 1996) report that 40 percent of their sample CEOs and approximately 50 percent of non-CEO business unit executives receive bonuses based on "individual performance evaluation," which they argue includes discretionary and subjective bonuses. For firms in our CEO sample with information about annual bonus criteria, collected as part of Hewitt Associates' annual survey, we repeat the frequency analysis reported in Table 5 after partitioning the firms into two groups: firms that use individual performance evaluation and those firms that do not. Untabulated results reveal that the frequency patterns reported in Table 5 are unaffected by firms' propensity to pay discretionary or subjective bonuses. That is, a CEO is more likely to earn an above-target bonus in the current year if he also earned an above-target bonus in the prior year—and this inference holds even for CEOs of firms that do not pay subjective bonuses (i.e., those firms that do not use individual performance evaluation).

We also repeat the CEO abnormal bonus regressions in Columns (1) and (2) of Table 6 after controlling for firms' use of individual performance evaluation. Including a dummy variable, IPE, which equals 1 if a CEO's bonus includes individual performance evaluation as a criterion, does not affect our inferences. The results, reported in Columns (1) and (2) of Table 7, show that the serial correlation in executives' abnormal bonuses occurs even after controlling for firms' use of subjective bonuses, and the insignificant interaction between the prior period abnormal bonus percentage and IPE suggests that individual performance evaluation is unrelated to the serial correlation in executives' abnormal bonuses.

Finally, we compare across the CEO and non-CEO executives in the frequency analysis reported in Table 5 as yet another way to corroborate our contention that discretionary bonuses do not affect our inferences. Murphy and Oyer (2002) provide evidence suggesting that CEOs are less likely than non-CEO executives to receive discretionary bonuses. Hence, if the payment of discretionary bonuses were responsible for the frequency patterns in Table 5, then we would expect these patterns to be more pronounced for non-CEOs. We find no significant differences across the CEO and non-CEO groups in Table 5. That is, a CEO's chances of receiving at least his target bonus given similar success in the prior period (72 percent in Table 5) is not significantly different than a non-CEO executive's corresponding chances of 69 percent (a test of differences in proportions yields a z-statistic of 1.5).

Performance Smoothing

Prior accounting research and the business press suggest that executives often engage in *income smoothing* because they perceive a benefit to reporting less-variable performance, or because reported performance may trigger real or perceived consequences around critical performance benchmarks (e.g., Dechow and Skinner 2000; Matsunaga and Park 2001). Executive bonus plans that have performance thresholds and caps similar to those in Figure 1 can motivate executives to smooth their reported performance. For example, Healy (1985) suggests that executives shift earnings to a subsequent period whenever performance is short of the threshold or exceeds the maximum. Gaver et al. (1995) and Holthausen et al. (1995a) also find that executives manage earnings downward when their reported performance exceeds the maximum, but show that executives manage earnings upward when below the threshold. An executive may also smooth performance in the incentive zone if the pay-for-performance relation is concave above the standard but convex below. For example, an executive can maximize his bonus by "saving" performance for a future period if he

TABLE 7
Robustness Tests
Persistence in CEOs' Abnormal Bonus Percentages, Subjective Individual Performance Evaluation, and Accruals[a]

Columns (1) and (2):

$$\frac{A_t - T_t}{T_t} = \alpha + \beta_1 \frac{A_{t-1} - T_{t-1}}{T_{t-1}} + \beta_2 BELOW_{t-1} + \beta_3 BELOW_{t-1} \cdot \frac{A_{t-1} - T_{t-1}}{T_{t-1}}$$
$$+ \beta_4 IPE + \beta_5 IPE \cdot \frac{A_{t-1} - T_{t-1}}{T_{t-1}}$$

Columns (3) and (4):

$$\frac{A_t - T_t}{T_t} = \alpha + \beta_1 \frac{A_{t-1} - T_{t-1}}{T_{t-1}} + \beta_2 BELOW_{t-1} + \beta_3 BELOW_{t-1} \cdot \frac{A_{t-1} - T_{t-1}}{T_{t-1}}$$
$$+ \beta_4 ACCRUAL + \beta_5 ACCRUAL \cdot \frac{A_{t-1} - T_{t-1}}{T_{t-1}}$$

	Subjective Individual Performance Evaluation[b]		*Accruals*[c]	
Independent Variables	*(1) Full Sample*	*(2) Zeros Excluded*	*(3) Full Sample*	*(4) Zeros Excluded*
Intercept	0.004 (0.05)	0.063 (0.98)	0.079 (0.95)	0.144 (1.84)
$\frac{A_{t-1} - T_{t-1}}{T_{t-1}}$	0.532** (7.44)	0.444** (6.88)	0.478** (5.01)	0.422** (4.97)
$BELOW_{t-1}$	−0.060 (−0.92)	−0.045 (−0.68)	−0.015 (−0.19)	−0.031 (−0.40)
$BELOW_{t-1} \cdot \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right)$	−0.286** (−2.60)	−0.315* (−1.96)	−0.098 (−0.55)	−0.123 (−0.52)
IPE	0.075 (1.77)	0.080 (1.85)	—	—
$IPE \cdot \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right)$	−0.041 (−0.57)	−0.016 (−0.19)	—	—
ACCRUAL	—	—	−0.074 (−0.99)	−0.067 (−0.96)
$ACCRUAL \cdot \left(\frac{A_{t-1} - T_{t-1}}{T_{t-1}}\right)$	—	—	0.083 (0.58)	0.054 (0.42)
Adjusted R^2	24.06%	19.35%	24.68%	20.83%
F-statistic	10.91	7.20	8.08	5.79
n	688	568	496	419

(Continued on next page)

TABLE 7 (Continued)

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[a] A_t is an executive's actual bonus for fiscal year t, scaled by his year-t base salary. T_t is an executive's target bonus for fiscal year t, scaled by his year-t base salary. $BELOW_{t-1}$ is a dummy variable equaling 1 if an executive's actual bonus was less than his target bonus in year t–1. IPE is a dummy variable equaling 1 if a CEO's bonus plan includes individual performance evaluation as a criterion in determining his annual bonus. ACCRUAL is a dummy variable equaling 1 if the absolute value of a firm's total accruals as a percentage of its book value of assets exceeds the sample median. Total accruals are the change in current assets – change in cash – change in current liabilities + change in short term debt – depreciation and amortization expense – deferred tax effect on earnings + equity investment income effect on earnings. The regressions also include year and industry dummy variables, but Table 7 suppresses the coefficients. All columns include only executive-years with $A_t < 3T_t$, and $A_{t-1} < 3T_{t-1}$. In addition, columns (2) and (4) restrict the sample to only those executive-years where an executive earned a bonus in both years t and t–1 (A_t and $A_{t-1} > 0$). Asymptotic t-statistics based on White (1980) standard errors appear in parentheses.

[b] Columns (1) and (2) estimate the regression model for CEO-years for which Hewitt Associates reports the firms' use (or nonuse) of individual performance evaluation as an executive bonus criterion.

[c] Columns (3) and (4) estimate the regression model for a subsample of CEO-year observations with available firm-level Compustat data to compute accounting accruals.

expects his performance to be above standard, and "borrowing" from a future period if he expects his performance to be below standard.

Executives can smooth performance by altering real economic transactions (e.g., capital expenditures) or by manipulating accounting reports (e.g., write-offs). Either way, smoothing reported performance may lead to a positive serial correlation in abnormal bonuses, at least when abnormal bonuses are a good proxy for abnormal performance. Hence, the issue is whether the positive correlation in executives' abnormal bonuses that we observe in Tables 5 and 6 could be attributed to executives smoothing their reported performance, and not to the way firms set and revise executives' performance standards.

Performance smoothing is not directly observable, so we cannot completely rule out this alternative interpretation of our results. Nevertheless, we test whether firms' *opportunity* to smooth performance, as proxied by the magnitude of accruals, affects our results. The magnitude of accruals captures both real smoothing of economic transactions and accounting manipulation. Since we have no priors as to how executives smooth their performance, we use the magnitude of *total accruals* as our proxy for performance smoothing rather than alternative measures such as *abnormal accruals* that exclude real smoothing.[15]

The magnitude of accruals is the sum of the change in current assets, change in short-term debt, and equity investment income, less the sum of the change in cash, change in current liabilities, depreciation and amortization expense, and deferred taxes. We rank our sample based on the absolute value of firms' total accruals (as a fraction of assets) and consider those firms ranked above the sample median as high-accrual firms and those ranked below the median as low-accrual firms. We then repeat the abnormal bonus regressions of Table 6 (Columns (1) and (2)) on the CEO sample, including a dummy variable ACCRUAL, that equals 1 for high-accrual firms and 0 otherwise.[16] If the serial correlation in abnormal bonuses is due to executives using accruals to smooth their reported performance, then the serial correlation in executives' abnormal bonuses should be more pronounced for high-accrual firms than for low-accrual firms. The results in Columns (3) and (4) of Table 7 provide no support for the contention that the time-series patterns in executives' abnormal bonuses is attributable to large accruals. Specifically, the results show that the serial correlation in abnormal bonuses occurs even after we control for the magnitude of accruals, and the insignificant interaction between prior period abnormal bonus percentage and ACCRUAL suggests that performance smoothing is unrelated to the serial correlation in executives' abnormal bonuses.

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[15] Dechow et al. (1995) suggest that total accruals provides as powerful a test of earnings management when earnings management is present in sample firms, as do other proxies for discretionary accruals. However, Dechow et al. (1995) also suggest that using total accruals, rather than some measure of abnormal accruals, yields more false positives when earnings management is in fact absent. Overestimating the incidence of earnings management biases our results against our hypothesis, and in favor of the alternative interpretation that smoothing explains the positive serial correlation in executives' abnormal bonuses.

[16] We confine our tests to the CEO sample because accruals are measured at the firm-wide level, and CEOs are more likely than non-CEO executives to have discretion over firm-wide accruals.

Overall, the sensitivity analysis in Table 7 confirms that firms do not fully adjust performance standards for executives' past performance. The evidence of an asymmetric standard-setting process, where firms revise future standards downward to reflect the bad times more aggressively than they revise them upward to reflect the good times, remains mixed.

VI. CONCLUSION

Executive bonus plans are a key component of corporate management control systems. Most bonus plans have a *target bonus*—the bonus a firm pays if an executive's performance equals a prespecified performance standard. For a large sample of CEOs and lower-level business unit executives, we find that the determinants of target bonuses are generally consistent with the implications of agency theory. Executives' target bonuses are negatively associated with a proxy for measurement noise in accounting-based performance metrics, and positively associated with proxies for firms' growth opportunities and the extent of executives' decision-making authority. Target bonuses for non-CEO executives are also positively associated with firms' propensity to offer incentive compensation to CEOs.

Since an executive earns more than his target bonus if he performs above his preestablished performance standard, and he earns less than his target bonus if he performs below standard, the probability that an executive earns more than his target bonus corresponds to the probability that he performs better than his standard. We use this link between executives' performance standards and their target bonuses to draw inferences about the (unobservable) performance standards that underlie executives' target bonuses. We find that an executive is more likely to receive an above-target bonus if he received an above-target bonus in the prior year than if he did not. This evidence suggests that firms do not update performance standards to fully reflect executives' past performance. We also find some support for a standard-setting process that responds asymmetrically to past performance, whereby firms revise standards downward more aggressively in bad times than they revise them upward in good times. We interpret our results as generally consistent with agency-theoretic arguments that, in dynamic environments, firms' commitment to discount executives' past performance in setting their future compensation improves managerial incentives to increase firm value.

Our study is subject to a number of limitations, most of which we acknowledge and address in our tests. Perhaps the most important limitation is that, due to lack of performance standard data, we compare executives' actual bonuses to their target bonuses in order to draw inferences about executives' performance standards. Although our evidence about performance standards is necessarily indirect, our inferences are valid in most contexts where executives' bonuses are explicitly linked to their performance (e.g., the incentive zone in Figure 1). Our inferences are also robust to alternative interpretations such as the possibility that executives receive discretionary bonuses or that they smooth reported performance. Thus, we believe our study provides important large-sample evidence on the evolution of executives' performance standards.

APPENDIX
Proofs

Proof of Observation 1

If the joint density of X_1 and X_2 is $f(x_1, x_2)$ and the marginal densities are $f(x_1)$ and $f(x_2)$, then we can write $p = \Pr(X_2 \geq S_2|X_1 \geq S_1)$ and $q = \Pr(X_2 \geq S_2|X_1 < S_1)$ as:

$$p = \frac{\int_{S_1}^{\infty} f(x_1) \int_{S_2}^{\infty} f(x_2|x_1)\, dx_2 dx_1}{\int_{S_1}^{\infty} f(x_1)\, dx_1} \quad \text{and} \quad q = \frac{\int_{-\infty}^{S_1} f(x_1) \int_{S_2}^{\infty} f(x_2|x_1)\, dx_2 dx_1}{\int_{-\infty}^{S_1} f(x_1)\, dx_1}$$

It follows that $p = q$ if and only if $\int_{S_2}^{\infty} f(x_2|x_1)\, dx_2 = \Pr(X_2 \geq S_2|X_1)$ does not depend on the realization of x_1.

It remains to show that $S_2 = E(x_2|x_1)$ is sufficient for $p = q$ when the joint distribution $f(x_1, x_2)$ is normal or lognormal. For a lognormal joint distribution $f(x_1, x_2)$, the conditional distribution, $f(x_2|x_1)$, is also lognormal and is given by:

$$f(x_2|x_1) = x_2^{-1}\, [2\pi(1 - \rho^2)\sigma_2^2]^{-1/2} \exp\left\{ - \frac{\left(\ln x_2 - \mu_2 - \rho \frac{\sigma_2}{\sigma_1} (\ln x_1 - \mu_1)\right)^2}{2(1 - \rho^2)\sigma_2^2} \right\}$$

where $(\ln x_1, \ln x_2)$ are joint normally distributed with parameters μ_1, μ_2, σ_1^2, σ_2^2, and ρ. Given the conditional distribution above, we derive $E(x_2|x_1)$, the conditional expectation of x_2 given x_1, as follows:

$$E(x_2|x_1) = \int_0^{\infty} x_2 f(x_2|x_1) dx_2 = \exp\left(\frac{(1 - \rho^2)\sigma_2^2}{2} + \mu_2 + \rho \frac{\sigma_2}{\sigma_1} (\ln x_1 - \mu_1)\right).$$

Now, letting $y = [(1 - \rho^2)\sigma_2^2]^{-1/2}\, [\ln x_2 - \mu_2 - \rho \frac{\sigma_2}{\sigma_1} (\ln x_1 - \mu_1)]$, we state $\Pr(X_2 \geq S_2|X_1)$ as:

$$\Pr(X_2 \geq S_2|X_1) = \int_{S_2}^{\infty} f(x_2|x_1)dx_2 = \int_{L}^{\infty} \frac{\exp[-\tfrac{1}{2}y^2]}{\sqrt{2\pi}}\, dy$$

where $L = [(1 - \rho^2)\sigma_2^2]^{-1/2}(\ln S_2 - \mu_2 - \rho\, (\sigma_2/\sigma_1)\, (\ln x_1 - \mu_1))$.

Therefore, if $S_2 = E(x_2|x_1)$, $L = [(1 - \rho^2)\sigma_2^2]^{1/2}/2$, which does not depend on x_1. Hence $\Pr(X_2 \geq S_2|X_1)$ does not depend on the realization of x_1, which implies $p = q$.

A comparable derivation illustrates that $S_2 = E(x_2|x_1)$ is sufficient for $p = q$ when the joint distribution $f(x_1, x_2)$ is normal.

Linking Abnormal Bonuses and Abnormal Performance

Let $A_t - T_t = g_t(X_t - S_t)$ define the pay-for-performance over the incentive zone where $g_t(\cdot)$ is a continuous monotonic function and $g_t(0) = 0$. We have:

$$
\begin{aligned}
E(A_2 - T_2|A_1 - T_1) &= E[g_2(X_2 - S_2)|g_1(X_1 - S_1)] \\
&= E[g_2(X_2 - S_2)|X_1 - S_1] \text{ by the invertability of } g_1(\cdot).
\end{aligned}
$$

Using Taylor's Series approximation, we have:

$$
\begin{aligned}
E(A_2 - T_2|A_1 - T_1) &\cong E[g_2(0) + g_2'(0)(X_2 - S_2) + g_2''(0)(X_2 - S_2)^2|X_1 - S_1] \\
&\cong g_2'(0)[E(X_2|X_1) - S_2] + g_2''(0)E[(X_2 - S_2)^2|X_1 - S_1]
\end{aligned}
$$

If $S_2 = E(X_2|X_1) + +$ constant, then:

$$E(A_2 - T_2|A_1 - T_1) \cong \text{constant} + g_2''(0)\text{Var}(X_2|X_1).$$

It follows that if the pay-for-performance relation is linear (i.e., $g_2''(0) = 0$) or the bivariate distribution of X_1 and X_2 is normal (so that $\text{Var}(X_2|X_1)$ is independent of the realization of X_1), then abnormal bonuses are serially uncorrelated.

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Changes in Analysts' Information around Earnings Announcements

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ABSTRACT: In this study we examine changes in the precision and the commonality of information contained in individual analysts' earnings forecasts, focusing on changes around earnings announcements. Using the empirical proxies suggested by the Barron et al. (1998) model that are based on the across-analyst correlation in forecast errors, we conclude that the commonality of information among active analysts decreases around earnings announcements. We also conclude that the idiosyncratic information contained in these individual analysts' forecasts increases immediately after earnings announcements, and that this increase is more significant as more analysts revise their forecasts. These results are consistent with theories positing that an important role of accounting disclosures is to trigger the generation of idiosyncratic information by elite information processors such as financial analysts (Kim and Verrecchia 1994, 1997).

Keywords: *common information; idiosyncratic information; earnings announcements; financial analysts; forecast aggregation.*

Data Availability: *All data are available from public sources.*

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I. INTRODUCTION

Theoretical models traditionally regard public earnings announcements as commonly observed by all market participants (e.g., Diamond 1985; Kim and Verrecchia 1991, among others). Such public information reduces market participants' incentives to develop private information, if the public and private information are substitutes. This in turn reduces the amount of uniquely private (or idiosyncratic) information in the market (see Verrecchia 1982; Diamond 1985; Kim 1993).

In contrast to the traditional view, another stream of theoretical research posits that public announcements can increase idiosyncratic beliefs among market participants. For example, public announcements create idiosyncratic beliefs in Varian (1989), Harris and Raviv (1993), and Kandel and Pearson (1995) because market participants possess different prior beliefs or likelihood functions. Holthausen and Verrecchia (1990) and Indjejikian (1991) model public disclosures as signals with both common and idiosyncratic error components, which is equivalent to assuming that public announcements can create idiosyncratic beliefs. In Kim and Verrecchia (1994, 1997) a public release of information triggers agents with diverse information-processing skills to generate new idiosyncratic information from the public announcement.

The question of whether public accounting disclosures trigger significant idiosyncratic information is central in understanding the role of accounting disclosures. For example, if public disclosures of accounting information trigger significant generation of idiosyncratic information, then it becomes important to know (1) who is generating this information, (2) how they are generating and transmitting this idiosyncratic information to market participants, and (3) whether the total effect of such releases is to level the informational playing field (Lev 1988), or the opposite.

Empirical evidence relating to this issue is limited and mixed. Krinsky and Lee (1996) show that the asymmetric information component of bid-ask spreads increases around earnings announcements, and Kandel and Pearson (1995) show that earnings announcements that do not lead to price changes nonetheless stimulate trading. Both of these results are consistent with earnings announcements stimulating production of diverse idiosyncratic information. However, this evidence from financial markets is indirect and limited because the observable variables, bid-ask spreads and trading volume, are market aggregates tied to the accounting users' information through a complex and not completely understood market mechanism. The resulting inferences from such evidence are therefore indirect, often tenuous, and subject to alternative explanations.

Rather than using market aggregates to draw inferences about investors' use of accounting information, we focus on another major user of accounting information, financial analysts. Evidence on how analysts' information changes around earnings announcements provides valuable insights into how accounting releases influence the beliefs of analysts, who are themselves important users of accounting information (Schipper 1991).

More importantly, to the extent that analysts' information mirrors that of investors, the availability of analysts' earnings forecasts provides more direct evidence about analysts'/investors' information than that inferred from financial market aggregates alone. Since analysts tend to revise their forecasts of future earnings after the announcement of prior earnings (Stickel 1989), we can directly investigate the effect of prior annual and interim earnings announcements on analysts' forecasts of upcoming annual earnings. Our transformation of forecast data into estimates of important properties of analysts' information follows from the Barron, Kim, Lin and Stevens (1998) (hereafter BKLS) model of how analysts transform their information into forecasts.[1]

Using the method developed by BKLS, which is based on the across-analyst correlation in forecast errors, we develop separate empirical proxies for the precisions of individual analysts' common information and idiosyncratic information, both before and after prior annual and quarterly earnings announcements. We use these estimates to draw inferences about the extent to which earnings announcements stimulate the generation of idiosyncratic information relative to common information. Because we cannot directly observe the extent to which analysts possess common versus idiosyncratic information, our evidence is necessarily indirect. Nonetheless, subsequent analyses suggest that our inferences are unlikely an artifact of violations of the assumptions necessary for the BKLS measures to be valid estimates of both the unobservable level of commonality among analysts, and the precisions of individual analysts' common information and idiosyncratic information.

Our empirical analyses lead us to draw the following conclusions. Early forecasts of annual earnings, issued more than one year before the firm announces these earnings, contain mostly common information. Later forecasts reflect both more common and more idiosyncratic information. Increases in common and idiosyncratic information concentrate around prior earnings announcements. Our results suggest that the absolute increases in the precision of common information are larger than the increases in the precision of idiosyncratic information. However, we find that the *percentage* increases in idiosyncratic information are greater, and this in turn decreases commonality—the fraction of information that is common to all forecasts.

Our conclusion that earnings announcements *reduce* the commonality of the information contained in individual analysts' forecasts starkly contrasts with inferences that could be drawn from prior research on changes in the variation or "dispersion" in forecasts. For example, Brown and Han (1992) show that most earnings announcements result in a decrease in forecast variance (dispersion), a result that could be interpreted as indicating that earnings announcements increase the commonality of information among analysts. However, BKLS show that forecast dispersion reflects not only the (lack of) commonality in analysts' information, but also the lack of precision (or level of uncertainty) in individual analysts' forecasts.[2] Since forecast precision increases over time as analysts obtain more information, regardless of whether that information is common or idiosyncratic, forecast dispersion can decrease even if the commonality of analysts' information decreases. We use the BKLS commonality measure, which abstracts from the effects of changes in precision, in order to better isolate the degree of commonality in analysts' information.

Our evidence that earnings announcements decrease the commonality of the information contained in individual analysts' forecasts of future earnings suggests that investors' demand for individual forecasts is likely to increase after earnings announcements. For our sample of actively updating analysts, we find that as more analysts revise their forecasts after earnings announcements, there are larger increases in the precision of idiosyncratic information, and larger decreases in the commonality of information. This result is consistent with the view that analysts fulfill an important role as sophisticated processors of accounting disclosures. In addition, if individual forecasts convey relatively more idiosyncratic information after earnings announcements (i.e., there is lower commonality), then investors should expect the mean forecast to be a more efficient aggregator of individual analysts' diverse idiosyncratic information after an earnings announcement than before the announcement. Our empirical tests confirm this expectation.

---

[1] The BKLS model converts the properties of forecasts into the properties of analysts' information under the assumption that *information contained in forecasts* is *information analysts observe or possess*. The two are the same only if analysts faithfully report forecasts strictly based on their available information. We examine the robustness of our results to this and other assumptions in Section V.

[2] Prior studies have used forecast dispersion (variance) as a proxy for both commonality (e.g., Ziebart 1990) and uncertainty (e.g., Imhoff and Lobo 1992; Barron and Stuerke 1998).

The remainder of this paper is organized as follows. In Section II we explain how public announcements trigger the generation of new idiosyncratic information in the Kim and Verrecchia (1994, 1997) models, our expectations regarding changes in the commonality of analysts' information, and how BKLS infer the properties of analysts' information from their earnings forecasts. We describe our study design and data in Section III, and present the empirical results in Section IV. Section V addresses the simplifying assumptions of the BKLS model and presents empirical analyses supporting the robustness of our results. We conclude in Section VI.

II. THEORETICAL BACKGROUND AND BKLS METHOD

In this section we begin by briefly using the models of Kim and Verrecchia (1994, 1997) to explain how public accounting disclosures can provide analysts with the means (or the triggering mechanism) by which to produce idiosyncratic information. We then use the Indjejikian (1991) and Fischer and Verrecchia (1998) models to identify analysts' motives to increase the idiosyncratic information conveyed by their forecasts. Finally, we outline how BKLS provide a method of estimating the characteristics of analysts' information in the above models, based on transformations of the observable properties of analysts' forecasts.

The Means by which Earnings Announcements May Trigger Idiosyncratic Information

As in Kim and Verrecchia (1994, 1997), consider an environment in which period $t-1$ earnings (y_{t-1}) provides information about a period t signal (e.g., earnings (y_t)) such that:

$$y_{t-1} = y_t + \varepsilon. \qquad (1)$$

Analyst i observes a private signal of the form $O_i = \varepsilon - \varepsilon_{Oi}$, where ε_{Oi} is idiosyncratic error. The analyst cannot use this "O" signal by itself to learn about y_t. However, once combined with the public earnings announcement of y_{t-1}, it triggers a new uniquely private (idiosyncratic) signal about y_t such that:

$$y_{t-1} - O_i = y_t + \varepsilon_{Oi}. \qquad (2)$$

The analyst could obtain such indirect "O" knowledge before or after the triggering public earnings announcement. "O" knowledge can come in many forms. For example, it could be firm-specific knowledge about events affecting future performance such as planned capital expenditures or an important customer's nonrenewal of a contract. Such knowledge can also be analyst specific. For example, consider two analysts: The first is a Certified Public Accountant who understands when and how specific accruals will reverse and affect future earnings. The second, a former political economist, correctly expects that backorders from a Brazilian customer will soon decline and decrease earnings. Neither of these analysts can use his or her indirect "O"-type knowledge in isolation. Yet they can combine their knowledge with reported earnings to produce "uniquely privately inferred information" about future earnings.[3]

Analysts' Motives to Convey Idiosyncratic Information in Forecasts

In Kim and Verrecchia (1994, 1997), changes in the commonality of information contained in analysts' forecasts around earnings announcements are mainly determined by exogenous factors, and thus they are silent on the factors influencing the commonality of information in analysts' forecasts. Fischer and Verrecchia's (1998) theoretical study analyzes factors influencing the commonality of information contained in individual analysts' forecasts. The Fischer and Verrecchia (1998) model highlights the tension between two opposing forces. On the one hand, an analyst and his clients are adversely affected by price changes caused by the announcement of other analysts' forecasts that are inconsistent with the analyst's own forecast. This force decreases the analyst's incentives to impound idiosyncratic information in his forecast. On the other hand, the analyst and his clients can benefit if the analyst impounds more idiosyncratic information in his forecast, (i.e., leading to less correlated forecast errors among analysts), to the extent that the analyst expects greater profits from trading on his idiosyncratic information. Fischer and Verrecchia (1998) show that risk-averse analysts prefer more correlated forecast errors, because they (or their preferred clients) are relatively more concerned about the adverse effects of price changes caused by the announcement of other analysts' forecasts that are inconsistent with their own forecasts. Conversely, the less risk-averse among analysts prefer less correlated forecast errors because they are more concerned with reaping the benefits of lower direct competition among analysts due to the greater expected profits that accrue to trading on each analyst's own information. This tension identified in Fischer and Verrecchia's (1998) model suggests that analysts prefer less correlated forecast errors when they are either less risk-adverse or, more broadly, less exposed to the price changes caused by the announcement of other analysts' forecasts.

In the context of our study, analysts are less concerned about the adverse effects of other analysts' forecasts as more investors' trading horizons extend to the revelation of the variable being forecasted, which in our case is an annual earnings announcement. Investors' trading horizons are increasingly likely to extend to this annual announcement if, as the announcement draws nearer, investors are more likely to delay unwinding their investment positions until after the announcement. In this case, then with the passage of time (and prior earnings announcements), reducing direct competition among analysts becomes individual analysts' dominant concern and they prefer less correlated errors.

In another related theoretical study, Indjejikian (1991) examines the effect of costly information interpretation (or processing) on the optimal level of idiosyncratic interpretation investors will demand at varying levels of public disclosure by a firm's manager. His model suggests that when individual investors are equally well informed and risk-tolerant, the disutility that arises from all investors becoming equally more informed by increased public disclosure increases investor demand for idiosyncratic interpretations of the disclosure.[4]

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[3] Evidence from interviews with stock analysts reported in *The Wall Street Transcript* supports the existence of such knowledge. Specifically, there are differences in the analytic models analysts say they use to forecast earnings.

[4] Our purpose in this section is to identify theories that suggest theoretically motivated reasons why analysts may prefer to reduce the correlation in forecast errors, and not to make predictions about the effects of any type of event on this correlation. Indjejikian (1991) only suggests investor benefits in the form of higher expected trading gains and more efficient risk sharing that *may* result in a lower correlation in forecast errors *holding other factors constant*. This *ceteris paribus* assumption may not hold before and after specific types of events (e.g., earnings announcements) because other factors besides the level of management disclosure may change after the event. For example, there may be a change in the nature of investor clientele after the event.

One could view information intermediaries such as analysts as the natural market agents to provide such idiosyncratic interpretations.

The BKLS Correlation Measure (ρ) and Precision Measures (h and s)

We measure the commonality of analysts' information using the BKLS correlation measure (ρ), which is a measure of the across-analyst correlation in forecast errors and is thus a direct measure of the construct examined by Fischer and Verrecchia (1998). One can also interpret the BKLS correlation metric as a measure of the consensus construct in Holthausen and Verrecchia (1990) and Indjejikian (1991). BKLS define *consensus* as the across-analyst correlation in forecast errors, which can also be expressed as the ratio of the common forecast error to the average total forecast error. BKLS show that one can estimate the common and total forecast errors, and in turn *BKLS consensus*, using observable features of analysts' forecasts, as follows:

$$\rho = \frac{C}{V} = \frac{\text{Common Forecast Error}}{\text{Average Total Error}} = \frac{\left(SE - \frac{D}{N}\right)}{\left(SE - \frac{D}{N}\right) + D}, \tag{3}$$

where:

C = the expected covariance of error in individual forecasts;
V = the expected variance of error in individual forecasts;
SE = the expected squared error in the mean forecast;
D = the expected sample variance (or dispersion) in forecasts; and
N = the number of forecasts.

BKLS further show that under certain assumptions (which we review in Section V), one can use this measure of commonality in analysts' forecast errors to draw inferences about the information individual analysts possess at the time they report their forecasts. Let the precision of individual analysts' common and idiosyncratic information be h and s, respectively. Then, BKLS (p. 427) show that $\rho = h \times V = h/(h + s)$. In other words, the BKLS correlation measure captures the ratio of the precision of analysts' common information to the precision of their total information. Under the assumptions necessary to use BKLS measures to draw inferences about the information individual analysts possess, the common-to-total precision ratio coincides with the common-to-total error ratio (or BKLS correlation) because the commonality of analysts' forecast errors is due solely to the commonality of their information. As with estimation of ρ, estimation of h and s is based on observable features of analysts' forecasts (BKLS, 428):

$$h = \rho \times \frac{1}{V} = \frac{\left(SE - \frac{D}{N}\right)}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2}, \tag{4}$$

$$s = (1 - \rho) \times \frac{1}{V} = \frac{D}{\left[\left(SE - \frac{D}{N}\right) + D\right]^2}. \tag{5}$$

The BKLS measures allow us to use observable attributes of analysts' forecasts to derive empirical estimates of two key (unobservable) dimensions of an information structure among multiple analysts: (1) the precision of their information (i.e., the inverse of uncertainty), and (2) the commonality among different analysts' beliefs (i.e., their consensus), measured as the across-analyst correlation in forecast errors. The omission of either dimension cripples a researchers' ability to characterize multiple analysts' information. For example, BKLS (p. 427) show that the expected dispersion in analysts' forecasts, D, equals $(1 - \rho) \times V$. That is, forecast dispersion equals 1 minus commonality (i.e., diversity) times uncertainty (i.e., lack of precision). Thus, the use of dispersion as a proxy either for commonality alone (as in Ziebart 1990) or for uncertainty alone (as in Imhoff and Lobo 1992) may lead to erroneous interpretations of empirical evidence.

III. STUDY DESIGN AND SAMPLE

Selection of Individual Forecasts Used to Calculate BKLS Measures

We examine forecasts of annual earnings that analysts issued immediately before and after four prior earnings announcements: the prior year's annual earnings announcement and the subsequent announcements of first, second, and third quarter earnings for the current year. We view these four prior earnings announcements as noisy signals about future annual earnings. Our goal is to ascertain how these (prior) earnings announcements affect the nature of the information contained in individual analysts' forecasts of upcoming annual earnings.[5] The last three earnings announcements, however, represent partial realizations of the upcoming forecasted annual earnings. This is a limitation of our study design, as one could alternatively view the interim earnings announcements, which realize part of the upcoming annual earnings being forecasted, as altering the components of annual earnings that analysts are forecasting before and after interim earnings announcements. Our primary results, however, are not attributable to this aspect of our study design, because we obtain consistent results using alternative designs where the prior earnings announcements are not partial realizations of the earnings number being forecasted. Specifically, we obtain identical inferences (1) for prior annual earnings announcements, which, of course, are not partial realizations of next year's annual earnings, and (2) in robustness tests examining the effects of prior earnings announcements on the information contained in analysts' forecasts of first, second, third, and fourth quarter earnings.[6]

We use the 45-day period prior to the Compustat earnings announcement date as the pre-announcement forecast window (days −45 through −1, inclusive, where day 0 is the Compustat earnings announcement date), and the 30-day period immediately after as the post-announcement forecast window (days 0 through +29, inclusive). We use a longer pre- than post-announcement window because analysts are less likely to update their forecasts of (future) annual earnings immediately before prior earnings announcements than immediately after the earnings announcement (Stickel 1989).[7]

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[5] The BKLS method generates measures of analysts' residual common and idiosyncratic uncertainty regardless of the model used to link the forecast measures to analysts' information. BKLS also show that in a simple information structure, analysts' residual uncertainty is the inverse of the precision of analysts' information. Then, the common-to-total uncertainty ratio (the BKLS measure of commonality) is equivalent to analysts' common-to-total information ratio. The same interpretation is valid in our context with quarterly earnings if we treat quarterly earnings as a signal about annual earnings.

[6] In a related study, Byard (1998) shows that, on average, the commonality of analysts' information about quarterly earnings decreases over time, although he does not examine the specific effects of prior earnings announcements.

To be included in our sample, an individual analyst must issue an earnings forecast for a firm in the 45-day pre-announcement window and then update that forecast in the 30-day post-announcement window. This procedure ensures that when comparing the BKLS measures of analysts' information before and after earnings announcements, we base our analysis on the same set of individual analysts.[8]

We substitute *ex post* realized dispersion ($\hat{D}$) and squared error in these analysts' mean forecast ($\hat{SE}$) for the *expected* dispersion (D) and squared error in the mean forecast (SE) used in the BKLS model. We calculate *ex post* measures as follows:

$$\hat{SE}_{jtw} = (A_{jt} - \bar{F}_{jtw})^2, \tag{6}$$

$$\hat{D}_{jtw} = \frac{1}{N_{jtw} - 1} \sum_{i=1}^{N} (F_{ijtw} - \bar{F}_{jtw})^2, \tag{7}$$

where $\hat{SE}_{jtw}$ is the observed squared error in the mean forecast among our selected analysts' forecasts for firm j in year t for forecast window w; $\hat{D}_{jtw}$ is the observed dispersion among our selected analysts' forecasts for firm j in year t made during forecast window w; A_{jt} is the actual earnings for firm j in year t; F_{ijtw} is the forecast from analyst i for firm j in year t, made during forecast window w; $\bar{F}_{jtw}$ is the mean of the analysts' forecasts for firm j in year t, made during forecast window w; and N_{jtw} is the number of selected forecasts for firm j in year t, made during forecast window w. Substituting *ex post* realizations for expected values introduces measurement error into our measures of expected dispersion and squared error in the mean forecast and, in turn, into our estimates of BKLS measures. Such measurement error is ameliorated in large samples of firm-years such as those used here.

Sample Selection

Our sample includes firm-years from 1986 to 1997 that meet the following three requirements:

1. Quarterly earnings announcement dates for first, second, third, and fourth quarter earnings for the current year, and for the fourth quarter of the prior-year, are available from either the Compustat Active or Research quarterly files.
2. Annual EPS data for the current year are available from the I/B/E/S Actual earnings announcements file.

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[7] The 30-day post-announcement forecast window likely encompasses the filing and public release of a firm's 10-K and 10-Q reports following the announcement of a firm's prior-year annual and interim earnings, respectively (Easton and Zmijewski 1993). Thus, not only do forecast revisions issued during the 30-day post-announcement forecast window reflect analysts' reactions to the earnings announcement, but these forecast revisions may also reflect analysts' reactions to the information contained in the 10-K and 10-Q reports. In subsequent tests using shorter forecast windows (a 10-day post-announcement forecast window), however, the tenor of our results is unchanged.

[8] This procedure also ensures that the number of forecasts for each firm-year are the same across each matched pre- and post-earnings announcement window. As a result, our analysis of the pre- vs. post-earnings announcement changes in BKLS measures are not distorted by changes in the number of forecast observations (N) used to estimate BKLS measures.

3. Forecasts of annual earnings for the current year are available on the I/B/E/S Detail file from the same individual analysts in both the 45-day forecast window before and the 30-day forecast window after the Compustat earnings announcement dates. Pre- and post-announcement period forecasts must be available from at least two analysts for each of the four prior earnings announcements.[9]

The resulting sample contains 990 firm-years matched across all eight forecast windows (pre and post windows for the four prior earnings announcements). This sample consists of firm-year observations from 365 distinct firms spread fairly equally across the 12 sample years. Although these data requirements restrict the sample to relatively heavily followed (large) firms, with relatively surprising earnings, these sample selection criteria also allow us to compare changes in analysts' information throughout the year prior to annual earnings announcements for a consistent set of firm-years.[10] We subscript forecast windows before earnings announcements by B, and those after earnings announcements by A; we denote prior-year annual earnings announcements as PY, while we denote the first, second, and third quarter earnings announcements for the current year as Q1, Q2, and Q3, respectively. Thus, our eight forecast windows consist of four 45-day forecast windows before (prior) earnings announcements denoted PY_B, $Q1_B$, $Q2_B$, and $Q3_B$, and four matched 30-day forecast windows after these prior earnings announcements, denoted PY_A, $Q1_A$, $Q2_A$, and $Q3_A$.

IV. RESULTS

Descriptive Statistics

We compute estimates of ρ, h, and s—$\hat{\rho}$, $\hat{h}$, and $\hat{s}$—by substituting the number of our selected analysts' forecasts ($\hat{N}$), together with the *ex post* realized dispersion ($\hat{D}$) and squared error in the mean forecast ($\hat{SE}$) for these forecasts, both scaled by the absolute actual earnings per share, into the BKLS equations shown here in Equations (3), (4), and (5). Our inferences are unchanged when we scale by stock price at fiscal year-end or when we use unscaled data. We compute $\hat{\rho}$, $\hat{h}$, and $\hat{s}$ for forecasts of annual earnings both before (denoted B) and after (denoted A) the announcement of prior annual and interim earnings.

To describe the characteristics of our sample, we use as an example the $Q2_B$ forecast window (results are similar for the other seven forecast windows). Table 1 reports that the median values of scaled $\hat{D}$ and $\hat{SE}$ for this forecast window are 0.008 and 0.037, respectively. The median number of forecasts selected ($\hat{N}$) is 4, whereas the median total analyst following (FOLLOWING) for these 990 firm-years is 33.[11] Our selected analysts are more active than those not selected. Untabulated analyses reveal that of all the analysts forecasting for these 990 firm-years, our selected analysts issued a median of 14 total forecasts per

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[9] In rare cases in which multiple forecasts are available from the same individual analyst within a forecast window, we use the forecast closest to the earnings announcement date.

[10] Of the 365 different firms in our sample, 162 firms contribute only 1 firm-year to our sample. Running an analysis on only this sample of 162 firm-years produces similar results, so our results are not attributable to lack of independence (see Keane and Runkle 1998) among multiple firm-year observations for the same firms. Our inferences are also unaffected when we adjust each individual forecast for the average level of optimism in that forecast window in that year. In addition, we relax the requirement that a firm-year be matched across all eight forecast windows. Simply matching across forecast windows before and after each earnings announcement produces larger samples of firm-years for each of the four earnings announcements (all greater than 2,500 firm-year observations). Using these samples produces results consistent with those reported in Tables 2 through 4.

[11] The median number of individual forecasts matched before and after the prior annual, the first quarter, and second quarter earnings announcements is four. The median number of individual forecasts matched before and after the third quarter announcement is five. The total analyst following (FOLLOWING) is the total number of analysts who issued at least one forecast (of annual or quarterly earnings, or of long-term growth rate) during the corresponding Compustat calendar year.

TABLE 1
Summary Statistics for a Sample of Forecasts of Annual Earnings Made in the 45-Day Period before Second Quarter Earnings Announcements ($Q2_B$ Forecast Window)
($n = 990^a$; Sample Period: 1986 through 1997)

	Mean	*Median*	*Std. Dev.*
$\hat{D}$/|Actual EPS|	0.067	0.008	0.279
$\hat{SE}$/|Actual EPS|	0.638	0.037	2.928
$\hat{N}$	5.30	4	3.334
FOLLOWING	34.23	33	13.31
Market Capitalization (n = 832)[b]	7,401	2,878	12,072
$\hat{\rho}$–BKLS Correlation	0.626	0.833	0.446
$\hat{h}$–BKLS Precision of Common Information	118.28	6.793	2,077
$\hat{s}$–BKLS Precision of Idiosyncratic Information	207.84	2.691	2,360

where:

$\hat{D}$/|Actual EPS| = the sample variance (or dispersion) in forecasts, deflated by absolute value of actual EPS;
$\hat{SE}$/|Actual EPS| = the squared error in the mean forecast, deflated by absolute value of actual EPS;
$\hat{N}$ = the number of forecasts selected;
FOLLOWING = the total number of analysts following a firm (any analyst who issued at least one forecast for a firm during that year);
Market Capitalization = measured at end of the fiscal year (shown as millions of 1985 CPI-deflated dollars); and
$\hat{\rho}$ (BKLS correlation) = a measure of the commonality in analysts' information, as captured by the across-analyst correlation in forecast errors.

The BKLS variables are calculated as follows:

$$\hat{\rho} = \frac{\hat{SE} - \frac{\hat{D}}{\hat{N}}}{\left(1 - \frac{1}{\hat{N}}\right)\hat{D} + \hat{SE}} = \text{BKLS Correlation (the across-analyst correlation in forecast errors).}$$

As explained in Section V, the BKLS correlation measure ($\hat{\rho}$) does not require as many restrictive assumptions as the BKLS precision measures ($\hat{h}$ and $\hat{s}$). However, to the extent that the additional assumptions underlying $\hat{h}$ and $\hat{s}$ are met, we can interpret $\hat{\rho}$ as: $\hat{\rho} = \hat{h}/(\hat{h} + \hat{s})$.

$$\hat{h} = \frac{\left(\hat{SE} - \frac{\hat{D}}{\hat{N}}\right)}{\left[\left(\hat{SE} - \frac{\hat{D}}{\hat{N}}\right) + \hat{D}\right]^2} = \hat{\rho} \times \frac{1}{\hat{V}} = \text{BKLS Correlation} \times (1/\text{Total Forecast Error}),$$

$$= \text{Prevision of Common Information.}$$

(*Continued on next page*)

TABLE 1 (Continued)

and

$$\hat{s} = \frac{\hat{D}}{\left[\left(\hat{SE} - \frac{\hat{D}}{\hat{N}}\right) + \hat{D}\right]^2} = (1 - \hat{\rho}) \times \frac{1}{\hat{V}} = (1 - \text{BKLS Correlation}) \times (1/\text{Total Forecast Error}),$$

$$= \text{Precision of Idiosyncratic Information.}$$

where $\hat{V}$ = variance of error in individual analysts' forecasts.

We use $\hat{SE}$/|Actual EPS| and $\hat{D}$/|Actual EPS| in our calculations of BKLS measures. Using unscaled $\hat{D}$ and $\hat{SE}$, or scaling by stock price at fiscal year-end, produces consistent results.

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[a] Sample of 990 firm-years matched across all eight forecast windows. We selected forecasts of annual earnings from matched forecast windows before and after four prior earnings announcements: the prior year earnings announcement (denoted PY), and the announcements of first, second, and third quarter earnings for the current year (denoted Q1, Q2 and Q3, respectively).

[b] Of the 990 firm-year sample, only 832 firm-years have Compustat market capitalization data.

firm during the fiscal year, whereas the corresponding median for the analysts we did not select for these firm-years was only six forecasts. Our sample is also composed of larger firms. Of the 990 firm-years, we could match 832 with Compustat market capitalization data. As Table 1 shows, the median market capitalization for these 832 firm-years (all deflated to 1985 Consumer Price Index dollars) is \$2.878 billion. This substantially exceeds the \$111 million median market capitalization for all matched I/B/E/S-Compustat firm-years for our sample period.

Table 1 shows the median and mean levels of BKLS correlation ($\hat{\rho}$) and the BKLS precision measures ($\hat{h}$ and $\hat{s}$) for the $Q2_B$ forecast window. The distributions of BKLS measures are skewed (untabulated Shapiro-Wilk statistics reject normality at $p < 0.01$). Thus, we use the sample median to characterize the average levels of BKLS measures, and a nonparametric (sign) test of changes in BKLS measures around earnings announcements.

Primary Results: Changes in BKLS Measures around Earnings Announcements

Our key test statistics are based on the sign of the changes in $\hat{\rho}$, $\hat{h}$, and $\hat{s}$ across the "before" and "after" forecast windows around (prior) earnings announcements. Our sign tests aggregate these signs across firm-years. As a result, mixtures of distributions or scale effects (see Brown and Rozeff 1978) are unlikely to materially affect our inferences. We first test for changes in BKLS correlation around earnings announcements. We then test for increases in the precision of the common and idiosyncratic information contained in individual analysts' forecasts of future annual earnings after the announcement of prior earnings. Finally, we compare the changes in the precision of individual analysts' common (Δh) and idiosyncratic (Δs) information around earnings announcements.

Recall that the BKLS correlation measure ρ directly captures the across-analyst correlation in forecast errors, which is the theoretical construct in the Fischer and Verrecchia (1998) model. The theoretical value of ρ ranges between zero and one, where zero indicates that the information contained in individual forecasts is entirely idiosyncratic, and one indicates that all forecasts are the same. This BKLS measure captures how much common vs. idiosyncratic information is *contained in the mean forecast* (see BKLS, p. 415). Under assumptions set out in Section V, we can further interpret ρ as the ratio of common-to-total information *possessed by individual analysts*: $\rho = h/(h + s)$. Table 2 and Figure 1 show that the median level of BKLS correlation ($\hat{\rho}$) starts out at 0.92 before the prior annual earnings announcement (PY_B), close to its limit of 1.0, but drops to 0.60 after the third interim earnings announcement ($Q3_A$). The decrease in BKLS correlation is significant around each of the four prior earnings announcements, ($p < 0.01$, two-tailed sign test). In other words, the commonality of information contained in analysts' forecasts declines throughout the year leading up to the announcement of annual earnings.[12]

Even though the commonality of information, as measured by BKLS correlation, ρ, declines as the year progresses, Table 2 reports that the precision of *both* common *and* idiosyncratic information increase over the year leading up to annual earnings announcements. These increases in both $\hat{h}$ and $\hat{s}$ are significant around the release of the four prior earnings announcements ($p < 0.01$, two-tailed sign test). The next subsection reports the results of tests that show that the decrease in $\hat{\rho}$ and increases in both $\hat{h}$ and $\hat{s}$ are larger around earnings announcements than in non-announcement periods.

The rightmost column of Table 2 reports the results of a statistical test comparing the magnitudes of $\Delta\hat{h}$ and $\Delta\hat{s}$ around earnings announcements. These results indicate that the increase in the precision of common information, $\Delta\hat{h}$, typically exceeds the increase in the precision of idiosyncratic information, $\Delta\hat{s}$, up to and including the second quarter earnings announcement.[13] This finding suggests that earnings announcements typically cause individual analysts' forecasts to contain more new common information than new idiosyncratic information. However, the *percentage* increases in the precision of individual analysts' idiosyncratic information after earnings announcements are larger than the percentage increases in the precision of analysts' common information, primarily because analysts start out the year with little or no idiosyncratic information about long-term (i.e., more than one year ahead) earnings prospects. As a result, the proportion of individual analysts' total information that is common, $\hat{h}/(\hat{h} + \hat{s})$, declines around announcements of prior period earnings.

Earnings Announcement Periods vs. Non-Announcement Periods

Because the BKLS correlation, ρ, can be interpreted as the percentage of information that is common to all analysts, ρ provides a way of summarizing our results regarding changes in both analysts' common and idiosyncratic information. The evidence reported in Table 2 and Figure 1 is consistent with accounting earnings announcements triggering analysts to acquire or develop idiosyncratic information about upcoming annual earnings. In this section we present evidence that our reported decline in BKLS correlation, and increases in the estimated precisions of common and idiosyncratic information, over the entire forecast horizon PY_B through $Q3_A$ are primarily attributable to the effects of earnings announcements.

To better isolate the effects of earnings announcements, we restrict this analysis to forecasts issued in shorter windows around earnings announcement dates (days -29 to -1 and 0 to $+9$). This restriction yields a subsample of 103 firm-years matched over all eight forecast windows. Statistical tests of changes in $\hat{\rho}$ around earnings announcements for this

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[12] Another way to articulate our results is that $\widehat{SE}$ decreases faster than $\hat{D}$ around prior earnings announcements.

[13] Although the results from the sign test indicate an insignificant difference between $\Delta\hat{h}$ and $\Delta\hat{s}$ for the third quarter earnings announcement, this particular result is test and sample specific. Using a Wilcoxon signed ranks test, we find that $\Delta\hat{h}$ significantly exceeds $\Delta\hat{s}$ for all four prior earnings announcements studied ($p < 0.01$, two-tailed). In addition, using a larger sample of all firm-years with data available for the third quarter earnings announcement (a sample of 4,068 firm-years), we find that $\Delta\hat{h}$ exceeds $\Delta\hat{s}$ using both a sign test and a Wilcoxon signed ranks test ($p < 0.01$, two-tailed, for both).

TABLE 2
BKLS Correlation ($\hat{\rho}$) and the Precision of Common ($\hat{h}$) and Idiosyncratic ($\hat{s}$) Information Contained in Individual Analysts' Forecasts of Annual Earnings before and after Prior Earnings Announcements
(n = 990)[a]

Forecast Windows[b]	*Median* $\hat{\rho}$	*Test of Change in* $\hat{\rho}$ *around Earnings Announcement (sign test p-value)*[c]	*Median* $\hat{h}$	*Test of Change in* $\hat{h}$ *around Earnings Announcement (sign test p-value)*[c]	*Median* $\hat{s}$	*Test of Change in* $\hat{s}$ *around Earnings Announcement (sign test p-value)*[c]	*Comparison of Change in* $\hat{h}$ *and Change in* $\hat{s}$ *around Earnings Announcement (sign test p-value)*[d]
PY_B	0.92		2.75		0.39		
PY_A	0.91	<0.01	3.49	<0.01	0.65	<0.01	<0.01[e]
$Q1_B$	0.89		4.54		0.94		
$Q1_A$	0.85	<0.01	5.78	<0.01	1.87	<0.01	<0.01[e]
$Q2_B$	0.83		6.79		2.69		
$Q2_A$	0.78	<0.01	10.13	<0.01	6.54	<0.01	<0.01[e]
$Q3_B$	0.73		13.74		10.77		
$Q3_A$	0.60	<0.01	20.63	<0.01	35.04	<0.01	0.07[f]

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[a] Sample of 990 firm-years matched across all eight forecast windows.

[b] Forecast windows before earnings announcements (subscripted B) run from day −45 through −1, where day 0 is the Compustat earnings announcement date. Forecast windows after earnings announcements (subscripted A) run from day 0 through +29. The earnings announcements include the prior-year annual earnings announcement (denoted PY) and the announcements of first, second, and third quarter earnings for the current year (denoted Q1, Q2, and Q3, respectively). For each announcement, our sample requires the selected individual forecasts of upcoming annual earnings made in the 45-day forecast window before an announcement to be updated in the 30-day forecast window after the announcement (must be a minimum of 2 such forecasts). We use these individual forecasts to measure $\hat{h}$, $\hat{s}$, and $\hat{\rho}$ before and after the prior earnings announcements.

[c] Two-tailed sign test of the null hypotheses that $\Delta\hat{\rho} = 0$, $\Delta\hat{h} = 0$, and $\Delta\hat{s} = 0$. A Wilcoxon signed ranks test produces similar results.

[d] Two-tailed sign test. We base tests on the statistic: $\mathrm{Diff}_{\hat{h}-\Delta\hat{s}} = \Delta\hat{h} - \Delta\hat{s}$, and we use a sign test to test the hypotheses that $\mathrm{Diff}_{\Delta\hat{h}-\Delta\hat{s}} = 0$.

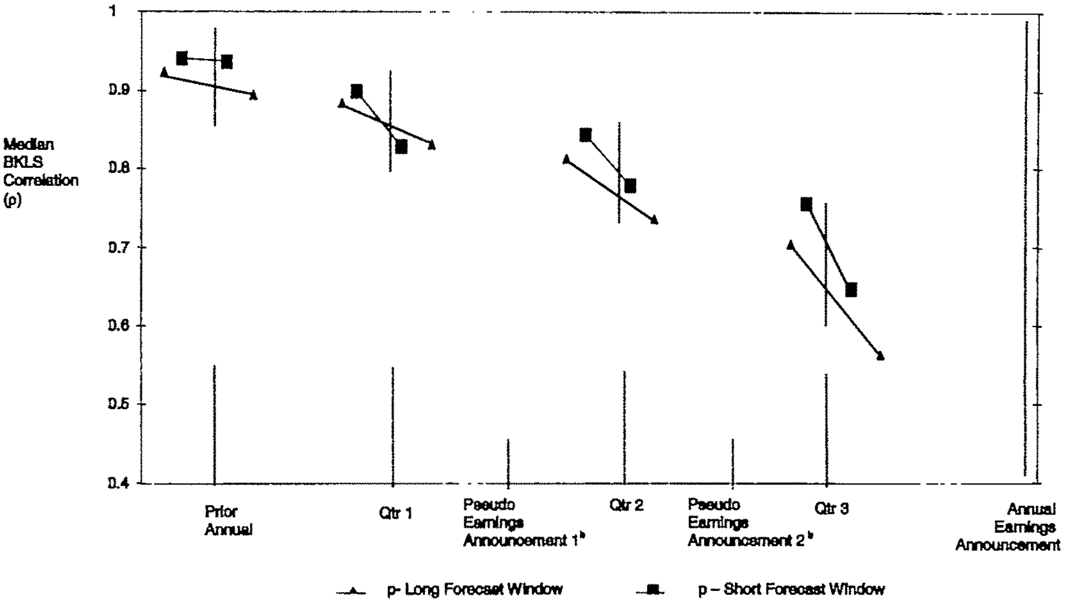
[e] Result indicates that $\Delta\hat{h} > \Delta\hat{s}$. A Wilcoxon signed ranks test produces similar results.

[f] Result indicates that $\Delta\hat{h} < \Delta\hat{s}$. This particular result, however, is test- and sample-specific. Using a Wilcoxon signed ranks test we find that $\Delta\hat{h}$ exceeds $\Delta\hat{s}$ ($p < 0.01$). In addition, using a larger sample of all available firm-years for the third quarter earnings announcement (a sample of 4,068 firm-years), we find that $\Delta\hat{h}$ exceeds $\Delta\hat{s}$ using both a sign test and a Wilcoxon signed ranks sign test ($p < 0.01$ two-tailed for both).

FIGURE 1

BKLS Correlation (i.e., the Across-Analyst Correlation in Earnings Forecast Errors), ρ, for Forecasts of Annual Earnings before and after Prior Earnings Announcements

Primary (Long Forecast Window) Matched Sample (n = 990 firm-years) and
Short Forecast Window Matched Samples (n = 103 firm-years)[a]



[a] The primary (long forecast window) sample consists of individual analysts' forecasts of annual earnings made within a 45-day forecast window before a prior earnings announcement that the same analysts then update within 30 days after the prior earnings announcement. A minimum of two analysts must forecast before and update after each of the four prior earnings announcements. The sample consists of 990 firm-years matched across all four earnings announcements. The short forecast window sample consists of individual forecasts made within a 30-day forecast window before an earnings announcement that the same analysts then update within 10 days after the earnings announcement. The sample consists of 103 firm-years matched across all four earnings announcements.

[b] Pseudo-earnings-announcements show the position of non-earnings-announcement dates selected in order to compare the changes in BKLS correlation around earnings announcements and non-announcements. Because of more limited data availability for these pseudo-announcements, the samples of firm-years available for these pseudo-announcements are not comparable with the earnings announcement period samples shown here.

short-window sample yield results consistent with those for our primary sample. Specifically, $\Delta\hat{\rho}$ is significantly negative around all four earnings announcements. As Figure 1 depicts, the decreases in $\hat{\rho}$ around earnings announcements in this short-window sample are of a similar magnitude to those documented for our long-window primary sample. This short-window evidence that the percentage of information that is common to all analysts declines around earnings announcements supports our inferences that earnings announcements trigger analysts to acquire or develop significant idiosyncratic information.[14]

To test whether earnings announcements appear to trigger unusual changes in analysts' information, we compare the changes in the percentage of information that is common to all analysts, $\hat{\rho}$, across earnings announcements and pseudo-announcements. Specifically, we replicate the short-window samples (based on forecasts issued during days -29 to -1 and updated during days 0 to $+9$) for two pseudo-announcement dates in non-announcement periods: 45 days after the actual announcement dates for first and second quarterly earnings announcements (see Figure 1).[15] Comparing the percentage change in $\hat{\rho}$ across all available firm-years for the three actual interim earnings announcements and the two pseudo-earnings announcements, we find the percentage *decrease* in $\hat{\rho}$ is larger around actual earnings announcements than for the pseudo-announcements ($p < 0.01$ for a two-tailed median scores test). This indicates that earnings announcements are associated with unusual shifts in the weighting of analysts' forecasts *toward* idiosyncratic information. We also find that the percentage increases in both $\hat{h}$ and $\hat{s}$ are larger around actual earnings announcements than around the pseudo-announcements ($p < 0.01$, two-tailed median scores test).

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[14] A less convincing alternative explanation is that analysts acquire significant idiosyncratic information during non-announcement periods and wait until announcement periods to release this information in their forecasts.

Forecast Revision Frequency and Changes in Analysts' Information around Earnings Announcements

Stickel (1989) documents that an abnormal flurry of annual earnings forecast revisions occurs immediately after interim earnings announcements. Also, Lang and Lundholm (1996, 471 and 490) find that firms' disclosure quality is (1) positively associated with the number of forecasts analysts issue, but (2) negatively associated with the magnitude of differences in forecasts (dispersion) across analysts. Lang and Lundholm (1996) suggest two possible explanations for these results. One explanation is that public disclosures such as earnings announcements reduce analysts' forecasting costs and in turn increase the number of forecasts that analysts supply. However, this decrease in cost and increase in supply would have to be large enough to counteract the decreases in investors' demand for forecasts arising because individual analysts' forecasts appear more similar and are more heavily weighted with the common information that investors can possibly find in public disclosures. The second explanation is that investor demand for analyst reports actually increases with the level of a firm's disclosure, because analysts serve as *information processors*, who transform the information in firms' financial reports for investors in a way that complements these reports.

Our evidence is consistent with this second explanation. Specifically, the evidence in Table 2 and Figure 1 suggests that after earnings announcements, BKLS correlation decreases and the amount of idiosyncratic information contained in individual forecasts increases. Thus, the increase in the number of individual analysts' forecast revisions around earnings announcements may result in part from investor demand for the idiosyncratic information contained in multiple individual analysts' forecast revisions. We further probe this conjecture by examining the association between the number (and percentage) of forecast revisions around an earnings announcement and the extent to which individual analysts' forecasts contain new idiosyncratic information after these announcements.

Unlike our prior analysis, which focused on how characteristics of information contained in analysts' forecasts change throughout *the entire year* leading up to the actual earnings announcement, this analysis focuses on the contemporaneous relation between the change in the properties of individual analysts' information and the incidence of forecast revisions around specific earnings announcements. Thus, for this analysis we relax our sample selection criteria to enhance the generalizability of our results. For example, we include all first quarter earnings announcements for which at least two analysts issue forecasts of annual earnings within 45 days before the first quarter earnings announcement, and then update this forecast in the 30-day period after the announcement. This yields a sample of 2,591 firm-years for the first-quarter earnings announcement. We construct the samples of firm-years for the prior annual, second quarter, and third quarter earnings announcements in the same manner.

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[15] Matching across the short forecast windows before and after each actual quarterly announcement, we obtain samples of 1,215, 1,258, and 1,754 firm-years for the first, second, and third quarter earnings announcements, respectively. The same sample selection procedures, however, yield samples of only 277 and 270 firm-years for the pseudo-announcements 45 days after the first and second quarter announcements, respectively. The larger samples for the actual earnings announcements further supporting our conclusion that analysts engage in more information processing around actual earnings announcements than in non-earnings announcement periods.

Panel A of Table 3 indicates that the number of forecast revisions ($\hat{N}$) around an earnings announcement is negatively correlated with the change in BKLS correlation ($\Delta\hat{\rho}$). That is, earnings announcements associated with steeper drops in $\hat{\rho}$ (i.e., larger drops in the percentage of information that is common to all analysts) also stimulate more analysts to issue forecast revisions (significant at $p < 0.01$, two-tailed). Panel A of Table 3 also reports that increases in the precision of individual analysts' idiosyncratic information ($\Delta\hat{s}$) are positively associated with $\hat{N}$ (significant at $p < 0.01$, two-tailed).[16] In contrast, the number of forecast revisions is marginally *negatively* associated with the increase in the precision of common information ($\Delta\hat{h}$). Also, Panel B of Table 3 shows that the percentage of analysts following a firm who revise their forecasts around an earnings announcement is positively associated with the increase in the precision of individual analysts' idiosyncratic information ($\Delta\hat{s}$; significant at $p < 0.01$, two-tailed, for three of the four prior earnings announcements), but is not significantly related to the increase in the precision of analysts' common information ($\Delta\hat{h}$). In sum, we find that the number (and percentage) of analysts who revise forecasts of annual earnings around a prior earnings announcement is positively related to the amount of new idiosyncratic information contained in each individual analysts' forecasts made after the announcement.[17] These results support our inferences that analysts serve as interpreters of financial disclosures, and help explain (in terms of the informational role of analysts) Stickel's (1989) documentation of a flurry of forecast revisions immediately after earnings announcements.

BKLS Correlation and the Superiority of the Mean Forecast

When aggregating individual forecasts, the degree of commonality among these forecasts determines the *likelihood* that the mean forecast will be more accurate than any one of the individual forecasts.[18] The decrease in BKLS correlation after earnings announcements indicates that, after an earnings announcement, a higher percentage of the information impounded in the mean forecast is idiosyncratic information. This increases the likelihood that the mean forecast is more accurate than an individual forecast because the idiosyncratic information is not redundant to an investor who observes multiple forecasts.

We now develop a probit model to test our conjecture that mean forecasts are more likely to be superior to (more accurate than) individual forecasts after earnings announcements. Our tests include all available forecasts by the same individual analysts matched across (45-day pre-, and 30-day post-) forecast windows before and after any of the four

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[16] Our tests of a relation between $\hat{N}$ and both $\Delta\hat{h}$ and $\Delta\hat{s}$ are based on the assumption of minimal differential precision (or accuracy) across analysts. To the extent differential precision exists in our sample, it may overstate our finding and related inference about a relation between $\hat{N}$ and $\Delta\hat{s}$ (which concern effects at the individual analyst level). However, a similar caveat does not exist about our finding of a relation between $\hat{N}$ and $\Delta\hat{\rho}$ (which concern effects at the mean or aggregate forecast level).

[17] Multiple regression analysis of $\hat{N}$ on both $\Delta\hat{h}$ and $\Delta\hat{s}$ confirms that $\hat{N}$ is positively associated only with $\Delta\hat{s}$.

[18] O'Brien (1988, 72–74) makes a similar conjecture, but is unable to test it due to the presence of stale forecasts in her sample. Our sample selection criteria limit staleness in our data, and as footnote 19 reports, our inferences hold after explicitly incorporating a control for forecast recency.

TABLE 3

Spearman Correlations between the Level of Analyst Forecasting Activity and Changes in Information Contained in Individual Analysts' Forecasts of Annual Earnings before and after Prior Earnings Announcements (two-tailed p-values)

Panel A: Correlation between Number of Forecast Revisions ($\hat{N}_{jt}$) around a Prior Earnings Announcement and Changes in BKLS Correlation ($\hat{\rho}$) and the Precision of Common ($\hat{h}$) and Idiosyncratic ($\hat{s}$) Information Contained in Individual Analysts' Forecasts

Earnings Announcement		$\Delta\hat{\rho}_{jt}$	$\Delta\hat{h}_{jt}$	$\Delta\hat{s}_{jt}$	*Sample Size (firm-years)*
Prior Annual	$\hat{N}_{jt}$	−0.063 (<0.01)	−0.033 (0.06)	0.074 (<0.01)	2,591
1st Quarter	$\hat{N}_{jt}$	−0.067 (<0.01)	−0.022 (0.16)	0.073 (<0.01)	2,997
2nd Quarter	$\hat{N}_{jt}$	−0.067 (<0.01)	−0.045 (<0.01)	0.078 (<0.01)	3,273
3rd Quarter	$\hat{N}_{jt}$	−0.051 (<0.01)	−0.024 (0.09)	0.068 (<0.01)	4,086

Panel B: Correlation between the Percentage of Analysts Following a Firm Who Revise Their Forecasts ($\hat{N}_{jt}$/FOLLOWING$_{jt}$) around a Prior Earnings Announcement and Changes in BKLS Correlation ($\hat{\rho}$) and the Precision of Common ($\hat{h}$) and Idiosyncratic ($\hat{s}$) Information Contained in Individual Analysts' Forecasts

Earnings Announcement		$\Delta\hat{\rho}_{jt}$	$\Delta\hat{h}_{jt}$	$\Delta\hat{s}_{jt}$	*Sample Size (firm-years)*
Prior Annual	$\hat{N}_{jt}$/FOLLOWING$_{jt}$	−0.052 (<0.01)	−0.002 (0.92)	0.050 (<0.01)	2,591
1st Quarter	$\hat{N}_{jt}$/FOLLOWING$_{jt}$	−0.046 (0.02)	−0.023 (0.23)	0.040 (0.04)	2,997
2nd Quarter	$\hat{N}_{jt}$/FOLLOWING$_{jt}$	−0.047 (0.01)	−0.016 (0.39)	0.048 (0.01)	3,273
3rd Quarter	$\hat{N}_{jt}$/FOLLOWING$_{jt}$	−0.061 (<0.01)	−0.019 (0.32)	0.057 (<0.01)	4,086

$\hat{N}_{jt}$ = the number of active revisions of forecasts of upcoming annual earnings around the prior earnings announcement for firm j in year t. Measured as the number of analysts who issue a forecast of year t annual earnings in the 45-day period prior to the earnings announcement date and then revise this forecast in the 30-day period after the earnings announcement;

$\Delta\hat{\rho}_{jt}$ = the change in the BKLS correlation measure for forecasts of annual earnings made before and after prior earnings announcement for firm j in year t. Based on a sample of individual analysts matched before and after the prior earnings announcement;

$\Delta\hat{h}_{jt}$ = the change in the precision of the common information contained in individual analysts' forecasts of annual earnings before and after prior earnings announcement for firm j in year t. Based on a sample of individual analysts matched before and after the prior earnings announcement;

$\Delta\hat{s}_{jt}$ = the change in the precision of the idiosyncratic information contained in individual analysts' forecasts of annual earnings before and after prior earnings announcement for firm j in year t. Based on a sample of individual analysts matched before and after the prior earnings announcement; and

FOLLOWING$_{jt}$ = the total number of analysts following firm j in year t (any analyst who issued at least one forecast for firm j during year t).

prior earnings announcements. For each firm-year observation matched both before and after an earnings announcement, we compare the accuracy (absolute error) of the mean forecast (E^M) to the accuracy of the most recent individual forecast (E^I)—i.e., the last individual forecast in the forecast window. The mean and most recent individual forecasts are based on the same set of individual analysts who forecast before and also update after earnings announcements. Our dependent variable is an indicator variable that equals 1 when the mean forecast is more accurate (has smaller absolute error) than the most recent individual forecast (i.e., if $E^M < E^I$), and 0 otherwise. Our focal explanatory variable, AFTER, equals 1 when the forecast window is after the earnings announcement, and 0 when the window is before the announcement. Therefore, AFTER captures *the change in* the likelihood that the mean forecast is more accurate than the most recent individual forecast after an earnings announcement. Because our samples include multiple observations for the same firms in different years, we also include firm (FD) and year (YD) dummy variables to control for dependence in our data.

We use the following fixed-effects probit model to evaluate the *change in* the relative frequency with which the mean forecast is more accurate than the most recent individual forecast after earnings announcements:

$$\text{Prob}(E^M_{jtw} < E^I_{jtw}) = \alpha_0 + \alpha_1 \text{AFTER}_w + \sum_{j=2}^{J} \delta_j \text{FD}_j + \sum_{t=2}^{T} \gamma_t \text{YD}_t + \varepsilon_{jtw} \qquad (8)$$

where:

E^M_{jtw} = the absolute error in the mean forecast for firm j in year t, during forecast window w; and E^I_{jtw} is the absolute error in the most recent individual forecast for firm j in year t, during forecast window w. A dummy variable equals 1 when $E^M_{jtw} < E^I_{jtw}$, and 0 otherwise. Thus, we are estimating the probability that the mean forecast is more accurate than the most recent individual forecast;
AFTER_w = a dummy variable equal to 1 for the forecast window after an earnings announcement (e.g., PA_A) and 0 for the forecast window before an earnings announcement (e.g., PA_B);
FD_j = a set of firm dummy variables equal to 1 for firm j and 0 for all other firms; and
YD_t = a set of year dummy variables equal to 1 for year t and 0 for all other years.

Table 4 shows that, for the first, second, and third quarterly earnings announcements, the relative frequency with which the mean forecast is more accurate than the most recent individual forecast is higher after an earnings announcement than before ($p \leq 0.01$ one-tailed Chi-squared). The results, however, are not significant for the prior annual earnings announcement, perhaps because BKLS correlation decreases the least around prior annual earnings announcements (see Figure 1).[19]

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[19] Our inferences are unaffected when we repeat this analysis after controlling for: (1) the difference in forecast age between the most recent individual forecast and the average age of the forecasts composing the mean forecasts, and (2) the number of forecasts aggregated ($\hat{N}$).

TABLE 4
The Superiority of Mean Forecasts Relative to the Most Recent Individual Forecasts before and after Earnings Announcements[a]

$$Prob(E^{M}_{jtw} < E^{I}_{jtw}) = \alpha_0 + \alpha_1 AFTER_w + \sum_{j=2}^{J} \delta_j FD_j + \sum_{t=2}^{T} \gamma_t YD_t + \varepsilon_{jtw}$$

Earnings Announcement	*AFTER ($\hat{\alpha}_1$) One-tailed Chi-Squared (p-value)*	*Number of Firms*	*Number of Years*	*Number of Firm-Year Observations*[b]	*Goodness of Fit: Likelihood Ratio Chi-Square (two-tailed p-value)*
Prior Annual	0.054 (0.07)	650	12	5,182	6,325 (<0.01)
1st Quarter	0.093 (<0.01)	736	12	5,994	7,445 (<0.01)
2nd Quarter	0.127 (<0.01)	1,300	12	6,546	7,920 (<0.01)
3rd Quarter	0.109 (<0.01)	1,300	12	8,172	9,911 (<0.01)

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[a] The dependent variable equals 1 if the mean forecast is more accurate than the most recent individual forecast, and 0 otherwise.

[b] Each firm-year appears in the sample both before and after the earnings announcement. As a result, the number of observations is twice the number of firm-years.

E^{M}_{jtw} = the absolute error in the mean forecast for firm j in year t, during forecast window w;

E^{I}_{jtw} = the absolute error in the most recent individual forecast for firm j in year t, during forecast window w.

A dummy variable equals 1 when $E^{M}_{jtw} < E^{I}_{jtw}$ and 0 otherwise. Sample is based upon individual forecasts made in the 45-day window before the announcement and updated by the same individual analyst in the 30-day window after the announcement (must be a minimum of two such forecasts). We calculate the mean forecast using just these forecasts, and we select the most recent forecast only from these individual forecasts.

$AFTER_w$ = a dummy variable equal to 1 for the forecast window (w) after the earnings announcement (PA_A, $Q1_A$, $Q2_A$, or $Q3_A$) and 0 for the forecast window before the earnings announcement (PA_B, $Q1_B$, $Q2_B$, or $Q3_B$);

FD_j = a set of firm dummy variables equal to 1 for firm j, and 0 for all other firms; and

YD_t = a set of year dummy variables equal to 1 for year t, and 0 for all other years.

V. ROBUSTNESS: TESTING SIMPLIFYING ASSUMPTIONS OF BKLS

In this section we address concerns regarding the validity of our conclusions, including those related to the simplifying assumptions of the BKLS model. BKLS's main result concerning how to measure the across-analyst correlation in forecast errors can be demonstrated without reference to their measures of h and s, and most of the assumptions that accompany these measures. Only two assumptions are required to interpret the BKLS correlation measure, ρ, as a measure of the percentage of common information *contained in the mean forecast*: (1) analysts' forecasts are unbiased and that some of the apparent commonality in the forecasts is not meaningless and subtracted out by investors, and (2) individual earnings forecast errors reflect the properties of information analysts want to convey and these errors do not reflect managers efforts to reduce unwanted earnings surprises. After examining the robustness of our inferences to these two assumptions, and discussing the possibility of herding among analysts (Trueman 1994; Welch 2000) within our sample, we then consider three additional assumptions necessary to interpret ρ as the ratio of common-to-total information *possessed* by individual analysts, and h and s as the precisions of the common and idiosyncratic information *possessed* by individual analysts.

Forecast Bias

BKLS assume that forecasts are unbiased. Prior research suggests that analysts systematically issue optimistic forecasts for annual earnings (e.g., Francis and Philbrick 1993), but more recent evidence in Brown (2001) and Matsumoto (2002) suggest that analysts' apparent optimistic bias is diminishing, that analysts appear to be optimistic for only some periods, and that much of analysts' optimism arises from firms reporting losses (Brown 2001).[20] A systematic bias in forecast errors would inflate SE relative to D, thus upwardly biasing the BKLS correlation measure. We examine the possible effects of this optimistic bias in two ways. First, repeating our analysis for our primary sample of 990 firm-years, but excluding firm-years with reported losses, leads to identical inferences. Second, our inferences are unchanged using "debiased" forecasts (adjusting each individual forecast in our primary sample of 990 firm-years for the average level of optimism in that forecast window across all firms and years) to compute a debiased measure of the squared error in the mean forecast (SE):

$$\hat{SE}^{*}_{jtw} = \left((A_{jt} - \bar{F}_{jtw}) - \frac{1}{T}\frac{1}{J}\sum_{t=1}^{T}\sum_{j=1}^{J}(A_{jt} - \bar{F}_{jtw})\right)^{2}, \tag{9}$$

where $\hat{SE}^{*}_{jtw}$ is the observed squared error in the mean forecast for firm j in year t and forecast window w, adjusted for the average level of optimism for all forecasts for that forecast window; A_{jt} is the actual earnings for firm j in year t; and $\bar{F}_{jtw}$ is the mean forecast for firm j in year t for forecast window w.[21] We also conduct an analysis adjusting each individual forecast in our primary 990 firm-year sample for the average level of bias in that forecast window in that year. This analysis not only controls for average bias, but also adjusts for the average effect of any time-period dependence in the data (Keane and Runkle 1998). Our inferences hold after each type of debiasing.

Management of Analysts' Expectations or Earnings

Recent research suggests that firms manage both analysts' expectations and earnings to "meet or beat" analyst forecasts (e.g., Matsumoto 2002). Management of analysts' expectations by making more pre-announcement disclosures to analysts does not violate any of the assumptions underlying the BKLS model. However, to the extent that managers manipulate earnings to meet or beat analysts' expectations, the resulting actual earnings realization is dependent on the mean forecast, and the mean forecast error is biased toward zero, which in turn downwardly biases the BKLS correlation measure.

Earnings management to "meet or beat" analysts' expectations is unlikely to drive our inferences, however. First, to the extent that analysts anticipate earnings management, they account for it as part of their information sets. Second, prior research (e.g., Richardson et al. 2002) reports that forecast management or earnings management to meet analysts' expectations are more common after about 1992. Our results are robust when we split our primary sample of 990 firm-years into pre- and post-1992 subsamples, and conduct our analyses for both subsamples. Significantly, the decrease in BKLS correlation ($\hat{\rho}$) across the full year leading up to the earnings announcement is almost identical in both subperiods: median $\hat{\rho}$ falls from 0.93 to 0.62 in the pre-1992 subsample, and from 0.92 to 0.60 in the post-1992 subsample.

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[20] During our sample period (1986–1997), the incentives to bias forecasts upward to win investment banking services may have been higher for analysts following firms in high-tech industries. As a robustness check, we split our sample into high-tech and non-high-tech firm subsamples, using the same SIC code-based classification for high-technology industries as Barron et al. (2002). Our inferences hold for both subsamples.

[21] Consistent with prior evidence that optimistic bias declines as the earnings announcement date approaches (Richardson et al. 2002; Darrough and Russell 2002), the median percentage error in our individual forecasts decreases over the eight forecast windows from 7.12 percent (PY_B) to 0 percent ($Q3_A$).

Nevertheless, we conducted three sets of additional analyses, all of which reveal that our inferences continue to hold using subsamples of firm-years for which management has less incentive to manage earnings to meet or beat analysts' forecasts. For our first test, we undertook a similar analysis, but computing $\hat{\rho}$, $\hat{h}$, and $\hat{s}$ based on analysts' forecasts of annual cash flows, rather than forecasts of annual earnings. Consistent with our results for forecasts of annual earnings, we find that BKLS correlation for annual cash flow forecasts also decreases throughout the year prior to the announcement date, a result that cannot be attributable to accruals-based earnings management. Our second test uses a subsample of firm-years for which it is unlikely that earnings are managed upward to meet forecasts because, for the entire year before the earnings announcement date, these forecasts are considerably below the actual earnings the firm eventually announces. From our primary sample of 990 firm-years, we select a subsample in which actual earnings exceeds the mean forecast by at least 20 cents in each of the eight forecast windows (PA_B through $Q3_A$, inclusive). Forty-eight firm-years meet this criterion. For this subsample the median BKLS correlation drops from 0.96 to 0.84 over the period PA_B to $Q3_A$ and decreases in BKLS correlation after earnings announcements are statistically significant. The drop in $\hat{\rho}$ is significant at the 0.05 level for the first and third quarter earnings announcements and at the 0.10 level for the second quarterly earnings announcement (one-tailed sign test).[22]

For our third test, we replicate our analyses excluding small positive earnings surprises from our primary sample, as these are more likely to reflect earnings management (e.g., Burgstahler and Eames 2001; Beatty et al. 2002). Deleting firm-years for which the earnings surprise (measured as the difference between actual earnings and the median forecast in the 45-day window immediately prior to the annual earnings announcement date) is between 0 and 5 cents, does not affect our inferences.

Analyst Herding

Prior research also suggests that analysts may engage in herding (Trueman 1994; Welch 2000), thereby overweighting common information, which would in turn upward bias the BKLS correlation. Our research design mitigates the possible bias induced by herding, as we select only analysts who actively update after earnings announcements, and such analysts are more likely to base their forecasts on their own information rather than herd (Barron and Stuerke 1998). Moreover, our primary result, that BKLS correlation *decreases* over the year prior to annual earnings announcements, is inconsistent with widespread herding among the analysts in our sample. In further contrast to what we would expect under herding, evidence reported in Table 3 suggests that our individual analysts' forecasts contain relatively more idiosyncratic information when more forecasts by other analysts are observable.

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[22] This sample selection criterion selects firm-years for which the squared error in the mean forecast, SE, is high throughout the year leading up to the actual earnings announcement, thus self-selecting firm-years for which the common error is relatively high throughout the year leading up to an earnings announcement. It follows that this subsample biases toward measuring high levels of BKLS correlation and *against* finding decreases in this correlation.

We now examine the robustness of our inferences to the three additional assumptions necessary to interpret ρ as the ratio of common-to-total information *possessed* by individual analysts, and h and s as the precisions of the common and idiosyncratic information *possessed* by individual analysts.

Information is Either Common or Idiosyncratic

To make their model tractable, BKLS assume analysts' information can be represented as either common to all analysts or idiosyncratic (uniquely private) to individual analysts. To test the validity of our inferences, we developed a subsample of firm-years with only two analysts matched before and after each earnings announcement. Because there are only two analysts, any non-common information reflected in their forecast dispersion must be completely idiosyncratic to only one of the two analysts, by construction. We start with our sample of 990 firm-years matched across all eight forecast windows. For firm-years with more than two analysts forecasting both before and after an earnings announcement, we randomly select two analysts from those available. Repeating our analysis using this alternative sample yields identical inferences.

Normally Distributed Forecast Errors

Another assumption underlying the BKLS precision measures (h and s) is that the errors in individual analysts' common and idiosyncratic information are normally distributed for any given firm (or firm-year). This assumption, combined with the assumption that analysts are rational Bayesians trying to forecast accurately, suggests that realized errors in individual analysts' forecasts will be, on average, self-fulfilling representations of analysts' beliefs about their underlying information, and thus normally distributed. At first, these assumptions may seem inconsistent with the distribution of individual forecast errors across our entire sample of firm-years, which is positively skewed with fat tails (excess kurtosis). However, these departures from normality are largely attributable to the mixing of forecast error distributions across firms that have different scale parameters.[23] The distribution of forecast errors within subsamples with more homogeneous magnitudes of reported earnings (actual earnings per share [EPS]) ranging from \$1.01 to \$1.05, \$1.06 to \$1.10, and so forth) are much closer to normality. For example, for our primary 990 firm-year sample in the $Q3_A$ forecast period, the excess kurtosis statistic for the cross-sectional distribution of all individual forecasts used is 23.22. The median excess kurtosis statistic for the subsamples based on 10-cent ranges of actual EPS declines to 7.08. Using 5-cent ranges of actual EPS, the median excess kurtosis declines further, to 2.72. Thus, the excess kurtosis appears largely attributable to mixtures of different distributions in the full sample of individual forecasts pooled across firm-years.

We conclude that the assumption of normally distributed forecast errors is a reasonable first approximation *for any given firm* (*or firm-year*). Our sample forecast errors are, however, influenced by differences in scale across firms (firm-years). We base our key test statistics on a sign test of differences across two points in time for the same firm-year.[24]

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[23] The mixture of distributions hypothesis (MDH) makes a similar argument concerning the skewness and kurtosis in empirical distributions of stock returns (e.g., Kon 1984).

[24] Nevertheless, standard t-tests yield results consistent with those reported in this paper.

We then aggregate these signs across firm-years. As a result, such mixtures of distributional/scale effects are unlikely to materially affect our inferences.

Homogeneous Precision (of Individual Analysts' Information)

Finally, the BKLS model is most readily applicable when the *expected* (or *ex ante*) precision of idiosyncratic information (s_i) is the same across analysts. We next argue that the assumption that $s_i = s$ for all analysts i is a reasonable first approximation, and outline how our research design mitigates the possibility of differential precision within our sample. We then develop a modified BKLS precision measure that adjusts for possible departures from this assumption, and summarize the results of robustness tests using this modified measure for a subsample of our data.

Early research failed to find significant differences in forecast accuracy across analysts (e.g., Butler and Lang 1991). More recent studies that have found differences in accuracy across analysts typically find that any differences are small and short-lived (e.g., Stickel 1992; Clement 1999; Jacob et al. 1999). For example, Stickel (1992) reports that All-American analysts supply annual earnings forecasts that are only 2.82 cents more accurate than other analysts. Furthermore, the competitive market in which analysts with less accurate forecasts are more likely to lose their jobs (see Mikhail et al. 1999) also minimizes differences in accuracy across analysts.

Our study design selects only those analysts who issue revisions of forecasts shortly after earnings announcements. These must be revisions of forecasts the analysts made shortly before the earnings announcement date. As such, our sample selection criteria target only frequently updating analysts. Such analysts are likely to be actively following these firms, and are unlikely to be informationally disadvantaged *vis-à-vis* other analysts (see Jacob et al. 1999). Thus, there are likely to be smaller differences in precision among the subset of analysts included in our analyses.

In order to use BKLS to measure individual analysts' common (h) and idiosyncratic (s) information, it needs to be true that either the s_i's are equal across analysts *or* N is large enough to average out idiosyncratic errors from the mean forecast. When differences in s_i exist but N is large, the following approximations hold:

$$h \approx \frac{SE}{(SE + D)^2} = \alpha \frac{1}{(SE + D)} \tag{10}$$

$$\alpha \equiv \frac{1}{N} \sum_{i=1}^{N} \frac{h}{h + s_i} \approx \frac{SE}{(SE + D)} \tag{11}$$

Since α is the average influence of common information (h) on forecasts relative to total information ($h + s_i$), then $(1 - \alpha)$ measures the average relative influence of idiosyncratic information. The ratio of these two influences, $(1 - \alpha)/\alpha$, suggests an absolute measure of the precision of idiosyncratic information impounded in average forecasts, which we denote as s_α, where:

$$S_\alpha = \frac{(1 - \alpha)}{\alpha} h \approx \frac{D}{(D + SE)^2}. \tag{12}$$

Because the approximation in Equation (12) holds when N is large, we sample with N

> 8 analysts' forecasts both before and after an earnings announcement. Repeating our analyses using this modified measure of the precision of individual analysts' idiosyncratic information does not affect our inferences.

VI. CONCLUSIONS

We report evidence suggesting that the precisions of active analysts' common and idiosyncratic information (about upcoming annual earnings) increase over time, and especially after prior earnings announcements. Our results suggest that the relative increases in idiosyncratic information are larger, so that the percentage of total information that is common across all analysts decreases around earnings announcements. These results are consistent with the models of Kim and Verrecchia (1994, 1997), in which public financial disclosures trigger production of new idiosyncratic information by agents (such as financial analysts) possessing unique information-processing skills.

We also document that our estimate of the average amount of new idiosyncratic information contained in individual analysts' forecasts increases as more analysts revise their forecasts. Our results are consistent with analysts serving as interpreters of financial disclosures (Schipper 1991), which provide one possible explanation for Stickel's (1989) evidence that analysts issue a flurry of forecast revisions immediately after earnings announcements. Finally, consistent with the increase in the proportion of idiosyncratic information around earnings announcements, we document that the likelihood that the mean forecast is more accurate than the most recent individual forecast increases after earnings announcements. More broadly, our results cast doubt on the notion that earnings announcements *level* the informational playing field (Lev 1988), at least among our sample of actively updating-analysts following large firms.

Our conclusions are subject to a number of limitations. First, analysts' common and idiosyncratic information is of course unobservable. Relying on the BKLS model, we use observable characteristics of analysts' forecasts to develop proxies for these two unobservable dimensions of analysts' information structure. Section V first outlines the assumptions required to interpret the BKLS measures ρ as the percentage of common information *contained in the mean forecast*. It then outlines the additional assumptions needed to interpret ρ as the ratio of common-to-total information *possessed by individual analysts*, and to interpret h as the precision of individual analysts' common information, and s as the precision of individual analysts' idiosyncratic information. Finally, it shows that our inferences do not appear to be attributable to violations of these assumptions. Second, our analysis is based on properties of analysts' forecasts, which may not generalize to the properties of investors' information. Though the commonality of analysts' information likely reflects that of investors, the two may differ. For example, market prices may efficiently aggregate analysts' idiosyncratic information and investors may be able to extract aggregated idiosyncratic information from observable price changes. Third, our sample consists primarily of a subset of frequently updating analysts who issue forecasts for relatively large, heavily followed firms, thus potentially restricting the generality of our results.

The main contribution of this study is evidence—albeit indirect evidence—suggesting that analysts generate significant idiosyncratic information around earnings announcements. It is not clear whether earnings announcements create or trigger diverse information that would not be produced if not for earnings announcements, or whether the timing of private information-gathering activities happens to coincide with the timing of public earnings announcements, perhaps for analysts' convenience. Either way, our results are suggestive of significant interaction between public accounting disclosures and private information production.

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Using Budgets for Performance Evaluation: Effects of Resource Allocation and Horizontal Information Asymmetry on Budget Proposals, Budget Slack, and Performance

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ABSTRACT: Business executives and academics frequently criticize budget-based compensation plans as providing incentives for subordinates to build slack into proposed budgets. In this paper, we examine whether either of two practices—using budgets to allocate scarce resources, or providing information about co-workers—reduces budget slack and increases subordinate performance when organizations use budgets for performance evaluation. The results from our experiment show that using budgets for both resource allocation and performance evaluation not only eliminates budget slack, but also

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increases subordinates' effort and task performance. Additionally, we find that an internal reporting system that provides information about subordinates' budgets and performance to their co-workers mitigates budget slack when superiors do not use budgets as a basis for resource allocation. These results highlight the synergies between the planning (resource allocation) and control (performance evaluation) functions of managerial accounting practices such as budgeting. Our results also suggest that by designing the internal information system to reduce information asymmetry among subordinates, the firm can increase subordinates' incentives to provide more accurate budgets.

Keywords: *budgeting; performance evaluation; resource allocation; information; slack; performance.*

Data Availability: *Data gathered in this study are available from the authors upon request.*

I. INTRODUCTION

Traditional budget-based compensation plans provide economic incentives for subordinate managers to misrepresent (understate) their productivity and build slack into budgets (Horngren et al. 2000, 185; Jensen 2001). Slack creates a bias in budgets and can reduce firm profits due to costly planning errors and greater compensation or perquisite consumption for subordinate managers. To mitigate budget slack, researchers developed "truth-inducing" compensation schemes (e.g., Weitzman 1976) that explicitly reward subordinates for the truthful revelation of their private information.

Empirical research documents that truth-inducing compensation schemes do reduce subordinates' misrepresentations of productivity and budget slack (see, e.g., Young and Lewis 1995). However, firms rarely use truth-inducing schemes in practice (Waller 1994; Atkinson et al. 1997, 672). Apparently, either the benefits of using traditional budget-based compensation plans outweigh the costs of budget slack, or other mechanisms in the budgeting process counteract the negative effects of these traditional plans on subordinates' propensity to build slack into their budgets.

In this paper, we examine two such mechanisms, specifically whether the use of budgets to allocate scarce resources or the provision of information about co-workers reduces the slack subordinates build into their budgets when firms use budgets for performance evaluation. Both mechanisms naturally arise in budget settings with multiple subordinates, and both mechanisms introduce or emphasize competition among subordinates. We posit that such competition not only will reduce the rents subordinates earn as a result of budget slack, but will also increase subordinates' effort and task performance, in contrast to truth-inducing schemes (e.g., Chow et al. 1988).

When the firm uses budgets to allocate scarce resources, subordinates have incentives to *overstate* their productivity to acquire a greater share of fixed resources. We predict that such motivations will counterbalance subordinates' incentives to *understate* their productivity in budget-based evaluation plans. Additionally, we predict that subordinate (firm) performance will increase when superiors use the budget to allocate resources. This is because superiors can allocate more resources to more productive subordinates, and because subordinates will exert more effort to exceed their higher budget levels and to signal high productivity in an effort to attract higher resource allocations in the future.

We also examine the effects of an internal information system that provides information about subordinates' budget proposals, budget levels, and actual performance to their co-workers. We predict that reducing the horizontal information asymmetry among subordinates promotes social competition, thereby motivating subordinates to increase their budget proposals and performance. We posit, however, that the effects of resource allocation will dominate those of information about co-workers. We expect this to occur because resource allocation gives subordinates a strong economic incentive for high budget proposals and performance, whereas providing information about co-workers simply increases the transparency of incentives and relations already existing in the budget process.

We tested our hypotheses by conducting a laboratory experiment in which 174 undergraduate business students, working in groups of 3, each acted as either a superior manager or as one of two subordinates. All subordinates worked for three periods under a traditional budget-based compensation plan that provided incentives for subordinates to insert slack into their budget proposals, and for superiors to minimize slack. We manipulated both resource allocation and horizontal information asymmetry between subjects at two levels. In the no-resource allocation condition, each subordinate received a fixed amount of compensated production time (salary), whereas in the resource allocation condition the superior allocated production time between the two subordinates. In the low (high) information asymmetry condition, each subordinate was (was not) aware of the other subordinate's budget proposals and counteroffers, the superior's budget counteroffers to the other subordinate, and the other subordinate's final budget level and work-session performance.

The results indicate that allowing the superior to allocate productive resources among subordinates significantly increased subordinates' budget proposals, decreased budget slack, and increased subordinate task performance. Specifically, when the superior used budgets to allocate resources, subordinates did not have any slack in their final budgets, and subordinate performance increased by 25 percent. Additionally, when superiors did not use budgets to allocate resources, reducing the information asymmetry among subordinates led subordinates to submit higher budget proposals that reduced budget slack but, contrary to our expectations, did not lead to higher subordinate performance.

Our results suggest that using budgets to allocate resources as well as to evaluate performance provides complementary incentives that lead subordinates to reveal their private information more truthfully and increase their effort and task performance. The mutual discipline imposed by this dual use of budgets may help explain why firms often use a single budget for both planning and performance evaluation (Umapathy 1987, 27) and rarely use truth-inducing compensation schemes (Waller 1994). Moreover, our findings highlight the synergies between the planning and control functions of managerial accounting practices such as budgeting. For example, our results suggest that using a single budget both to allocate resources and to evaluate performance may help mitigate the *ex post* inefficiencies created by slack and capital rationing described in analytical research (e.g., Antle and Eppen 1985). Our findings also have implications for the design of a firm's internal reporting system. Specifically, sharing information about co-workers can reduce budget slack when the firm uses budgets only for performance evaluation (and not for resource allocation). However, such information sharing may not provide incremental benefits when the firm uses budgets to allocate resources as well as to evaluate performance.

The remainder of this paper is organized into four sections. Section II develops the hypotheses, and Section III explains the methods employed to test these hypotheses. Section IV presents the results, and Section V provides a summary and discussion of the results.

II. BACKGROUND AND HYPOTHESES

We consider a participative budgeting scenario in which negotiation between a subordinate and a superior determines the budget (Fisher et al. 2000). The subordinate begins the process by submitting a budget proposal, and the superior sets the final budget if the two parties do not reach agreement (Umapathy 1987, 24; Anthony and Govindarajan 2001, 370–371). The final budget serves as a benchmark for evaluating and compensating a subordinate's actual performance (Demski and Feltham 1978; Umapathy 1987, 36; Merchant 1998, 333).

Using Budgets for Performance Evaluation

Equation (1) illustrates a budget-based (gainsharing) contract commonly employed in corporate practice (Umapathy 1987, 36; Henderson 2000; Lawler 2000).[1] Subordinates' compensation includes a salary component (base pay) and a piece-rate (bonus) for each unit produced in excess of the budget. Superiors receive a percentage of the residual profit after payment to subordinates, as shown in Equation (2).

$$P_{subordinate} = \begin{cases} S & \text{if } X \leq B \\ S + A(X - B) & \text{if } X > B \end{cases} \tag{1}$$

$$P_{superior} = \begin{cases} R(DX - S) & \text{if } X \leq B \\ R(DX - [S + A(X - B)]) & \text{if } X > B \end{cases} \tag{2}$$

where:

P = total compensation (pay);
S = the subordinate's salary compensation (base pay);
A = the subordinate's compensation per unit of production over budget;
X = actual production;
B = budget;
R = the superior's percentage of firm profit; and
D = profit, exclusive of subordinate compensation, per unit produced.

Prior research has documented both benefits and costs associated with the budget-based contract shown in Equation (1). Specifically, relative to fixed-wage or pure profit-sharing (piece-rate) contracts, traditional budget-based contracts increase subordinate effort and performance (Bonner et al. 2000). This occurs because a traditional budget-based contract explicitly links pay to performance (Chow 1983) and delineates precise goals for subordinates (Locke and Latham 1990), both of which are motivational mechanisms (Bonner and Sprinkle 2002).

However, traditional budget-based contracts provide a strong incentive for subordinates to use their private information to create budget slack by understating their expected productivity in budget proposals (i.e., by attempting to set a low B). Budget slack represents the discrepancy between a subordinate's best estimate of performance based on his private information and the budgeted level of performance (Young 1985; Chow et al. 1988; Waller 1988). As Equations (1) and (2) indicate, budget slack increases subordinates' compensation and reduces superiors' compensation.

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[1] This contract not only has an explicit link between budget attainment and a subordinate's current compensation, but it also exemplifies contracts with an implicit link between budget attainment and a subordinate's future compensation. This implicit link arises because the superior's subjective evaluation of a subordinate's current performance relative to budget can influence the subordinate's future compensation (e.g., salary and promotions).

Empirical research indicates that subordinates insert significant amounts of slack under traditional budget-based compensation schemes (Young 1985; Chow et al. 1988), and that single-person truth-inducing budget-based compensation schemes (e.g., Weitzman 1976) reduce slack, but typically do not eliminate it (Chow et al. 1988; Waller 1988). Further, subordinates' performance under truth-inducing schemes is comparable to that under traditional budget-based schemes (Chow et al. 1988; Chow et al. 1991). Perhaps because of the limited benefits, firms rarely use truth-inducing compensation schemes in practice (Waller 1994).

We investigate whether two other practices reduce subordinates' productivity understatements and improve subordinates' performance under traditional budget-based schemes: using budgets to allocate resources and providing information about subordinates' budgets and performance to their co-workers. Both practices arise naturally in budget settings involving multiple subordinates. We expect both practices to increase competition among subordinates, which should in turn reduce their productivity understatements and enhance their subsequent performance.

Mitigating Effects of Budget-Based Resource Allocation

Companies use budgets for planning and as a basis for allocating scarce human, physical, and financial resources (Anthony and Govindarajan 2001, 302–303). Traditional budget-based compensation schemes give subordinates economic incentives to acquire resources. First, the salary component of compensation (S in Equation [1]) generally increases with the amount of resources a subordinate controls (e.g., division size; Murphy 1985). Second, *ceteris paribus*, the piece-rate component in Equation (1) increases with the level of resources the subordinate can use to produce output. Indeed, prior research documents that when a superior uses budget proposals as a basis for resource allocation, subordinates overstate their true (exogenously endowed) productivity to receive more resources, and that these overstatements lead to an inefficient allocation of resources among subordinates (Waller and Bishop 1990; Chow et al. 1994, 1995; Chow et al. 2000).

Research has also found that multi-person and single-person truth-inducing budget-based compensation schemes (e.g., Groves 1973; Groves and Loeb 1979; Osband and Reichelstein 1985) are only partially effective in eliminating subordinates' overstatements in resource allocation settings, due to both the difficulty of setting appropriate parameter values for these schemes (Chow et al. 2000) and subordinates' inability to understand the schemes (Waller and Bishop 1990). Moreover, firms rarely use either single-person or multi-person truth-inducing compensation schemes.

We expect that under traditional budget-based schemes, subordinates' incentives to overstate their productivity when budgets are used for resource allocation purposes will mitigate their incentives to understate their productivity when budgets are used for performance evaluation purposes.[2] Accordingly, when superiors evaluate and compensate subordinates using traditional budget-based schemes, we posit that subordinates will propose higher budgets when the superior has the authority to allocate resources than when resources are fixed exogenously. The compensation scheme in Equation (2) leads the superior to prefer higher budget levels, so higher budget proposals by the subordinate should result in higher final budget levels, and thus in lower budget slack.[3]

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[2] Both the Groves and Osband/Reichelstein "truth-inducing" schemes incorporate budget-related parameters and recognize the opposing incentives created by using budgets for resource allocation and performance evaluation. Our investigation of the dual use of budgets for resource allocation and performance evaluation differs from prior empirical research in two important ways. First, we focus on the efficacy of budget-based schemes commonly observed in practice rather than on the more complex, and rarely used, truth-inducing schemes derived by analytic research. Second, prior studies exogenously endow participants with a productivity level (which mechanistically maps into performance), thereby focusing primarily on misrepresentations of that productivity. In contrast, we examine a setting in which participants' performance depends on both their endogenous ability and effort. This allows us to examine whether using budgets for both performance evaluation and resource allocation affects subordinates' effort and concomitant performance.

> **H1a:** Subordinates will submit higher initial budget proposals when the superior allocates productive resources to subordinates than when the superior does not allocate productive resources to subordinates.
>
> **H1b:** Slack in the final budget will be lower when the superior allocates productive resources to subordinates than when the superior does not allocate productive resources to subordinates.

Prior research provides little guidance as to whether the lower budget slack (i.e., higher budget levels) proposed in H1b will increase subordinate performance. Studies employing individual subordinate settings have not found that truth-inducing (Weitzman 1976) contracts lead to consistent differences in performance relative to traditional budget-based contracts, even though truth-inducing schemes result in higher budget levels (Chow et al. 1988, 1991). Additionally, multi-subordinate resource allocation studies have not allowed subordinates' ability and effort to endogenously determine their productivity.

We expect that subordinates will perform at a higher level when superiors allocate resources and when subordinates' performance is a function of their ability and effort. A superior can allocate more resources to the subordinate whose budget or past performance signals greater productivity (i.e., greater ability or effort). Additionally, subordinates likely will be motivated to increase their effort and produce more output to exceed the higher budget levels that exist when superiors allocate resources (as predicted by the lower budget slack in H1b). This motivation arises from subordinates' desire to increase both their current compensation and future resource allocations, which are likely to depend on past performance.

> **H1c:** Subordinate performance will be higher when the superior allocates productive resources among subordinates than when the superior does not allocate productive resources among subordinates.

Notwithstanding our predictions in H1a–H1c, there are several reasons why resource allocation may not increase subordinate budget proposals, reduce budget slack, and increase subordinate performance. For example, psychological factors such as equity concerns (e.g., Luft 1997; Luft and Libby 1997; Kachelmeier and Towry 2002) might diminish the effectiveness of resource allocation in mitigating slack and improving performance. If superiors allocate resources based on differential productivity, then subordinates allocated fewer resources may feel unfairly treated and reciprocate with lower budget proposals and performance. Analogously, if superiors under-reward (as perceived by subordinates) higher productivity, then higher-performing subordinates may feel unfairly treated and reduce their budget proposals and effort. Moreover, even if subordinates perceive the distribution of resources to be fair, they may view their (predicted) higher budgets under resource allocation to be unfair, as suggested by prior research that has hypothesized a negative relation between budget levels and performance (Chow et al. 1988, 116).

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[3] We could present and test a hypothesis similar to H1b for final budget levels. Given the high relation between budget levels and budget slack (a correlation of -0.94, $p < 0.01$ in our experiment), we examine and report results only for budget slack.

Our multiple-subordinate setting also provides the superior with natural benchmarks for comparing budget levels and performance, potentially reducing the information asymmetry among subordinates and superiors regarding subordinates' production capabilities. Furthermore, our multiple-subordinate setting may foster social pressure and competition among subordinates, increasing subordinates' desires to outbid and outperform their co-workers (Young et al. 1993). Both types of benchmarking can reduce slack in subordinates' budgets and improve subsequent performance, even if superiors do not use the budget as a basis for allocating resources (Locke and Latham 1990, 119, 136–138; Hansen and Mowen 2003, 559). We explore the effects of such information in the next section.

Mitigating Effects of Information

The presence of multiple subordinates raises the issue of whether a superior should use the firm's information system to provide subordinates with information about their co-workers' budgets and performance. Under such relative performance evaluation (e.g., benchmarking), subordinates receive information regarding the activities and performance of comparable referents.[4] We refer to this as the "low horizontal information asymmetry" condition. Alternatively, the superior may choose to keep this information private ("high horizontal information asymmetry" condition). We expect that low horizontal information asymmetry will lead subordinates to increase their budget proposals and decrease their budget slack due to a combination of economic and psychological factors related to competition among subordinates.

From an economic perspective, if the superior uses higher budget proposals as a basis (benchmark) for setting all subordinates' budgets, then providing co-workers' budget and performance information to subordinates emphasizes that the economic benefits of submitting low budget proposals are likely to be low. Moreover, superiors may sanction subordinates who submit significantly lower budgets than their co-workers by substantially increasing these subordinates' final budget levels.[5]

From a psychological/sociological perspective, subordinates likely feel social pressure not to appear as if they have less ability or motivation than their co-workers (see also Young 1985). Furthermore, regardless of social pressure, some subordinates probably desire to outperform their co-workers, in terms of both their budget proposals and their actual performance. In this regard, information about co-workers gives subordinates explicit benchmarks by which to gauge their own budget and performance, thereby serving as a catalyst for social competition (Locke and Latham 1990, 136-137).

We further propose that the effect of providing subordinates with information about their co-workers will depend on whether the superior allocates resources among subordinates. Hypothesis 1 proposes that resource allocation provides subordinates with a strong economic incentive to propose budgets near their capabilities, which leaves little room for information about co-workers to affect budget proposals and budget levels. In contrast, when resources are fixed, subordinates have incentives to submit budgets with more slack, and information about co-workers is thus more likely to affect budget proposals and budget levels. Consequently, we posit that information on co-workers' budgets and performance will prompt subordinates to set more challenging budgets for themselves which, in turn, will lead to higher final budget levels containing less slack, but only when the superior does not allocate resources among subordinates.

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[4] Superiors also provide such information to subordinates to communicate and coordinate organizational activities (Horngren et al. 2000, 180). For example, Hilton et al. (2000, 625) note that, to facilitate coordination, "each manager throughout the organization must be aware of the plans made by other managers." We examine whether this common practice affects subordinates' motivation to misrepresent their private information and increase their task performance.

[5] The interaction among subordinates and superiors can be viewed as a strategic game. Thus, information about one subordinate's strategy (e.g., actual budget proposals, budget levels, and performance) can change another subordinate's strategy. For example, assume the superior attributes "large" differences between subordinates' budget proposals to self-interest (and ratchets up the lower bidding subordinate's final budget), but attributes "small" differences between subordinates' budget proposals to variations in skill level (and does not ratchet up the lower bidding subordinate's final budget). Given this retaliation (or "tit-for-tat") strategy by the superior, knowledge of coworkers' budget bids can affect subordinates' bidding strategies and concomitant budget levels.

H2a: Knowledge of other subordinates' budget proposals, counteroffers, budget levels, and performance will increase subordinates' initial budget proposals when resource allocations are fixed, but not when the superior allocates productive resources among subordinates.

H2b: Knowledge of other subordinates' budget proposals, counteroffers, budget levels, and performance will reduce the slack in the final budget when resource allocations are fixed, but not when the superior allocates productive resources among subordinates.

There are several reasons to expect that low information asymmetry will also increase subordinates' performance. First, if H2b is supported, then subordinates must produce more to receive compensation above their salary (base pay). Second, information about co-workers gives subordinates external performance goals that may increase their motivation (Locke et al. 1981; Locke and Latham 1990). Finally, to the extent that subordinates' budget proposals simply represent "cheap talk," social competition may manifest primarily in performance, which more directly reflects subordinate's ability and motivation. As with H2a and H2b, we predict in H2c that information about peer subordinates will affect subordinates' performance only when the superior does not allocate productive resources among subordinates.

H2c: Knowledge of other subordinates' budget proposals, counteroffers, budget levels, and performance will increase subordinates' own performance when resource allocations are fixed, but not when the superior allocates productive resources among subordinates.

Notwithstanding our predictions in H2a–H2c, there are several reasons why providing subordinates with information about their co-workers' budgets and performance may not increase subordinate budget proposals, reduce budget slack, and increase subordinate performance when resources are fixed. For example, superiors' use of higher-productivity subordinates as a benchmark for lower-productivity subordinates is more transparent when horizontal information asymmetry is low. Subordinates may perceive inequity in such explicit benchmarking and respond with lower budget proposals and performance. Knowledge of co-workers' actions (strategies) may also help subordinates implicitly collude to keep budget proposals low and budget slack high.

III. METHOD

Participants and Design

One hundred seventy-four undergraduate students participated in our computer-based laboratory experiment, which employed a 2 (resource allocation) × 2 (information asymmetry) × 3 (periods) mixed factorial design.[6] We manipulated resource allocation and information asymmetry between subjects, and we manipulated periods (work sessions) within subjects.

First, we assigned participants to one of two resource allocation settings. In the resource allocation condition, superiors allocated a total of six production minutes between two subordinates. In the no-resource allocation condition, each subordinate received a fixed three minutes of production time. Second, we assigned participants to one of two information asymmetry conditions. In the low (high) information asymmetry condition, each subordinate was (was not) aware of the other subordinate's budget bids and counteroffers, the superior's budget counteroffers to the other subordinate, and the other subordinate's final budget level and work-session performance. Finally, participants in all between-subjects conditions completed three negotiation/production work sessions (periods).

Procedure

The experimental procedure consisted of ten steps:

1. One-third of the participants assumed the role of superior, and two-thirds of the participants assumed the role of subordinate. The administrator matched each superior with two anonymous subordinates; solid room partitions separated all three matched participants.
2. The first few computer screens explained the production task, which was adapted from Chow (1983) and involved decoding numbers into letters.[7] All participants completed a brief practice session and three 3-minute training sessions to ensure they understood the task and learned their performance capabilities. In each training session, subordinates earned two cents for each item correctly decoded and received feedback regarding the number of items they correctly and incorrectly decoded.
3. After the third (last) training session, subordinates provided their best estimates of the number of items they could correctly decode in both a one-minute and a three-minute work session. These estimates formed the basis for calculating budget slack.
4. The next computer screens explained subordinates' and superiors' compensation contracts. Equations (1) and (2), respectively, describe these contracts, and computer screens illustrated these contracts to participants, using several numerical examples. For each subordinate, the salary (S) equaled \$0.35 per production minute assigned (\$2.10 in total for each work session across both subordinates), and the piece-rate per unit of production over budget (A) equaled \$0.05.[8] For superiors, profit per unit produced exclusive of subordinate compensation (D) equaled \$0.08, and the share of firm (residual) profit (R) equaled 100 percent.[9]

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[6] We take as given that budgets are used for performance evaluation and, as a result, do not manipulate the use of budgets for performance evaluation. This assumption reflects the pervasive use of budgets for performance evaluation (Umapathy 1987, 36), as well as the idea that in multiperiod settings, subordinates' past performance can serve as a benchmark for subjective performance evaluation, even in the absence of formal budget-based compensation schemes.

[7] Subordinates received a different decoding key for each session of the experiment. To minimize end-game effects, participants did not know the exact number of work sessions, and they received several extra decoding keys.

[8] S increases with the number of production minutes assigned to reflect the positive relation between compensation levels/perquisite consumption and resources, including the fact that salary (total base pay) typically increases as the amount of time worked increases (Henderson 2000, 665).

[9] Each subordinate received a minimum compensation in each work session of S × the number of production minutes assigned. All superiors were endowed with 6 × S = \$2.10 in each work session so that they would not have to use their own resources in the event firm profit before subordinate compensation was less than subordinates' base pay.

5. Participants learned that superiors would negotiate a separate (per-minute) budget for each subordinate for each work session, and that the superior would unilaterally set the final budget if the negotiating dyad did not reach agreement after two offer-counteroffer rounds. In the low information asymmetry condition, each subordinate also learned that he or she would receive information on the other subordinate's initial budget proposal and counteroffers, the superior's counteroffers to the other subordinate, and the other subordinate's final budget.[10]
6. In the resource allocation condition, computer screens informed participants that each subordinate's final budget equaled the negotiated (or superior-set) per-minute budget multiplied by the number of work session minutes the superior assigned that subordinate. In the no-resource allocation condition, the corresponding screens informed participants that each subordinate's final budget equaled the negotiated (or superior-set) per-minute budget multiplied by three minutes.
7. Participants reviewed the instructions until they could correctly answer a series of questions about the task, the compensation schemes, the budget-setting process, how production minutes were assigned, and the information available to each party during the experiment.
8. The budget-negotiation process began, with superiors and subordinates communicating via negotiation forms. Subordinates initiated the process by separately writing their initial budget proposals on the form. Once both subordinates made their initial budget proposals, the administrator delivered the negotiation forms to the superior, who wrote a counteroffer on each form. The administrator then delivered each form back to the appropriate subordinate, who had the opportunity to make a counteroffer. An equal offer and counteroffer indicated budget agreement and ended the negotiation process. If negotiation dyads failed to reach agreement within two offer-counteroffer rounds, then the superior set the budget. The negotiation procedures were identical between the low and high information asymmetry conditions, with one exception. In the low information asymmetry condition, the administrator also wrote on each subordinate's form the other subordinate's initial bid (and any counteroffers) as well as the superior's counteroffer(s) (including final budget levels) to the other subordinate before delivering the negotiation forms back to each subordinate.
9. At the conclusion of the budget negotiations, superiors in the resource allocation condition allocated the six production minutes between the two subordinates, with a minimum of one minute per subordinate.
10. When final budget levels were set, subordinates completed their work sessions. Afterward, each subordinate-superior dyad learned the subordinate's performance in the work session. Additionally, each subordinate in the low information asymmetry condition learned the other subordinate's work session performance. Participants then began the negotiation process for the second work session (period), and so on, until all participants had completed three negotiation/production work sessions. At the conclusion of the third work session, all participants completed an

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[10] Additionally, we informed all participants that 95 percent of past subordinates completing the letter-decoding task were able to correctly classify between 10 and 18 letters per minute. Thus, in both the resource allocation and no-resource allocation conditions, superiors knew the range of performance capabilities for all subordinates but not the performance capabilities of the specific subordinates with whom they were paired, whereas subordinates knew both their own performance capability and the range of performance capabilities of all subordinates.

exit questionnaire. The administrator then calculated and disbursed participants' compensation.

Dependent Measures

We examine three dependent measures: subordinates' initial budget proposals, subordinates' slack in the final budget, and subordinates' performance. Our analysis of initial budget proposals uses the initial per-minute budget bid each subordinate proposed in each work session. Because the number of production minutes can differ across subordinates in the resource allocation condition (but with a constant six-minute total across both subordinates), we perform our analyses of budget slack and performance using totals aggregated over both subordinates. Budget slack for each subordinate equals: (the subordinate's best estimate of the number of items he or she can correctly decode in a one-minute work session × the number of minutes allotted minus the final budget for the allotted time). We measure performance as the number of letters correctly decoded.

IV. RESULTS

Presentation of Experimental Results

We present our results in three tables, one for each dependent measure: initial budget proposals, budget slack, and performance. Panel A of each table presents means over the three periods for each dependent measure, by resource allocation condition and information asymmetry condition. Panel B of each table reports the results of the related ANOVA, with resource allocation and information asymmetry as between-subjects factors, and period as the within-subjects (repeated-measures) factor.

We organize tests by our two groups of hypotheses: resource allocation (H1a–H1c) and information asymmetry (H2a–H2c). Hypotheses 1a–1c would be supported by a significant main effect of resource allocation in the related ANOVA, in which the mean budget proposals are higher, budget slack is lower, and subordinate performance is higher when the superior uses budgets as a basis for allocating resources than when resource allocations are fixed.

We test H2a–H2c via planned comparisons of the simple effect of information asymmetry at each level of resource allocation.[11] First, H2a–H2c would be supported by a significant effect of information asymmetry in the no-resource allocation condition, in which the mean budget proposals are higher, budget slack is lower, and subordinate performance is higher when subordinates know the budget proposals, counteroffers, budget levels, and performance of the other subordinate than when they are not privy to this information. Second, H2a–H2c would be supported by an insignificant effect of information asymmetry in the resource allocation condition.

Resource Allocation (Hypotheses 1a, 1b, and 1c)

Panels A and B of Tables 1–3 document that our results are consistent with hypotheses 1a, 1b, and 1c. First, Panels A and B of Table 1 show that the mean initial subordinate budget proposal of 11.39 letters per minute in the resource allocation condition is significantly greater than the mean initial subordinate budget proposal of 7.74 letters per minute in the no-resource allocation condition ($F = 52.43$, $p < 0.01$). Second, Panels A and B of

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[11] Keppel (1991, 112) indicates that planned comparisons, rather than the main or interaction effects in the overall ANOVA, are the appropriate tests when specific relations are posited in hypotheses. Moreover, Keppel (1991, 112) indicates that one should conduct such planned comparisons without reference to the significance or nonsignificance of the omnibus F-test (also see Winer et al. 1991, 342).

TABLE 1

Mean Initial Subordinate Budget Proposals over the Three Experimental Periods by Resource Allocation Condition and Information Asymmetry Condition, Including the Related ANOVA[a]

Panel A: Means (Standard Deviations) over All Periods for Initial Subordinate Budget Proposals[b]

Information Asymmetry[d]	*Resource Allocation*[c]		
	No	*Yes*	*Overall*
High	6.97 (3.91) n = 90	10.83 (3.81) n = 84	8.83 (4.31) n = 174
Low	8.50 (3.23) n = 90	11.94 (2.89) n = 84	10.16 (3.51) n = 174
Overall	7.74 (3.66) n = 180	11.39 (3.42) n = 168	9.50 (3.98) n = 348

Panel B: ANOVA on Initial Subordinate Budget Proposals

Source of Variation	*SS*	*df*	*MS*	*F*[e]	*p*[f]
Between Subjects					
Resource Allocation (RA)[c]	1,155.25	1	1,155.25	52.43	< 0.01
Information Asymmetry (IA)[d]	151.51	1	151.51	6.88	< 0.01
RA × IA	3.95	1	3.95	0.18	< 0.67
Subject(RA × IA)	2,467.73	112	22.03		
Within Subjects					
Period[b]	3.78	2	1.89	0.26	< 0.78
Period × RA	76.10	2	38.05	5.18	< 0.01
Period × IA	1.02	2	0.51	0.07	< 0.94
Period × RA × IA	1.43	2	0.72	0.10	< 0.91
Period × Subject(RA × IA)	1,646.81	224	7.35		

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[a] A subordinate's initial budget proposal is the number of items per minute the subordinate proposed for his budget at the start of each period's budget negotiations.

[b] We manipulated Periods as a within-subjects factor with three levels, 1–3. Participants in all conditions completed three negotiation/production work sessions.

[c] We manipulated Resource Allocation as a between-subjects factor with two levels, present and not present. Participant subordinates in the no-resource allocation condition had a fixed three-minute work session. Participant subordinates in the resource allocation condition had a work session that lasted between one and five minutes, with the length of this work session determined by their superior.

[d] We manipulated Information Asymmetry as a between-subjects factor with two levels, low and high. In the low (high) information asymmetry condition, each participant subordinate was (was not) aware of the other participant subordinate's bids, counteroffers, final budget, and performance in each period of the experiment.

[e] The Subject(RA × IA) mean square is the appropriate error term for the between-subject effects. The Period × Subject(RA × IA) mean square is the appropriate error term for the within-subjects effects.

[f] We report a one-tailed p-value for the main effect of resource allocation given the directional prediction for this effect. All other p-values are two-tailed because we have no directional predictions for these effects.

TABLE 2
Mean Total Budget Slack over the Three Experimental Periods by Resource Allocation Condition and Information Asymmetry Condition, Including the Related ANOVA[a]

Panel A: Means (Standard Deviations) over All Periods for Total Budget Slack[b]

Information Asymmetry[d]	*Resource Allocation[c]*		
	No	*Yes*	*Overall*
High	16.75 (23.04) n = 45	−1.19 (17.55) n = 42	8.09 (22.36) n = 87
Low	7.39 (31.29) n = 45	−0.36 (13.91) n = 42	3.64 (24.66) n = 87
Overall	12.07 (27.73) n = 90	−0.78 (15.75) n = 84	5.87 (23.58) n = 174

Panel B: ANOVA on Total Budget Slack

Source of Variation	*SS*	*df*	*MS*	*F[e]*	*p[f]*
Between Subjects					
Resource Allocation (RA)[c]	7,169.86	1	7,169.86	10.17	< 0.01
Information Asymmetry (IA)[d]	794.26	1	794.26	1.13	< 0.29
RA × IA	1,130.09	1	1,130.09	1.60	< 0.21
Group(RA × IA)	38,067.71	54	704.96		
Within Subjects					
Period[b]	6,318.62	2	3,159.31	8.51	< 0.01
Period × RA	1,680.53	2	840.26	2.26	< 0.11
Period × IA	6.08	2	3.04	0.01	< 0.99
Period × RA × IA	677.32	2	338.66	0.91	< 0.41
Period × Group(RA × IA)	40,072.09	108	371.04		

[a] Budget slack for each subordinate equals the subordinate's best estimate of the number of items he could correctly decode in the allotted time minus the subordinate's final budget. Total budget slack equals the budget slack for subordinate 1 plus the budget slack for subordinate 2.

[b,c,d,e,f] See corresponding notes in Table 1.

Table 2 indicate that the mean total slack in the final budget of −0.78 in the resource allocation condition is lower than the mean slack of 12.07 in the no-resource allocation condition ($F = 10.17$, $p < 0.01$).[12] Finally, Panels A and B of Table 3 show that the mean total performance of 80.52 in the resource allocation condition is significantly greater (by

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[12] Mean slack in the final budget in the resource allocation condition is not significantly different from zero ($t = -0.45$, $p < 0.66$). This suggests that using budgets for both resource allocation and performance evaluation *eliminated* budget slack. Mean budget slack in the no-resource allocation condition, however, was significantly greater than zero ($t = 4.13$; $p < 0.01$).

TABLE 3
Mean Total Subordinate Performance over the Three Experimental Periods by Resource Allocation Condition and Information Asymmetry Condition, Including the Related ANOVA[a]

Panel A: Means (Standard Deviations) over All Periods for Total Performance[b]

Information Asymmetry[d]	*Resource Allocation*[c]		
	No	*Yes*	*Overall*
High	65.80 (20.62) n = 45	77.79 (13.86) n = 42	71.58 (18.58) n = 87
Low	61.49 (23.90) n = 45	83.24 (7.67) n = 42	71.98 (20.97) n = 87
Overall	63.65 (22.29) n = 90	80.52 (11.47) n = 84	71.79 (19.75) n = 174

Panel B: ANOVA on Total Performance

Source of Variation	*SS*	*df*	*MS*	*F*[e]	*p*[f]
Between Subjects					
Resource Allocation (RA)[c]	12,361.52	1	12,361.52	19.01	< 0.01
Information Asymmetry (IA)[d]	13.74	1	13.74	0.02	< 0.89
RA × IA	1,035.44	1	1,035.44	1.59	< 0.22
Group(RA × IA)	35,107.80	54	650.14		
Within Subjects					
Period[b]	255.48	2	127.74	0.76	< 0.47
Period × RA	240.47	2	120.23	0.72	< 0.49
Period × IA	284.44	2	142.22	0.85	< 0.44
Period × RA × IA	155.36	2	77.68	0.46	< 0.63
Period × Group(RA × IA)	18,082.37	108	167.43		

[a] Performance for each subordinate equals the number of letters correctly decoded in the allotted time. Total performance equals the performance of subordinate 1 plus the performance of subordinate 2.
[b,c,d,e,f] See corresponding notes in Table 1.

approximately 25 percent) than the mean total performance of 63.65 in the no-resource allocation condition ($F = 19.01$, $p < 0.01$).[13]

We performed additional analysis to distinguish between two possible explanations for subordinates' higher performance when their superiors used the budget as a basis for resource allocation: (1) superiors allocated more productive resources (minutes) to the more productive subordinates or (2) subordinates were motivated to increase their output. To assess the effect of differential resource allocation, we computed a benchmark performance assuming the superior allocated resources equally—i.e., three production minutes to each subordinate. When we multiplied each subordinate's average per-minute performance by three production minutes, the result was nearly identical to the actual performance reported in Table 3. Average performance under equal resource allocation only declined to 80.05 (from 80.52 under the actual resource allocation).[14]

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[13] Panel B of Tables 1 and 2 also reveal two statistically significant within-subjects effects. First, Panel B of Table 1 shows a significant period-by-resource allocation interaction effect for subordinate budget proposals ($F = 5.18$, $p < 0.01$). The (predicted) positive difference between initial subordinate budget proposals in the resource allocation condition vs. the no-resource allocation condition increased over periods, from 2.55 letters per minute in period 1 to 4.83 letters per minute in period 3. Second, Panel B of Table 2 shows a significant main effect of period on budget slack ($F = 8.51$, $p < 0.01$). This occurs because the mean total budget slack increased over periods, from an average of –2.84 in period 1 to 10.56 in period 3.

This result suggests that the higher subordinate performance in the resource allocation condition is not due to superiors assigning more resources to the more productive of the two subordinates. Rather, the higher production is due primarily to subordinates' increased motivation to acquire scarce resources and meet their (higher) budget goals. The higher initial subordinate budget proposals in the resource allocation condition and participants' subjective perception of their motivation support this interpretation. Our post-experimental questionnaire elicited participants' level of agreement with the statement "I was very committed to attaining each of my performance targets" on a scale on which 10 corresponded with "strongly disagree" and 70 with "strongly agree." The mean response of 54.53 in the resource allocation condition significantly exceeded the mean response of 45.42 in the no-resource allocation condition ($t = 2.50$, $p < 0.01$), suggesting that participants in the resource allocation condition were more motivated than those in the no-resource allocation condition to achieve their performance targets.[15]

Information Asymmetry (Hypotheses 2a, 2b, and 2c)

Panel A of Tables 1-3 document that our results generally are consistent with hypotheses 2a, 2b, and 2c. In the no-resource allocation condition the mean initial subordinate budget proposal of 8.50 letters per minute in the low information asymmetry condition was significantly greater than the mean budget proposal of 6.97 letters per minute in the high information asymmetry condition (planned comparison $t = 2.19$, $p < 0.02$, one-tailed). Additionally, mean budget slack of 7.39 in the low information asymmetry condition is significantly less than mean budget slack of 16.75 in the high information asymmetry condition (planned comparison $t = -1.68$, $p < 0.05$, one-tailed). However, contrary to predictions in H1c, the mean performance of 61.49 in the low information asymmetry condition is not statistically different from the mean performance of 65.80 in the high information asymmetry condition (planned comparison $t = -0.80$, $p < 0.79$, one-tailed).[16]

In the resource allocation condition, the difference in subordinates' budget proposals between the low and high information asymmetry conditions is not statistically significant

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[14] This analysis assumes that performance increases linearly with the number of production minutes. On average, subordinates in the experiment estimated that they could decode 13.98 letters in one minute and 41.55 letters in three minutes. This suggests that the relation between production minutes and performance is fairly linear throughout the time range we consider.

[15] The relative influence on performance of differential resource allocation vs. heightened motivation is partially task-specific, however. Compared to some field settings, there was modest variation in both the amount of resources the superior could allocate (between one and five minutes per subordinate) and actually did allocate in our experiment; across all periods, superiors opted for a 3-3 split of production minutes 33 percent of the time, a 4-2 split 55 percent of the time, and a 5-1 split only 12 percent of the time. There also was somewhat modest variation in the skill levels of participants performing the letter-decoding task; the standard deviation in performance for the last three-minute training session was 3.89 letters, or approximately 10 percent of average performance. Differential allocation of resources among subordinates is likely to have more impact on performance for a task that has substantial variation in either the amount of resources to be allocated or subordinate skill levels.

[16] The appropriate one-tailed p-value is $1 - \alpha_t$ because the difference in mean performance between information asymmetry conditions in the no-resource allocation condition is opposite to the direction predicted by H2c.

(11.94 vs. 10.83, respectively, as reported in Panel A of Table 1; planned comparison t = 1.52, $p < 0.14$, two-tailed). Moreover, there is no statistically significant difference in budget slack between low and high information asymmetry conditions (–0.36 vs. –1.19, respectively, as reported in Panel A of Table 2; planned comparison t = –0.14, $p < 0.89$, two-tailed), or in performance between low and high information asymmetry conditions (83.24 vs. 77.79, respectively, as reported in Panel A of Table 3; planned comparison t = 0.35, $p < 0.73$, two-tailed).

V. SUMMARY AND DISCUSSION

In this paper, we investigate whether two managerial practices—using budgets to allocate scarce resources, and providing information about subordinates to their co-workers—mitigates the dysfunctional effects on subordinates' behavior of using budgets for performance evaluation. We examine how these two practices affect two facets of subordinates' behavior: (1) their motivation to misrepresent their private information to create budget slack, and (2) their motivation to increase their effort and task performance after budgets are determined.

The results from our laboratory experiment are consistent with our hypotheses that using budgets both as a basis for allocating scarce productive resources among subordinates and as a basis for evaluating subordinates significantly increases subordinates' initial budget proposals, decreases slack in the final budget, and increases subordinate performance. Specifically, when superiors used budgets to allocate scarce resources, subordinates had no slack in their budgets, and their task performance increased by 25 percent. Additionally, we found that subordinates' task-performance increase was due to their heightened motivation to acquire scarce resources and exceed budget targets, rather than to the manner in which superiors allocated resources among subordinates.

We also find that the degree of information asymmetry among subordinates affects budget outcomes, but only when superiors do not use budgets to allocate resources. When the superior could not use budgets to allocate scarce resources, subordinates who knew the budget proposals, counteroffers, and performance of the other subordinate made higher initial budget proposals and had final budgets containing less slack than subordinates who did not have this information. When the superior could use budgets to allocate scarce resources, the degree of information asymmetry did not significantly affect subordinates' initial budget proposals or the slack in the final budget. Finally, the degree of information asymmetry between subordinates did not significantly affect subordinates' performance.

The results of our study demonstrate the interdependent effects of the planning and control roles of managerial accounting practices such as budgeting. Our research suggests that firms can mitigate problems arising when superiors use budgets only for performance evaluation or only as a basis for resource allocation by using the same budget for both purposes. Thus, our results provide insight into why firms frequently use a single budget for both planning and control purposes and employ traditional budget-based, rather than "truth-inducing," compensation schemes. Our results also suggest that *ex post* inefficiencies such as the existence of slack and capital rationing (i.e., the use of a hurdle rate in excess of the firm's cost of capital) described in analytic research (e.g., Antle and Eppen 1985; Antle and Fellingham 1997) may not be necessary when budgets provide a basis for both resource allocation and performance evaluation. More generally, our study highlights the advantages of simultaneously studying the multiple roles of managerial accounting tools, such as budgets, to evaluate their effectiveness (also see Sprinkle 2002).

In contrast to research that focuses on conflicts between the dual roles of budgets (Epstein and Manzoni 2001), our study suggests that the tension between these two roles can be beneficial. Accordingly, our study supplements research demonstrating the effort and performance-enhancing effects of budget-based contracts relative to other contracts (see, e.g., Bonner et al. 2000), by suggesting that firms can further increase employee productivity with budget-based contracts by linking resources to budget proposals or prior budget attainment. Alternatively, if the firm does not use the budget as a basis for allocating resources, then our results suggest that the design of a firm's internal information system can play an important role in mitigating subordinates' incentives to misrepresent their private information when their superiors use the budget only as a basis for performance evaluation. Here, we document that, similar to the effect of reducing vertical information asymmetry between subordinates and superiors (Young 1985; Fisher et al. 2002), reducing the degree of horizontal information asymmetry among subordinates reduces budget slack.

Some limitations of our study provide opportunities for future research. For example, we propose that the level of information asymmetry affects both economic and psychological/sociological factors, but we do not explore the relative influences of these factors. Future research could investigate more thoroughly the mechanisms governing our information asymmetry results by measuring subordinates' reputation concerns and perceptions of social pressure, and/or by manipulating whether such information affects the economic incentives associated with the strategic game among subordinates and superiors. Investigating these mechanisms and motivations may help explain why horizontal information asymmetry affects subordinate behavior and, in turn, when it is most likely to do so. Future research also might examine the robustness of our results to individual, task, and environmental variables (e.g., risk preferences, uncertainty, type of feedback), which we did not manipulate or measure but which can affect budget outcomes (see Luft and Shields 2002).

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The Role of Accounting Conservatism in Mitigating Bondholder-Shareholder Conflicts over Dividend Policy and in Reducing Debt Costs

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ABSTRACT: Using both a market-based and an accrual-based measure of conservatism, we find that firms facing more severe conflicts over dividend policy tend to use more conservative accounting. Furthermore, we document that accounting conservatism is associated with a lower cost of debt after controlling for other determinants of firms' debt costs. Our collective evidence is consistent with the notion that accounting conservatism plays an important role in mitigating bondholder-shareholder conflicts over dividend policy, and in reducing firms' debt costs.

Keywords: *accounting conservatism; dividend policy conflicts; cost of debt.*

Data Availability: *All data used in the study are publicly available from the sources identified in the text.*

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I. INTRODUCTION

Watts (1993) suggests that conservatism likely evolved from the contracting role of accounting.[1] He argues that accounting conservatism helps avoid inappropriate distributions to claim holders. Consistent with Watts (1993), we argue that conservatism mitigates conflicts of interest over dividend policy between shareholders and bondholders.[2] Specifically, we hypothesize that (1) firms experiencing more severe bondholder-shareholder dividend policy conflicts adopt more conservative accounting and (2) firms that use more conservative accounting incur a lower debt cost. The intuition for our hypotheses is as follows. Conservative accounting reduces the earnings and retained earnings amounts used in debt contracts to constrain dividends.[3] Thus, choosing more conservative accounting is one way the firm can reduce the risk to bondholders that it will pay excessive dividends to shareholders. This risk reduction is likely to be more important when bondholder-shareholder conflicts over dividend policy are more severe. Moreover, we expect that if managers choose more conservative accounting, then bondholders will accept a lower rate of return in light of the reduced risk of dividend overpayment.

We test these hypotheses using both "levels" and "changes" analyses based on data from two six-year periods: 1993–1998 and 1987–1992. We use two alternative measures of conservatism: a market-value-based measure and an accrual-based measure. Our first measure of conservatism, based on Beaver and Ryan (2000), is the component of the book-to-market ratio that reflects biased accounting due to earlier recognition of expenses and losses, and to deferred revenue recognition. This component captures the extent to which book value is persistently lower than market value. Beaver and Ryan (2000) demonstrate (and we confirm) that this component of book-to-market is associated with conservative accounting method choices.

Our second measure of conservatism is based on Givoly and Hayn (2000), who argue that conservative accounting leads to persistently negative accruals. In the long run, unbiased accounting results in the cumulative amount of net income before depreciation and amortization converging to operating cash flows. Thus, as both positive and negative accruals reverse over time, net cumulative accruals should approach zero. In contrast, conservative accounting results in a persistent pattern of negative accruals over time. This suggests that a firm's mean accrual over a reasonably long period provides an accounting-based, firm-specific proxy for conservatism.

Because we cannot directly observe the extent to which managers voluntarily make conservative accounting choices, our two proxies for conservatism may reflect GAAP-mandated conservatism as well as managers' choices of accounting methods, estimates, and assumptions. However, GAAP-mandated conservatism likely affects firms in the same industry in similar ways. Thus, to control for GAAP-mandated conservatism, we repeat our analyses using industry-adjusted measures of conservatism and firm characteristics. Furthermore, by using each firm as its own control, our "changes" analysis controls for any effects of GAAP-mandated conservatism that vary across firms.

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[1] Statement of Concepts No. 2 (FASB 1980) defines conservatism as "a prudent reaction to uncertainty to try to ensure that uncertainty and risks inherent in business situations are adequately considered." According to Statement of Concepts No. 6, (FASB 1985, para. 44), the motivation for expensing certain intangibles or not recognizing gains is *uncertainty* about whether the items qualify as assets or liabilities.

[2] Conservatism may *indirectly* affect other conflicts. For example, restrictions on dividend policy may lead borrowers to retain more funds, which would in turn reduce the underinvestment problem. Similarly, conservatism reduces book value of equity, which places constraints on financing policy via more conservative leverage ratios. We leave more complete analysis of these other conflicts for future research.

[3] Penman and Zhang (2002) point out that an exception to this generalization occurs when firms that use conservative accounting methods reduce their operating assets. As Penman and Zhang (2002) show, a reduction in operating assets can increase current period income due to the liquidation of hidden reserves. To the extent that reductions in operating assets inflate earnings, this biases against our hypotheses, which are based on the assumption that conservative accounting reduces income.

The results of our analyses are for the most part consistent with our first hypothesis, that firms with more severe bondholder-shareholder dividend policy conflicts, on average, use more conservative accounting. The results are also strongly consistent with our second hypothesis: firms that adopt more conservative accounting practices incur lower debt costs, on average. Our results hold after controlling for other factors (e.g., profitability, firm size, firm growth) that are potentially correlated with our proxies for conservatism and debt cost.

The study's results contribute to our understanding of the real economic effects of accounting conservatism in two ways. First, we provide evidence that accounting conservatism mitigates conflicts over dividend policy. Although prior studies have documented the presence of conservatism in debt contracts, we believe this is the first study that provides evidence suggesting that conservatism plays an important role in efficient debt contracting. Second, by documenting that conservatism is associated with a lower cost of debt, our results suggest that accounting choices can significantly affect firms' cost of capital. In other words, firms that choose to build a reputation for conservative accounting appear to reap real economic benefits. Our evidence that use of conservative accounting leads to lower cost of debt extends Botosan's (1997) and Sengupta's (1998) conclusions that higher quality disclosures lead to lower cost of equity and lower cost of debt, respectively.

The paper proceeds as follows. Section II builds on prior research to develop the hypotheses. Section III describes our empirical proxies for conservatism, debt cost, and bondholder-shareholder conflicts over dividend policy. Section IV describes our research design. Section V describes the sample and presents the results, and Section VI concludes the paper.

II. PRIOR RESEARCH AND HYPOTHESES

Prior Research

Considerable controversy remains whether accounting conservatism yields any real economic benefit to firms (Devine 1963; Sterling 1967). For example, Hendriksen and Van Breda (1992, 149) state, "conservatism is, at best, a very poor method of treating the existence of uncertainty in valuation and income. At its worst, it results in a complete distortion of accounting data." Similarly, Penman and Zhang (2002) suggest that conservatism results in lower quality earnings. Intuitively, conservative accounting practices accelerate the expensing of investment costs, thus reducing income and generating hidden reserves, as long as the firm is increasing investment expenditures. Consequently, conservative accounting leads to lower *cumulative* reported earnings. However, when the firm reduces its investment expenditure levels, those same conservative accounting practices liquidate hidden reserves, which in turn leads to higher *current* period income. Thus, conservative accounting leads to lower cumulative reported earnings, but the effect on current period earnings depends on investment growth.

Although many prior studies examine the relation between accounting choice and debt covenants, the more general role of conservatism in debt contracting has not received much attention. Prior research documents that debt contracts often require use of conservative accounting methods (Leftwich 1983; El-Gazzar and Pastena 1990; Leuz et al. 1998). For example, Leftwich (1983) finds that modifications to GAAP in debt contracts are typically conservative. However, there is no direct evidence on the role that accounting conservatism plays in mitigating bondholder-shareholder conflicts, or on the association between conservatism and a firm's cost of debt.

Role of Conservatism in Mitigating Dividend Policy Conflicts

Agency theory suggests that fixed and residual claimants in a firm have conflicting interests over dividend policy (Jensen and Meckling 1976). In particular, overpayment of dividends can transfer wealth from bondholders to shareholders by reducing the assets available for meeting bondholders' fixed claims and hence increasing the default risk for bondholders. Efficient contracting theory suggests that it is in the interests of all parties to mitigate these conflicts. To address this conflict, bondholders typically include dividend policy restrictions in debt contracts (Black 1976; Smith and Warner 1979; Kalay 1982; Leftwich 1983; Healy and Palepu 1990; El-Gazzar and Pastena 1990, 1991).

Kalay (1982) finds that firms use two types of dividend restrictions in their public debt contracts: direct and indirect. Direct restrictions specify an upper bound on dividends in terms of cumulative net earnings or retained earnings (Begley 1994). Kalay (1982) documents that 90 percent of his random sample of 150 firms, selected from the *Moody's Industrial Manual*, report a direct dividend restriction. Indirect restrictions constrain dividends indirectly by requiring the firm to maintain certain balance sheet ratios. For example, a cap on the debt-to-asset ratio constrains dividends because dividend payments reduce assets, and thus move the debt-to-asset ratio closer to its upper bound. We assume that our sample firms either have or expect to have dividend restrictions in their debt contracts.

Measurement of accounting numbers used in specifying restrictions on dividend policy plays an important role in debt contracting. Conservative accounting directly affects the earnings and retained earnings amounts used in debt contracts to constrain dividends, in that conservative accounting leads to lower (cumulative) reported earnings. Similarly, conservative accounting implies tighter restrictions on dividend policy by reducing the measures of assets in various balance sheet ratios that appear in debt contracts. In short, when debt contracts impose accounting-based restrictions on dividends, more conservative accounting reduces the likelihood that the firm will make excessive dividend payments to shareholders.

Debt contracts incorporate conservatism in at least two ways. First, bondholders can explicitly require the use of conservative accounting. Second, managers might implicitly commit to consistently use conservative accounting in order to build a reputation for conservative financial reporting. Milgrom and Roberts (1992) argue that reputational concerns may effectively check managers' willingness to renege on such a commitment by engaging in *ex post* opportunistic accounting choices.

In sum, we expect that when bondholder-shareholder conflicts over dividend policy are potentially more severe, borrowing firms are likely to use more conservative accounting. The borrowing firms' managers' willingness to choose more conservative accounting will also depend on the costs associated with increased conservatism. However, holding the costs of conservatism constant, we expect cross-sectional differences in conservatism to be related to the severity of bondholder-shareholder conflicts over dividend policy. Our first hypothesis (stated in alternate form) is as follows:

> **H1:** Firms that face more severe bondholder-shareholder conflicts over dividend policy adopt more conservative accounting, *ceteris paribus*.

Conservatism and the Cost of Debt

As discussed above, managers' choices of more conservative accounting tighten restrictions on dividend policy, decreasing the likelihood of excessive dividend payments. Thus, bondholders are likely to require a lower rate of return to compensate for the reduced risk of excessive dividend payments. Therefore, we expect conservatism to reduce a borrower's cost of debt. Our second hypothesis (stated in alternate form) is as follows:

H2: Firms that adopt more conservative accounting incur a lower cost of debt, *ceteris paribus.*

Our tests of these hypotheses assume that firms' investment opportunities drive their financing and dividend policies. Given the level of financing required to implement a firm's investment opportunities, we assume that managers wish to minimize the firm's cost of debt. Borrowing firms can reduce their cost of debt by accepting tighter restrictions on their ability to pay dividends, via conservative accounting. We recognize that borrowing firms might use other mechanisms to address the dividend policy conflict. For example, secured debt would ensure that specific assets would be available to meet bondholder claims, thereby obviating the need for conservative accounting to mitigate this conflict. However, secured debt also has disadvantages, such as reducing managerial flexibility regarding the disposal of secured assets, and exposing bondholders to the risk of decline in the asset values. If secured debt is a widely used alternative, then it will most likely bias against finding support for our predictions. We leave to future research the investigation of other ways borrowers can mitigate dividend policy conflicts.

The issue then arises as to what prevents *ex post* departures from conservatism, and how bondholders detect and respond to such departures. One disincentive to deviate from conservatism is that building a reputation for conservatism is costly in terms of reporting relatively lower firm performance. Departing from conservatism risks the loss of reputation (Milgrom and Roberts 1992). Debt-rating agencies periodically reevaluate firms' creditworthiness, so deviations from conservatism could increase interest costs on future borrowings. Furthermore, once the borrowing firm's reputation is damaged, bondholders are unlikely to view the firm favorably if and when it returns to the bond market. Thus, managers of firms that expect to issue more debt are likely to find it costly to deviate from conservatism.

This efficient-contracting perspective contrasts with two alternative hypotheses about accounting choice. The first alternative is the traditional debt hypothesis, in which managers have *ex post* incentives to choose aggressive accounting in order to avoid covenant violations. However, Mohrman (1996) shows that more recent debt contracts generally specify accounting methods, thereby restricting managers' ability to change accounting methods *ex post.* The second alternative hypothesis is that managers could choose aggressive accounting thereby increasing income in an attempt to appear less risky to prospective bondholders. This hypothesis assumes that bondholders cannot see through managers' accounting decisions, and thus are fooled by the appearance of less risky financial statements.

III. EMPIRICAL PROXIES

In this section, we first describe our proxies for accounting conservatism, as well as their limitations. We then discuss our proxies for the severity of bondholder-shareholder conflicts over dividend policy, and we conclude the section with a discussion of our proxy for the cost of debt.

Accounting Conservatism

Although conservatism broadly affects book value of equity and net income, accountants often focus on narrower forms of conservatism. For example, the choice of accelerated depreciation as opposed to straight-line, the immediate expensing of research and development expenditures per SFAS No. 2 (FASB 1974), or the lower-of-cost-or-market rule for inventory valuation are conservative accounting method choices. However, our study relies on a broader conservatism construct that reflects the *cumulative* effect of managers' accounting choices, including accounting estimates and assumptions, as well as accounting method choices.

Relying on the notion that conservative accounting generally reduces (1) book value of equity relative to market value of equity and (2) income relative to operating cash flows, we use two alternative measures of conservatism: a market-value-based measure following Beaver and Ryan (2000), and an accrual-based measure following Givoly and Hayn (2000). These measures reflect the aggregate effects of conservatism at a firm level, as opposed to an account or line-item level within the firm.[4]

Market-Value-Based Measure of Conservatism

Beaver and Ryan (2000) argue that variation in book-to-market ratios is a function of two components: biased accounting recognition and lagged accounting recognition. They suggest that the bias component of book-to-market reflects *persistent* differences between book and market values resulting, in part, from conservative accounting. This notion of conservatism—book values persistently below market values—is consistent with that in Feltham and Ohlson (1995). Beaver and Ryan (2000) also suggest that the lag component reflects *temporary* differences in book and market values due to the lag with which accounting recognizes unexpected gains and losses.

Beaver and Ryan (2000) empirically measure the bias (conservatism) and lag components by regressing book-to-market ratios onto current and lagged returns with fixed firm and time effects. The firm-specific effect reflects the persistent bias component primarily due to conservatism, the time-specific effect captures economy-wide temporal variation, and the lag component reflects market shocks not yet recognized in book value.

Following Ryan (1995) and Beaver and Ryan (2000), we measure conservatism based on the firm-specific coefficient, α_i, in the following model:

$$\mathrm{BTM}_{it} = \alpha + \alpha_i + \alpha_t + \sum_{k=0}^{6} \beta_k \, \mathrm{RET}_{it-k} + e_{it} \qquad (1)$$

where:

BTM_{it} = the book-to-market ratio for firm i at fiscal year-end t;
α = the intercept across all firms and years;
α_i = the persistent firm-specific bias component of book-to-market ratio over the sample period;
α_t = the year-specific component of book-to-market ratio across all firms; and
RET_{it} = the stock return (with dividends) for firm i in year t.

We perform the fixed-effects estimation separately on panel data for the two sample periods 1993–1998 and 1987–1992. The coefficient, α_i, reflects the persistent firm-specific component of book-to-market over each six-year period relative to the other firms in the sample. As a component of book-to-market, α_i measures conservatism *inversely* (i.e., α_i

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[4] Basu (1997) characterizes conservatism in terms of asymmetric recognition of good news vs. bad news in financial statements. We do not use Basu's (1997) measure of conservatism for two reasons. First, the relation between his measure and the conservatism of accounting choices is unclear. Shon (2000) concludes that firms that use more conservative accounting methods appear to be less conservative based on the Basu measure. Second, Muller and Riedl (2001) show that inferences based on the Basu (1997) measure could be an artifact of differences in variances of good news returns vs. bad news returns.

decreases as market value increases relative to book value). For ease of presentation we multiply α_1 by -1 and refer to this market-based proxy for conservatism as CONMKT. We expect more conservative accounting to yield higher measures of CONMKT, thus H1 predicts a positive relation between CONMKT and proxies for bondholder-shareholder conflicts over dividend policy. In addition to reflecting conservatism, CONMKT is also likely to reflect growth opportunities because market values reflect growth opportunities (unrecorded assets). We discuss controls for these other determinants of CONMKT in Section IV.

Accrual-Based Measure of Conservatism

Givoly and Hayn (2000) focus on the income-statement effects of conservatism over extended periods. They argue that conservative accounting leads to *persistently* negative accruals, in contrast with the expected pattern of accrual reversals. This suggests that a firm's mean accrual over a reasonably long period provides a firm-specific proxy for conservatism. Consistent with Givoly and Hayn (2000), we use accruals before depreciation as a proxy for conservative accounting.[5] Specifically, we use the sum of total accruals excluding depreciation (net income before extraordinary items plus depreciation expense less operating cash flows) deflated by assets and averaged within each of the two sample periods, 1993–1998 and 1987–1992, respectively, as our accrual-based proxy for conservatism.[6] We also multiply this measure by -1 so it is increasing in the amount of negative accruals, and refer to it as CONACC. We expect more conservative accounting to yield higher measures of CONACC.

CONACC includes changes in current assets and current liabilities, deferred taxes, pension accruals, asset write-downs, gains or losses on sale of assets, bad debt provisions, restructuring accruals, and other accrued or deferred expenses and revenues. Managers can exercise discretion over the conservatism with which they record most of these items in terms of the amount and timing. However, these accruals also reflect economic characteristics unrelated to conservatism, such as growth in operations (Givoly and Hayn 2000). We discuss controls for these other determinants of CONACC in Section IV.

Discussion of Conservatism Measures

Our two conservatism measures reflect the relative degree of conservatism across individual firms from two unique perspectives. CONMKT is a *stock* measure that reflects the cumulative effect of conservative accounting on book value of equity. CONACC, on the other hand, is a *flow* measure that reflects the effects of conservative accounting in the periods over which we calculate the flow measure.

As described in Section II, our hypotheses focus on the extent to which borrowing firms' managers voluntarily choose conservative accounting methods and estimates. However, our conservatism proxies do not directly measure the conservatism of managers' accounting choices. Thus, our conservatism proxies do not distinguish between the nondiscretionary (GAAP-mandated) component of conservatism and the discretionary component of conservatism. There are several reasons why the nondiscretionary component of conservatism is unlikely to significantly affect our inferences.

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[5] Depreciation accruals represent the reversal of a positive accrual when the asset is acquired (i.e., net income exceeds cash flows). Givoly and Hayn (2000) exclude the depreciation accrual because the originating positive accrual is not captured in the difference between income and *operating cash flow*. The use of accruals before depreciation ignores any conservatism in a firm's depreciation calculation, which biases against our hypotheses.

[6] Consistent with the implications of Givoly and Hayn (2000), aggregating accruals over an extended time period should better reveal firm-specific conservatism in accruals. We use six-year estimation periods for comparability with CONMKT. We repeat our tests with three-year estimation periods for this proxy. All results are qualitatively similar to those reported in the tables. Periods shorter than three years, in our view, would be less appropriate for measuring conservatism as *persistently* negative accruals. We also repeat our analyses using net income *after* extraordinary items, and all inferences remain the same.

First, even when GAAP appears to mandate conservatism, managers nonetheless have considerable discretion in its application. For example, although GAAP requires immediate expensing of R&D, managers can exercise discretion over the level and timing of R&D spending. Similarly, GAAP requires firms to use the lower-of-cost-or-market rule to value inventory, but managers must exercise discretion in estimating "market" value of inventory and determining the appropriate level of aggregation to apply the test. Thus, managers exercise discretion over the application of even GAAP-mandated conservative accounting principles.

Second, to the extent that certain GAAP requirements tend to be conservative, these standards generally apply to all firms. We have no *a priori* reason to expect that GAAP-mandated conservatism will be higher for firms with potentially more severe bondholder-shareholder conflicts over dividend policy, which could confound our analysis of the hypothesized relation between the conservatism of managers' *discretionary* choices and dividend policy conflicts.[7] Furthermore, it is unlikely that conservative GAAP result from standard-setters' attempts to ameliorate conflicts among different stakeholders.[8]

Third, the effect of mandated conservatism likely varies systematically across industries. For example, the level of R&D expenditures is higher in some industries (e.g., pharmaceuticals) than in others. Inventory write-downs also tend to cluster by industry (e.g., computer technology). To provide further assurance that our results are not a manifestation of mandated conservatism rather than the conservatism of managers' discretionary choices, we repeat our analysis using industry-adjusted conservatism measures and explanatory variables. Because results based on these industry-adjusted measures are similar to the results using unadjusted measures, it is unlikely that cross-sectional variation in mandated conservatism drives our results.

Finally, our "changes" analysis that uses each firm as its own control also controls for variation in GAAP-mandated conservatism across firms. Our consistent results using both levels and changes specifications provide further assurance that our results are not driven by GAAP-mandated conservatism.

Penman and Zhang (2002) conclude that conservative accounting can cause current period income to increase in periods of declining operations, due to the liquidation of hidden reserves (e.g., the LIFO reserve). To the extent that a firm has high income (relative to operating cash flows) due to such liquidation of hidden reserves, our CONACC measure will not classify the firm as using conservative accounting. This potential error in our measurement of accounting conservatism is unlikely to affect our inferences. First, our conservatism measures are averaged over six-year periods. This averaging mitigates any temporary effects on current income induced by temporary changes in investment levels. Second, we obtain identical inferences when we repeat our empirical tests after dropping firms with declining operations, proxied by negative sales growth.

An alternative approach would measure conservatism using specific accounting choices, such as LIFO vs. FIFO, or accelerated vs. straight-line depreciation methods. We do not use specific accounting method choices, per se, to measure conservatism, because (1) they are difficult to aggregate, (2) they do not reflect the magnitude of the effects of conservatism, and (3) they do not capture the conservatism of many other discretionary choices, such as estimates of bad debt expense, warranty expense, or impairment losses.[9] In Section V, we discuss the correlation between our proxies for aggregate conservatism and some of the specific accounting choices prior studies suggest as conservatism measures.

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[7] To the contrary, high-technology firms likely have more (mandated) conservative accounting (e.g., due to the writing off of intangibles) and pay lower dividends than low-technology firms with relatively high assets-in-place. This works against our hypothesized positive relation between discretionary conservatism and the level of dividends.

[8] We thank an anonymous referee for suggesting this point.

Measuring Bondholder-Shareholder Conflict over Dividend Policy

We use three proxies for bondholder-shareholder conflicts over dividend policy: operating uncertainty, the level of dividends (as a percentage of assets), and leverage. Firms that face a relatively high degree of operating uncertainty, such as demand or input price uncertainty, are more likely to experience large positive or negative shocks to their earnings and asset values. Large positive shocks that are unsustainable will tend to inflate retained earnings, possibly resulting in overpayment of dividends. In other words, greater uncertainty about future profits implies a greater risk that current dividends transfer too many resources to shareholders (Watts 1993). Thus, the higher the operating uncertainty, the more severe the bondholder-shareholder conflicts over dividend policy. We measure the borrowing firm's operating uncertainty by the standard deviation of its return on assets (STDROA).

As a second proxy for bondholder-shareholder conflicts of interest over dividend policy, we use the level of dividends, measured as a percentage of assets. If a firm pays a low level of dividends, then bondholders are less likely to be concerned about dividend overpayment. Conversely, paying a relatively high level of dividends potentially exacerbates the conflicts with debt holders, and thus a high level of dividend payments likely indicates more severe bondholder-shareholder conflicts over dividend policy.[10]

Our third proxy for the severity of bondholder-shareholder conflicts is leverage, measured as the ratio of long-term debt to assets. *Ceteris paribus*, higher leverage implies a relatively larger claim on the firm's assets by bondholders. From a bondholder's perspective, higher leverage intensifies the conflicts of interest with shareholders and the concern over excess distributions. Thus, firms with high leverage likely experience greater bondholder-shareholder conflicts over dividend policy.

Measuring Firms' Cost of Debt

Our measure of firms' cost of debt is their senior debt rating assigned by Standard and Poor's (S&P). Prior research has shown that a firm's debt ratings are closely associated with its eventual payoff of interest and principal obligations. For example, Altman (1992) finds that of the debt that ultimately defaults, more than 90 percent is rated below investment grade (BBB) at least a year prior to default. Furthermore, S&P (1986) documents that more favorable ratings are associated with lower interest rates. Hand et al. (1992) find that changes in bond ratings are positively related to changes in daily bond prices around rating-change announcements. Liu et al. (1999) also find similar results. Together, these studies provide substantial support for using debt ratings as a proxy for firms' cost of debt.

Kaplan and Urwitz (1979) find that debt ratings vary cross-sectionally with firm characteristics, such as profitability, firm size, and equity-risk. To examine whether accounting conservatism has an incremental effect beyond the effects of firm characteristics documented in prior work, our analyses control for these firm characteristics as discussed further in Section IV.

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[9] For example, two firms using the straight-line method for depreciation could still differ in terms of the extent to which their assets are understated relative to market values, if one firm estimates residual value more conservatively than the other firm.

[10] A potential limitation of this proxy is that firms may pay high dividends if they have high, free cash flows together with a low level of positive net present value investment opportunities. However, these low-growth opportunities would imply a negative relation between our market-based conservatism proxy and the level of dividends, biasing against finding the hypothesized positive relation.

IV. RESEARCH DESIGN

Conservatism Regressions

We employ the following model, estimated in both levels and changes, to explain cross-sectional variation in conservatism and test H1. The model includes our three proxies for bondholder-shareholder dividend policy conflicts, as well as control variables for profitability and firm size (both proxies for alternative motivations for and costs of conservatism), sales growth, and growth opportunities. (We discuss the control variables in more detail below.) Our dividend policy conflict proxies and control variables are measured as six-year averages, consistent with the estimation of our conservatism proxies:

$$\begin{aligned} CON_i = {} & \beta_0 + \beta_1 STDROA_i + \beta_2\, DIV_i + \beta_3\, LEV_i + \beta_4 ROA_i \\ & + \beta_5\, SIZE_i + \beta_6\, SALESGRO_i + \beta_7\, RNDADV_i + \varepsilon_i \end{aligned} \qquad (2)$$

where:

CON_i = one of two measures of conservatism for firm i:
CONMKT = the market-value-based conservatism proxy, defined as $-\alpha_i$ in equation (1), or
CONACC = the accrual-based conservatism proxy, defined as the mean of total accruals (net income before extraordinary items plus depreciation expense less operating cash flows) multiplied by -1 and deflated by assets;
$STDROA_i$ = the standard deviation of firm i's ROA;
DIV_i = firm i's common dividends divided by its total assets;
LEV_i = firm i's long-term debt divided by its total assets;
ROA_i = firm i's net income before extraordinary items, divided by its total assets (a control for profitability);
$SIZE_i$ = the natural log of firm i's total assets (a control for firm size);
$SALESGRO_i$ = the annual percentage change in firm i's sales (a control for sales growth); and
$RNDADV_i$ = firm i's R&D plus advertising expense, divided by its sales (a control for growth opportunities).

Hypothesis 1 predicts positive coefficients on $STDROA_i$, DIV_i, and LEV_i, our three proxies for the severity of bondholder-shareholder conflicts over dividend policy.

The additional variables in our tests control for (1) profitability, (2) size, (3) sales growth, and (4) growth opportunities. Sensitivity analyses discussed in Section V also control for industry.

Profitability and firm size are potentially associated with alternative motivations for and costs of conservatism, which H1 assumes constant. For low-profitability firms, the reduction in profits associated with conservative accounting would be relatively costly. In other words, more profitable firms can better afford conservative accounting choices. We include ROA in the model to control for this potential cost of conservatism, and we expect firms with higher ROAs to use more conservative accounting. However, note that more negative accruals imply both lower ROA and higher CONACC values. Thus, the mechanical, negative relation between ROA and CONACC could dominate the expected positive relation between conservatism and profitability.

Watts and Zimmerman (1978) argue that firms facing high political costs will tend to use more conservative accounting. Consistent with this argument, Zmijewski and Hagerman (1981) show that large firms use more conservative accounting methods. Accordingly, we include the natural log of total assets in the model to control for this alternative motivation for conservatism. We expect larger firms to make more conservative accounting choices. An additional motivation to control for total assets is that it helps to ensure that any observed positive relation between conservatism and dividends-to-assets or leverage is not induced mechanically by lower asset values in the denominator of the ratios. In other words, the coefficient on DIV is the incremental effect of dividend policy, holding constant the book value of total assets.

We control for sales growth for three reasons: (1) sales growth is likely to affect accruals such as changes in receivables and inventory, and hence CONACC, (2) negative sales growth indicates a decline in operations, under which our CONACC proxy is a poor measure of conservatism (Penman and Zhang 2002), and (3) sales growth may influence the market's expectations of future growth captured in CONMKT (Dechow and Sloan 1997).[11]

We include the sum of R&D and advertising expenses as a control for growth opportunities (positive net present value investment opportunities) potentially captured in CONMKT. However, because our sample is composed of relatively mature firms that are choosing to obtain a bond rating, our sample firms are likely to have more assets-in-place than growth opportunities. Thus, this effect may not be significant for our sample firms.

Cost of Debt Regressions

Hypothesis 2 predicts that firms choosing more conservative accounting will incur lower debt costs. We use the following model, which we estimate in levels and changes, to test this prediction:

$$\text{RATING}_i = \gamma_0 + \gamma_1\text{CON}_i + \gamma_2\text{ROA}_i + \gamma_3\text{LEV}_i + \gamma_4\text{SIZE}_i + \gamma_5\text{BETA}_i + \gamma_6\text{MSE}_i + u_i \quad (3)$$

where:

RATING_i = a numerical transformation of S&P's senior debt rating for firm i over the interval 2 through 21, where larger values correspond to a *less* favorable debt rating, averaged over each six-year estimation period;

BETA_i = the value-weighted market-model beta, obtained from market-model estimates over the six-year period for firm i, for firms with a minimum of 24 monthly returns; and

MSE_i = the mean squared error of the market-model residuals, obtained from market-model estimates over the six-year period for firm i, for firms with a minimum of 24 monthly returns.

All other variables are as defined above.

As discussed previously, our measure of debt cost is a firm's S&P senior debt rating. S&P rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to CCC– (indicating vulnerability to default and dependency on favorable business, financial, and economic conditions to make timely payments of principal and interest). Compustat codes these ratings on a scale from 2 to 21 such that AAA (the best S&P rating) corresponds with 2 and CCC– (the worst rating) with 21. Because lower values of RATING are associated with better debt ratings and lower cost of debt, H2 predicts a negative coefficient on CON, indicating that more conservative firms pay lower debt costs.

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[11] We repeated our analyses after dropping firms with declining sales (negative sales growth) from our analyses. None of our inferences are affected by this sensitivity check.

Equation (3) controls for several other determinants of debt ratings identified in prior research. Kaplan and Urwitz (1979) identify a model of debt ratings often used by subsequent researchers. The firm characteristics they find to be most associated with debt ratings include profitability, firm size, and equity-risk. More specifically, they find that more profitable, larger, less risky firms have better debt ratings. Thus, we include ROA, a leverage ratio (LEV), the log of total assets (SIZE), a measure of systematic equity risk (BETA), and a measure of idiosyncratic equity risk (MSE) in our debt-rating model to control for these firm characteristics that affect debt ratings.[12] We then use this model to assess whether our measures of the firm's conservatism are incrementally associated with the firm's cost of debt, as reflected in its debt rating.

V. RESULTS

Sample and Descriptive Statistics

We draw our sample from the 1998 Annual Industrial Compustat (Primary, Secondary, and Tertiary) files. Our CONMKT conservatism measure derives from the fixed-effects estimation of Equation (1) that requires that each firm have book and market values of equity throughout each six-year period, 1993–1998 and 1987–1992. For each book-to-market firm-year observation, we further require returns (with dividends) for the preceding six years. We then estimate the fixed-effects model for those firms with an S&P senior debt rating available on Compustat during the six-year period. The numbers of firms that meet these data requirements are 730 and 593 in 1993–1998 and 1987–1992, respectively.

Cross-sectional (levels) analyses of conservatism and the cost of debt require additional accounting and market variables as identified in Section IV. We obtain financial-statement data from Compustat, and monthly stock returns for a minimum of 24 months of the six-year sample periods from the 1998 CRSP database. These additional data requirements reduce our sample of firms to 704 in 1993–1998, and 575 in 1987–1992, for our CONMKT analysis.[13] For our CONACC analysis, additional financial statement data requirements reduce our sample to 632 firms in 1993–1998 and 517 firms in 1987–1992. Because we estimate our conservatism measures over six-year windows, we similarly use the firm-specific mean value of each explanatory variable over the same six-year window in our levels specification.

We further investigate these economic relations using a "changes" specification. For each firm, we calculate changes in all variables as the firm-specific mean value for the period 1993–1998 less the firm-specific mean value for the period 1987–1992. This differencing results in the loss of additional firms. Specifically, 568 and 484 firms have sufficient data for the changes specification using CONMKT and CONACC, respectively. Thus, we perform all change analyses on these reduced samples.

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[12] We use STDROA (an accounting measure of operating uncertainty) in the conservatism regression, and market-based measures of uncertainty (MSE) in the cost of debt regressions, because the former better reflects the risk of dividend overpayment whereas the latter better reflects default risk as suggested by option pricing theory.

[13] Overall, our fixed-effects estimation results are similar to Beaver and Ryan (2000); most importantly, the lag coefficients are negative and decline in magnitude monotonically as the lag length increases.

Table 1 reports descriptive statistics for the 1993–1998 period. Untabulated statistics for the 1987–1992 period are very similar.

CONMKT and CONACC are increasing measures of conservatism. That is, larger values of CONMKT indicate a relatively smaller book-to-market ratio, and greater values of CONACC indicate more negative accruals. The mean value of CONMKT for our final

TABLE 1

Descriptive Statistics for the Sample of 704 Firms with Conservatism Estimates, S&P Debt Ratings, and Control Variables 1993–1998

Variable	*Mean*	*Std. Dev.*	*First Quartile*	*Median*	*Third Quartile*
CONMKT	0.002	0.283	−0.146	0.028	0.205
CONACC	0.004	0.031	−0.009	0.003	0.016
RATING	9.936	3.326	7.833	9.833	12.000
STDROA	0.030	0.041	0.007	0.018	0.037
DIV	0.017	0.018	0.003	0.012	0.027
LEV	0.236	0.148	0.122	0.233	0.318
ROA	0.039	0.043	0.015	0.036	0.059
SIZE	8.067	1.518	6.985	7.947	9.085
SALESGRO	0.130	0.210	0.037	0.090	0.178
RNDADV	0.023	0.042	0.000	0.002	0.027
BETA	0.898	0.439	0.609	0.900	1.155
MSE	0.076	0.031	0.054	0.067	0.090

CONMKT = a market-based measure of conservatism; $-\alpha_i$ in Equation (1), which represents the relative firm effect from a regression of book-to-market on current and lagged stock returns, multiplied by −1 to yield an increasing measure of conservatism. Because we estimate CONMKT based on six years of data, the following variables are likewise based on the six-year means;
CONACC = an accrual-based measure of conservatism; the mean ratio of total accruals before depreciation, to total assets, over the six-year period, multiplied by −1 to yield an increasing measure of conservatism (of the 704 firms, 632 had sufficient data to estimate this conservatism measure);
RATING = a numerical transformation of the Standard & Poor's senior debt rating over the interval 2 through 21, where 2 corresponds to AAA and 21 corresponds to CCC–;
STDROA = the standard deviation of ROA (a proxy for operating uncertainty);
DIV = common dividends divided by total assets (a proxy for dividend policy);
LEV = long-term debt divided by total assets;
ROA = net income before extraordinary items, divided by total assets (a control for profitability);
SIZE = the log of total assets;
SALESGRO = the annual percentage change in sales (a control for growth opportunities);
RNDADV = research and development plus advertising expense, divided by sales (a control for growth opportunities);
BETA = the value-weighted market model beta estimated over the six-year period using monthly returns (a control for systematic equity risk); and
MSE = the mean squared error of the market model residuals over the six-year period using monthly returns (a control for idiosyncratic equity risk).

sample is 0.002, which is insignificantly different from zero.[14] CONACC, the accrual-based measure of conservatism, has a mean of 0.004, consistent with negative accruals on average because we have multiplied it by -1.

The mean debt rating for our sample is 9.936, corresponding to an S&P rating between BBB+ and A−. The standard deviation of 3.326 indicates that the sample firms exhibit reasonable variation in debt ratings. The mean of STDROA, our proxy for operating uncertainty, is 0.030, whereas the standard deviation of STDROA is 0.041. The average firm in our sample pays dividends equivalent to 1.7 percent of total assets and has 23.6 percent of its assets levered. The annual growth in sales is approximately 13 percent over the six years. Our sample is comprised of relatively large firms (mean total assets of $11,935 million) with low R&D and advertising (mean RNDADV of 2.3 percent) and low equity risk (mean BETA of 0.898 and MSE of 0.076). Overall, our sample statistics are similar to those of prior studies.

Beaver and Ryan (2000) document that the bias component of book-to-market ratios, our CONMKT, is significantly associated with accelerated depreciation methods and R&D expenditures. In untabulated analysis for both six-year periods, we verify that CONMKT captures accounting practices associated with lower asset valuations and unrecorded intangibles. Specifically, in both six-year periods CONMKT is significantly positively correlated with (1) accumulated depreciation deflated by gross property, plant, and equipment; (2) LIFO reserve deflated by total inventory; and (3) the ratios of R&D expense to sales and of advertising expense to sales.

We do not expect, nor do we find, similar relations between CONACC and these proxies for lower-valued or unrecorded assets for the following reasons. First, we measure CONACC before depreciation, so any relation with accumulated depreciation would be indirect. Second, any relation to the LIFO reserve is unclear because both increases and decreases in inventory (i.e., positive and negative accruals) could be associated with increases in the LIFO reserve. Finally, any relation between the cumulative net change in "accrual" accounts (e.g., the *change* in accounts payable) captured in CONACC, and levels of R&D or advertising expense, is not obvious.

As an alternative, we consider the relation between CONACC and signed special items reported on the income statement. To the extent that special items represent a more discretionary component of net income, we expect CONACC to be associated with more income-decreasing items.[15] The Pearson correlation coefficient between CONACC and special items is −0.348, significant at < 0.01, consistent with the idea that CONACC reflects managerial discretion in special items.

Table 2 presents Spearman (above the diagonal) and Pearson (below the diagonal) correlations among our conservatism measures, debt ratings, bondholder-shareholder dividend policy conflict proxies, and control variables. The two conservatism measures, CONMKT and CONACC, are weakly positively correlated. As discussed in Section III, CONMKT and CONACC are stock and flow measures, respectively. This fundamental

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[14] CONMKT is $-\alpha_i$ in Equation (1), and represents an intercept shift for each firm, incremental to the overall cross-sectional intercept. Thus, it is mean zero by construction in each of the two sample periods. The value reported in Table 1 differs slightly from zero because we estimate the fixed-effects model on the initial sample of 730 firms for the 1993–1998 period.

[15] For example, Moehrle (2002) documents that firms use restructuring charges, commonly coded as special items by Compustat, to accrue discretionary reserves, which the firm later reverses to boost income.

TABLE 2
Univariate Correlations for the Sample of 704 Firms with Conservatism Estimates, S&P Debt Ratings, and Control Variables 1993–1998

Variable[a]	*CONMKT*	*CONACC*	*RATING*	*STDROA*	*DIV*	*LEV*	*ROA*	*SIZE*	*SALESGRO*	*RNDADV*	*BETA*	*MSE*
CONMKT		0.041	−0.380	0.229	0.417	−0.040	0.422	0.130	−0.239	0.432	−0.022	−0.135
		0.306	*0.001*	*0.001*	*0.001*	*0.287*	*0.001*	*0.001*	*0.001*	*0.001*	*0.557*	*0.001*
CONACC	0.124		0.095	0.196	−0.018	0.111	−0.252	−0.008	−0.085	−0.078	−0.027	0.034
	0.002		*0.017*	*0.001*	*0.648*	*0.005*	*0.001*	*0.833*	*0.032*	*0.051*	*0.472*	*0.390*
RATING	−0.366	0.074		0.401	−0.532	0.425	−0.413	−0.523	0.061	−0.033	0.211	0.628
	0.001	*0.063*		*0.001*	*0.001*	*0.001*	*0.001*	*0.001*	*0.106*	*0.378*	*0.001*	*0.001*
STDROA	0.107	0.406	0.399		−0.081	0.149	0.051	−0.365	−0.139	0.403	0.225	0.589
	0.005	*0.001*	*0.001*		*0.031*	*0.001*	*0.178*	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*
DIV	0.372	0.083	−0.464	−0.058		−0.009	0.512	0.072	−0.253	0.083	−0.502	−0.571
	0.001	*0.037*	*0.001*	*0.124*		*0.808*	*0.001*	*0.058*	*0.001*	*0.028*	*0.001*	*0.001*
LEV	−0.003	0.076	0.433	0.106	−0.060		−0.185	−0.334	−0.054	−0.113	−0.225	0.121
	0.930	*0.057*	*0.001*	*0.005*	*0.114*		*0.001*	*0.001*	*0.155*	*0.003*	*0.001*	*0.001*
ROA	0.330	−0.342	−0.440	−0.365	0.434	−0.271		−0.057	0.067	0.345	−0.140	−0.201
	0.001	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*		*0.130*	*0.076*	*0.001*	*0.001*	*0.001*
SIZE	0.128	−0.041	−0.554	−0.336	0.013	−0.322	0.059		−0.087	0.011	0.083	−0.419
	0.001	*0.308*	*0.001*	*0.001*	*0.741*	*0.001*	*0.116*		*0.020*	*0.781*	*0.027*	*0.001*
SALESGRO	−0.153	0.048	0.131	0.198	−0.142	0.032	−0.160	−0.146		−0.144	0.183	0.161
	0.001	*0.229*	*0.001*	*0.001*	*0.001*	*0.399*	*0.001*	*0.001*		*0.001*	*0.001*	*0.001*
RNDADV	0.297	0.027	−0.112	0.150	0.157	−0.175	0.370	0.030	−0.056		0.178	0.201
	0.001	*0.503*	*0.003*	*0.001*	*0.001*	*0.001*	*0.001*	*0.434*	*0.137*		*0.001*	*0.001*
BETA	0.012	−0.021	0.223	0.224	−0.412	−0.134	−0.106	0.066	0.063	0.159		0.475
	0.759	*0.596*	*0.001*	*0.001*	*0.001*	*0.001*	*0.005*	*0.082*	*0.095*	*0.001*		*0.001*
MSE	−0.183	0.102	0.662	0.585	−0.436	0.197	−0.325	−0.459	0.316	0.101	0.443	
	0.001	*0.010*	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*	*0.008*	*0.001*	

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[a] See Table 1 for variable definitions.

Spearman (Pearson) correlations are above (below) the diagonal; p-values (two-tailed) are in italics.

difference likely contributes to the low univariate correlation between CONMKT and CONACC.[16]

Consistent with H1, CONMKT is significantly positively correlated with two of the three proxies for bondholder-shareholder dividend policy conflicts (STDROA and DIV). However, contrary to H1, CONMKT is not significantly correlated with leverage (LEV), the third proxy. The Pearson correlations between the accrual-based measure of conservatism, CONACC, and all three proxies—STDROA, DIV, and LEV—are positive, consistent with H1. The Spearman correlations between CONACC and two out of the three proxies—STDROA and LEV—are positive, also consistent with H1.

Hypothesis 2 predicts that conservatism reduces the cost of debt. The univariate correlations between CONMKT and RATING support H2, as the Pearson and Spearman correlations are –0.366 and –0.380, respectively, both with p-values < 0.01. In contrast, the correlation between CONACC and RATING is positive, contrary to H2. However, these univariate correlations do not evaluate the incremental effect of conservatism on debt ratings after controlling for other determinants of debt ratings, including profitability.

Conservatism and Bondholder-Shareholder Conflicts over Dividend Policy

Table 3 reports our tests of H1. Panel A presents the results of regressions with the market-based conservatism measure (CONMKT) as the dependent variable, whereas Panel B presents the results of regressions with the accrual-based measure of conservatism (CONACC). The first two columns of Panel A present results of estimating Equation (2) using levels data consisting of the mean firm-specific value for each variable, for the periods 1993–1998 and 1987–1992, respectively. Because the main results are similar for both time periods, the following discussion focuses on the 1993–1998 period.

Consistent with H1, the coefficient on STDROA is positive, 2.079 (p = 0.001). This suggests that firms with greater operating uncertainty adopt more conservative accounting. Similarly, consistent with H1, the coefficient on DIV is significantly positive, 3.557 (p = 0.001). LEV, with a coefficient of 0.316, is significant in the more recent sample period but exhibits a weaker relation in the earlier period.[17] Collectively, these results suggest that conservatism plays an important role in efficient debt contracting.[18]

Consistent with prior research, firm profitability (ROA) and size (SIZE) are significantly positively related to our conservatism proxies (p = 0.001) in the later time period. In the earlier time period, however, the coefficient on SIZE is insignificant.

If CONMKT also reflects growth opportunities, then it should be positively related to sales growth and to R&D and advertising expenditures. Table 3, Panel A shows that

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[16] The low univariate correlation is also partially attributable to not controlling for other determinants of CONMKT and CONACC, in particular common determinants with contrasting relations. For example, the Pearson correlation between ROA and CONMKT is 0.330. In contrast, the correlation between ROA and CONACC is −0.342. After controlling for this common determinant, we find that the partial Pearson correlation between CONMKT and CONACC increases to 0.259.

[17] Beaver and Ryan (2000) find a positive relation between leverage, defined as total liabilities deflated by the market value of equity, and the bias component of book-to-market ratio. Use of market value of equity as the denominator of both leverage and the book-to-market ratio can lead to a mechanical, positive relation between leverage and the book-to-market ratio. To avoid this possibility, we define leverage as long-term debt divided by total assets. When we use market value of equity instead of total assets as the deflator in defining leverage, we obtain results consistent with Beaver and Ryan (2000).

[18] We consider whether the positive relation between CONMKT and leverage might arise because conservative accounting decreases the book value of equity (thus increasing CONMKT) and decreases total assets (thus increasing long-term debt to total assets). However, the insignificant Pearson correlation between CONMKT and total assets of −0.017 (p = 0.66) suggests that this is unlikely. Further, controlling for the log of total assets (SIZE) in our models reduces the likelihood of a mechanical relation induced through the effects of conservatism on assets.

CONMKT is positively related to RNDADV. However, CONMKT is negatively related to SALESGRO. Although inconsistent with our initial expectations, the negative coefficient on SALESGRO may be due to a relation between realized growth and book-to-market ratios. Greater realized growth increases the proportion of new to old assets. This reduces the disparity between the book and market values for newer assets, which in turn leads to lower estimated conservatism. This effect may dominate the ability of SALESGRO to proxy for expected future growth.

TABLE 3

Summary Statistics from Regressions of Accounting Conservatism on Proxies for Bondholder-Shareholder Conflicts and on Control Variables

$$CON_i = \beta_0 + \beta_1\, STDROA_i + \beta_2\, DIV_i + \beta_3\, LEV_i + \beta_4\, ROA_i + \beta_5\, SIZE_i + \beta_6\, SALESGRO_i + \beta_7\, RNDADV_i + \varepsilon_i$$

Panel A: Dependent Variable—Market-Based Measure of Accounting Conservatism, CONMKT

Variable[a]	*Predicted Sign*	*All Variables Measured in Levels (1993–1998)*	*All Variables Measured in Levels (1987–1992)*	*All Variables Measured In Changes from 1993–1998 Period vs. 1987–1992 Period*
Intercept	?	−0.644 (0.001)	−0.318 (0.005)	0.001 (0.975)
Proxies for Bondholder-Shareholder Conflict:				
STDROA	+	2.079 (0.001)	1.692 (0.001)	1.395 (0.001)
DIV	+	3.557 (0.001)	2.563 (0.001)	3.472 (0.001)
LEV	+	0.316 (0.001)	0.178 (0.117)	0.309 (0.001)
Control Variables:				
ROA	+	2.060 (0.001)	2.900 (0.001)	2.482 (0.001)
SIZE	+	0.045 (0.001)	0.008 (0.227)	0.022 (0.005)
SALESGRO	+	−0.125 (0.942)	−0.143 (0.901)	−0.073 (0.813)
RNDADV	+	0.790 (0.001)	1.108 (0.001)	1.437 (0.001)
Sample Size		704	575	568
Adjusted R^2		29.2%	23.1%	25.6%

(*Continued on next page*)

TABLE 3 (Continued)

Panel B: Dependent Variable—Accrual-Based Measure of Accounting Conservatism, CONACC

Variable[a]	*Predicted Sign*	*All Variables Measured in Levels (1993–1998)*	*All Variables Measured in Levels (1987–1992)*	*All Variables Measured In Changes from 1993–1998 Period vs. 1987–1992 Period*
Intercept	?	−0.012 (0.154)	−0.014 (0.156)	0.000 (0.877)
Proxies for Bondholder-Shareholder Conflict:				
STDROA	+	0.216 (0.001)	0.150 (0.008)	0.157 (0.001)
DIV	+	0.404 (0.001)	0.197 (0.025)	0.295 (0.001)
LEV	+	0.001 (0.450)	0.000 (0.494)	−0.014 (0.901)
Control Variables:				
ROA	?	−0.241 (0.001)	−0.278 (0.001)	−0.270 (0.001)
SIZE	+	0.001 (0.040)	0.003 (0.001)	0.001 (0.064)
SALESGRO	−	−0.003 (0.246)	−0.024 (0.024)	−0.017 (0.022)
RNDADV	?	0.051 (0.087)	−0.003 (0.904)	0.053 (0.102)
Sample Size		632	517	484
Adjusted R^2		24.9%	24.2%	22.5%

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[a] See Table 1 for variable definitions.

Variables in the levels specifications are the firm-specific mean values for the periods 1993–1998 and 1987–1992. Variables in the changes specification are the firm-specific mean value for the period 1993–1998 less the mean value for the period 1987–1992. White (1980) corrected p-values (one-tailed where sign is predicted) appear in parentheses.

The last column of Table 3, Panel A, presents the results of estimating Equation (2) using a change specification with all variables calculated as the difference between the firm-specific mean value for the period 1993–1998 and the firm-specific mean value for the period 1987–1992. All results are consistent with those reported for the levels specification. Specifically, firms experiencing increased operating uncertainty, dividends, and leverage are associated with significantly increasing levels of accounting conservatism.

Overall, even after controlling for profitability, size, sales growth, and R&D and advertising expenditures, we find that accounting conservatism, as measured by CONMKT, is significantly increasing in proxies for bondholder-shareholder conflicts over dividend policy, in both levels and changes specifications.

Table 3, Panel B presents the results of estimating Equation (2) with the accrual-based conservatism measure, CONACC, as the dependent variable. Consistent with H1, coefficients on two of the proxies for bondholder-shareholder conflicts, STDROA and DIV, are significantly positive in both periods and in the changes specification. In contrast with H1, LEV is insignificant in both periods and in the changes specification. These results suggest that accounting conservatism, as captured in cumulative (signed) accruals, is associated with bondholder-shareholder conflicts stemming from profit volatility and dividend policy, but not from leverage.[19]

In Panel B, we expect a negative sign on sales growth, because higher sales growth likely increases current accruals, which in turn reduces CONACC. Furthermore, we do not predict the sign of the coefficients on RNDADV because *a priori* it is unclear how R&D and advertising might affect CONACC; the fact that a firm expenses R&D or advertising should not affect the level of its accruals. We also cannot predict the sign of the coefficient on ROA because there are two offsetting effects of ROA on CONACC. On the one hand, we expect more profitable firms to make more conservative accounting choices. On the other hand, more negative accruals would, *ceteris paribus*, reduce ROA and increase CONACC values.

Panel B shows that ROA has a negative coefficient, consistent with negative accruals both reducing ROA and increasing CONACC as discussed above. As expected, SIZE has a positive coefficient—larger firms record more conservative accruals, on average. The coefficient on SALESGRO is significantly negative in the earlier period and in the changes specification. This is consistent with high sales growth leading to large increases in accounts receivable and/or inventory, which reduce CONACC. The coefficient on RNDADV is not significant at conventional levels in either period of the levels analysis or in the changes specification.

In summary, the evidence reported in Table 3 suggests that accounting conservatism increases with firm characteristics that proxy for greater bondholder-shareholder conflicts over dividend policy. This result is generally robust to our two distinct measures of conservatism. CONMKT is associated with our proxies for operating uncertainty, level of dividends (as a percentage of assets), and leverage. CONACC is associated with our proxies for operating uncertainty and the level of dividends, but not leverage.

Conservatism and the Cost of Debt

Table 4 reports our test of H2, which posits that conservatism is associated with a lower cost of debt. The first two columns of Table 4, Panels A and B, report results of estimating Equation (3) using an ordered-logit model based on the firm-specific levels data for the two time periods. Because RATING is a categorical dependent variable, the basic assumptions of OLS are violated. The ordinal nature of RATING allows us to estimate Equation (3) as an ordered-logit model, which assumes that a discrete debt rating corresponds to the value of an unobservable, continuous, latent index, such as credit risk.

Because the inferences are similar for both periods, we discuss the more recent 1993–1998 period results. Consistent with H2, the coefficient on CONMKT is negative, –2.348

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[19] In an earlier version of this paper, we measured CONACC using accruals inclusive of depreciation, which exhibited a significant positive relation with LEV. Based on unreported analyses, we attribute that relation to asset intensity increasing both LEV and the depreciation charge. We thank an anonymous reviewer for pointing this out.

($p < 0.001$), suggesting that firms using more conservative accounting (as evidenced by book values persistently below market values) have significantly more favorable debt ratings, and in turn, significantly lower debt costs. In addition, all control variables have the expected signs and are significant. The pseudo-R^2 is 67.1 percent, indicating that the model explains a substantial amount of the variation in firms' debt costs.

The last column in Table 4 presents the results for Equation (3) with all variables measured in changes. This model tests for a relation between changes in conservatism and changes in firms' cost of debt. Again, the results are consistent with the levels specification. The coefficient on the change in CONMKT is −1.233 ($p < 0.001$) indicating that firms with higher conservatism also have more favorable debt ratings, and in turn, lower debt costs. The sign and significance of the control variables are also similar to the results reported for the levels specification except that the coefficient on BETA is not significant.

TABLE 4
Summary Statistics from Ordered-Logit Tests of Association between the Cost of Debt and Accounting Conservatism, after Controlling for Other Determinants of Debt Ratings

$$RATING_i = \gamma_0 + \gamma_1 CON_i + \gamma_2 ROA_i + \gamma_3 LEV_i + \gamma_4 SIZE_i + \gamma_5 BETA_i + \gamma_6 MSE_i + \varepsilon_i$$

Panel A: CON Variable—Market-Based Measure of Accounting Conservatism, CONMKT

Variable[a]	*Predicted Sign*	*All Variables Measured in Levels (1993–1998)*	*All Variables Measured in Levels (1987–1992)*	*All Variables Measured In Changes from 1993–1998 Period vs. 1987–1992 Period*
Proxy for Accounting conservatism:				
CONMKT	−	−2.348	−1.372	−1.233
		(0.001)	(0.001)	(0.001)
Control Variables:				
ROA	−	−13.725	−23.209	−13.930
		(0.001)	(0.001)	(0.001)
LEV	+	5.133	5.905	4.328
		(0.001)	(0.001)	(0.001)
SIZE	−	−0.565	−0.770	−0.553
		(0.001)	(0.001)	(0.001)
BETA	+	0.667	1.217	0.062
		(0.001)	(0.001)	(0.384)
MSE	+	38.123	15.233	5.171
		(0.001)	(0.001)	(0.019)
Sample Size		704	575	568
Pseudo R^2		67.1%	71.3%	62.8%

(Continued on next page)

TABLE 4 (Continued)

Panel B: CON Variable—Accrual-Based Measure of Accounting Conservatism, CONACC

Variable[a]	*Predicted Sign*	*All Variables Measured in Levels (1993–1998)*	*All Variables Measured in Levels (1987–1992)*	*All Variables Measured In Changes from 1993–1998 Period vs. 1987–1992 Period*
Proxy for Accounting conservatism:				
CONACC	–	−6.320 (0.006)	−3.411 (0.092)	−5.486 (0.003)
Control Variables:				
ROA	–	−18.238 (0.001)	−26.647 (0.001)	−18.003 (0.001)
LEV	+	4.786 (0.001)	5.627 (0.001)	3.900 (0.001)
SIZE	–	−0.584 (0.001)	−0.687 (0.001)	−0.564 (0.001)
BETA	+	0.354 (0.031)	1.051 (0.001)	0.012 (0.479)
MSE	+	40.974 (0.001)	17.680 (0.001)	8.189 (0.001)
Sample Size		632	517	484
Pseudo R^2		64.6%	70.0%	59.7%

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[a] See Table 1 for variable definitions.

Variables in the levels specification are the firm-specific mean values for the period 1993–1998 and 1987–1992. Variables in the changes specification are the firm-specific mean value for the period 1993–1998 less the mean value for the period 1987–1992. Models are estimated using Ordered Logit; Z-score p-values in parentheses (one-tailed where sign is predicted). RATING is coded so that a positive coefficient indicates the variable is associated with less favorable debt ratings, and a negative coefficient indicates the variable is associated with more favorable debt ratings.

The coefficients on CONACC in Panel B are also negative in every specification, consistent with H2. The coefficient is significant at the 1 percent level in the later period and at the 10 percent level in the earlier period. We also observe a significant negative relation between CONACC and debt costs in the changes specification. Thus, the evidence suggests that conservative accounting is associated with more favorable debt ratings, and thus lower debt costs, after controlling for profitability, leverage, firm size, and equity risk.

In summary, the evidence presented in Tables 3 and 4 suggests that conservatism helps mitigate bondholder-shareholder conflicts over dividend policy and thereby reduces firms' cost of debt. The collective evidence is consistent with the notion that accounting conservatism plays an important role in efficient debt contracting.

Robustness

In this section we discuss the results of additional robustness checks pertaining to (1) industry controls, (2) analyses of subsamples based on dividend payment or nonpayment, and (3) sensitivity to extreme values.

We repeat our analyses in Tables 3 and 4 using industry-adjusted measures. We measure all variables as deviations from the industry mean, with industry defined as those firms in the sample that have the same two-digit Standard Industrial Classification (SIC) code. Industry-adjusted variables help control for GAAP-mandated conservatism and other economic characteristics (such as growth) that likely vary across industries. All of our conclusions about the relation between conservatism and the severity of bondholder-shareholder conflicts over dividend policy are also supported by the industry-adjusted analyses. Similarly, the industry-adjusted measures confirm our conclusion that firms choosing more conservative accounting incur a lower cost of debt.

Next, we examine subsamples of firms based on whether they paid dividends. Bondholder-shareholder conflicts over dividend policy for firms that do not pay dividends are likely to be minimal. However, the threat of dividend initiation could still motivate the use of conservative accounting. Analyzing these subsamples provides evidence on the potential role of conservatism in controlling for the risk of dividend overpayment associated with the threat of dividend initiation. We identify 151 firms that did not pay dividends during the sample period. When we exclude these firms from the analyses, all inferences and conclusions remain identical to those reported in the paper. When we performed the analysis on only those firms that did not pay dividends, the results were generally consistent (despite the much smaller sample size) with those reported for the broader sample, except that the relation between debt cost and CONACC in the earlier period was not significant at conventional levels. This suggests that even if firms do not currently pay dividends, the threat that they may initiate dividends provides incentives to mitigate bondholder-shareholder conflicts over (future) dividend policy.

Finally, to ensure that our results are not driven by extreme values, we estimated the models in Tables 3 and 4 after deleting observations deemed outliers as defined by a Cook's D-statistic with a p-value less than or equal to 0.15. Results are consistent with those reported, indicating that influential observations are not driving the results.

VI. CONCLUSION

This study provides evidence suggesting that accounting conservatism helps: (1) mitigate bondholder-shareholder conflicts over dividend policy, and (2) reduce firms' debt costs. We use two alternative measures of conservatism: (1) a market-based measure (based on Beaver and Ryan [2000]), and (2) an accrual-based measure (based on Givoly and Hayn [2000]). We find that both measures are significantly positively correlated with two of the three proxies for bondholder-shareholder conflicts over dividend policy: standard deviation of ROA and the dividend-to-asset ratio. The evidence on leverage, our third proxy is mixed. Overall, we conclude that firms facing more severe bondholder-shareholder conflicts over dividend policy choose more conservative accounting.

Furthermore, we document that firms using more conservative accounting have more favorable debt ratings and so incur a lower cost of debt, after controlling for other determinants of the debt costs. Taken together, the evidence is consistent with accounting conservatism playing an important role in efficient debt contracting.

Our inferences are subject to several limitations. First, because the conservatism that managers voluntarily choose is not directly observable, our proxies potentially reflect both GAAP-mandated conservatism as well as managers' choices among more or less conservative accounting methods, estimates, and assumptions. However, it is unlikely that our results are driven by cross-sectional variation in GAAP-mandated conservatism because: (1) even GAAP-mandated conservatism requires managers to exercise discretion, (2) our two different proxies yield similar results even though they are likely to be affected differently by GAAP-mandated conservatism, (3) we find similar results with industry-adjusted measures that control for the effect of mandated conservatism that varies systematically across industries, and (4) our results hold in a changes specification, which further controls for the differential effects of GAAP-mandated conservatism on individual firms.

Second, our conservatism proxies are potentially affected by growth opportunities and operating asset growth. We control for these factors directly, with control variables, as well as by using industry-adjusted measures that control for industry-specific growth.

Third, we focus exclusively on the role of accounting conservatism in mitigating bondholder-shareholder conflicts over dividend policy. There are other mechanisms for resolving these conflicts, such as secured debt. Furthermore, there are other conflicts between bondholders and shareholders (e.g., over investment and financing policies). Conservatism may also play a role in mitigating these other conflicts. We leave analysis of these other mechanisms and other bondholder-shareholder conflicts for future research.

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Assessing Investor Response to Information Events Using Return and Volume Metrics

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ABSTRACT: Prior research addressing questions such as whether investors respond to a hypothesized information event used tests of unusual return and/or trading activity as alternative measures of investor response. We investigate which of these two metrics maximizes the likelihood that a researcher correctly detects the presence or absence of a response. Building on the repeated-sample framework established in Brown and Warner (1980, 1985) and Dyckman et al. (1984), we provide evidence that (1) volume-based metrics, especially measures based on numbers of transactions, provide more powerful tests of investor response to public disclosures than do return-based metrics; and (2) supplementing return-based measures with trading-based measures increases the power of tests designed to detect investor response. Our conclusions are particularly relevant when power is critical (i.e., when sample sizes are small or anticipated investor response is small). Our evidence also suggests that before concluding that investors do not respond to a public disclosure, based on a returns analysis, researchers should confirm the nonresponse inference with trading-based measures.

Keywords: *absolute returns; event study; information content; unexpected return; volume.*

Data Availability: *Data are from public sources as described in the text.*

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I. INTRODUCTION

Empiricists interested in testing whether accounting disclosures convey incremental new information to the market typically use return-based event-study designs, which Beaver (1968) introduced into the accounting literature. Beaver also introduced volume-based event-study designs, which are less common even though such tests yield insight into the extent to which announcements of accounting information prompt individual investors to act (i.e., to trade). Of course, researchers should use the metric that is conceptually most appropriate to the research question at hand. When the question of interest is whether information is associated with higher or lower prices (e.g., whether managers are inflating valuations by "hyping" stocks, as in Lang and Lundholm [2000]), then the associated design should employ directional price-based metrics (returns). If the question of interest is whether information directly affects trading decisions (e.g., Linsmeier et al.'s [2002] analysis of how market-risk disclosures dampen trading responses), then the associated design should use trading volume-metrics. However, in many cases, the focal question is simply whether investors respond to some disclosure. Large absolute returns or high volume of trading in a short window immediately surrounding the release of data suggest that the disclosed data *affect*, and are not simply associated with, investor decisions. Our study provides evidence on whether a return-based or trading-based metric leads to more powerful tests of investor response to information events.

Several recent studies focus on detecting the existence or nonexistence of short-window investor response to accounting-related information including: research and development spending disclosures (Boone and Raman 2002); 8-K filings (Carter and Soo 1999); earnings announcements and information released contemporaneously in the same disclosure (Francis et al. 2002); earnings announcements (Landsman and Maydew 2002); EDGAR 10-K filings (Qi et al. 2000); and real estate trust funds flow information (Vincent 1999). Of particular relevance to our analysis are studies that do not detect a statistically significant investor response to a hypothesized information event. Although the literature contains notable (widely cited) nonresponse findings such as Bernard and Stober's (1989) documentation of a nonresponse to cash flow information, and the absence of market response to various types of qualified audit opinions reported in Dodd et al. (1984), a more common outcome of a nonresponse "discovery" is an unpublishable or unfinished analysis.[1] Our analysis bears most directly on nonresponse settings because the reliability of such null "conclusions" is directly related to the underlying power of the research design.

Most short-window response studies use daily returns as their primary response metric. Variation in daily returns, however, is sizable, commonly as much as 4 percent of a firm's equity value. In the presence of such high variance, it is difficult to detect investor responses to information events that have modest valuation effects. Indeed, Cready and Mynatt (1991) document that even in sample sizes of near 600, the likelihood of detecting a half-percent shift in firm value occurring over a three-day event window is only about 60 percent, and they achieve this likelihood only by using an aggressive 20 percent Type I error rate (i.e., a 20 percent chance of rejecting the null hypothesis when it is true). The inherent noisiness of return metrics suggests investigating whether volume-based metrics increase the likelihood of correctly rejecting null hypotheses in low-power contexts. We examine this issue by evaluating return-based and volume-based metrics in earnings announcement periods.

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[1] Greenwald (1976), Lindsay (1994), and Bamber et al. (2000) discuss the peer review process's inherent bias against studies that fail to reject the null hypothesis.

Assuming that the null hypothesis—that the mean investor response to earnings announcements is zero—is false for the population of earnings announcements,[2] this analysis yields insight into the relative power of tests based on returns *vis-à-vis* volume in detecting investor responses to earnings announcements.

Prior research on event-study methods identifies which of several alternative return response metrics (Brown and Warner 1980, 1985; Rohrbach and Chandra 1989), or which of several alternative volume response metrics (Cready and Ramanan 1991, 1995), is most likely to correctly reject the null hypothesis of no investor response to information events when the null hypothesis is false. In contrast to these prior studies that focused on *either* return-based *or* volume-based measures, we compare return- and volume-based metrics. That is, given a null hypothesis that information events lead to no investor response, do return-based or volume-based metrics provide the most powerful test of this null? We address this question using a framework similar to Brown and Warner (1980, 1985) and Dyckman et al. (1984). Specifically, from the grand sample of earnings announcements, we form many relatively small subsamples of earnings announcements, and then we empirically estimate each metric's sample-size-specific rejection rate. We find that in earnings announcement periods, where we *assume* that the null hypothesis of no reaction to earnings announcements is false, rejection rates are significantly higher for tests using volume-based metrics than for similar tests using return-based metrics.

Volume and return are complementary measures that capture different aspects of investor response to information events. Bamber and Cheon (1995) suggest that the relation between volume and return responses to earning announcements is closer to independence than it is to a "strong positive relation." Kandel and Pearson (1995) report that trading volume around earnings announcements is, on average, abnormally high even for earnings announcements that stimulate negligible return response. If volume and return measures are sufficiently complementary, then tests of market response based on both metrics should be more precise than tests based on either metric alone. We find that supplementing a return-based analysis with a volume-based analysis increases the likelihood of correctly rejecting the null hypothesis of investor nonresponse. The converse, however, is not true. Hence, our analysis suggests that when the focal research question is determining the presence or absence of an investor response to information events, and when power is critical (e.g., small sample or anticipated modest investor response), volume-based designs outperform return-based designs. Moreover, our results suggest that research concluding that there is no investor response to information events (i.e., that the information event does not convey incremental new information) would be more reliable if confirmed by evidence of insignificant abnormal trading volume, particularly if power is crucial.

In our evaluation of alternative price-response metrics, we also find that nondirectional return metrics outperform directional tests that condition the return sign on the earnings surprise. That is, when information on the likely direction of the return response to information events is noisy, simply ignoring this directional information leads to more powerful tests of investor response or nonresponse than do signed returned metrics. Because directional tests based on perfect knowledge of the direction of the return are more powerful than their nondirectional price-based competitors (Brown and Warner 1985), our results suggest that conventional earnings expectation measures suffer from substantial measurement error.

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[2] Despite the evidence in Bamber et al. (2000) that most earnings announcements do not appear to stimulate investor responses, our assumption that mean investor response is not zero is reasonable—as long as the responses are not zero for at least one earnings announcement, the mean response is nonzero.

II. RESEARCH DESIGN

Most studies addressing the power of alternative market-response metrics seed existing values of the metric with specific permutations and then examine the ability of various statistical methods to detect the permutation correctly (by rejecting the null that no permutation is present). Unfortunately, such an approach is not feasible in cross-metric comparisons because a 1 percent return effect does not equal a 1 percent volume effect (or any other identifiable permutation of the volume effect). We address this problem by using earnings announcements, which we *assume* stimulate nonzero mean investor response (see footnote 2), as contemporaneous implicit return and volume seeds.

Limitations of Using Announcement-Defined Investor Responses

Using return and volume responses to actual earnings announcements leads to several limitations. First, we do not explicitly manipulate the magnitude of the return and volume inducements, so it is not possible to report rejection rates for 1 percent inducements, 2 percent inducements, and so forth, because earnings announcement returns and volumes implicitly define the inducements in a manner that the researcher cannot manipulate. However, we can probe rejection rate properties by varying sample size. The cross-sectional t-test statistic of the null hypothesis that the mean shift, U, equals zero is:

$$t = (n^{1/2})\, U/\sigma.$$

Since the numerator in this expression is the product of U, the mean shift, and n, the sample size, changing n holding U constant (assuming that U is nonzero) has the same directional effect on t as changing U holding n constant. Hence, in this context, observing how rejection rates behave when varying n is isomorphic to observing their behavior when varying U.

The largest sample size we examine is 40. Although studies employing sample sizes of such small magnitude are uncommon, they are far from rare. In an analysis of event-study sample sizes employed in papers published in *The Accounting Review* during the years 1992–2001, we identified 35 studies employing short-window response metrics. Of these, 16 reported at least one substantive analysis of a subsample or data partition containing fewer than 50 observations.[3] Moreover, the fungibility of the sample size and mean shift effects in the above t-statistic calculation allows us to extrapolate our small sample size results to larger samples. For example, if we expect that the mean investor response to the announcement of firms' monthly sales data is a proportion p, say 20 percent, of the population earnings announcement response effect (U), then we can extrapolate the results reported here for a sample size of n earnings announcements to a sample size of $n(1/0.20)^2$ for monthly sales announcements for which the mean investor response is 0.2U. Here, for instance, the results for a sample of 40 earnings announcements would correspond to the results of a sample of 1,000 ($40[1/0.20]^2$) monthly sales announcements.[4]

A second, related limitation is that because the earnings-announcement-period data identify the magnitudes of the underlying mean return and volume responses, our results directly pertain only to contexts in which the ratio of return to volume response is similar to that at earnings announcement dates. In other contexts where the ratio of return-to-volume response is greater than that for earnings announcements, the relative performance of volume-based tests should be lower than that reported here.

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[3] Details of this analysis are available from the authors.

[4] This correspondence holds exactly when the event does not affect the estimated standard deviation (σ), such as when tests estimate the standard deviation over a nonannouncement estimation period. In contrast, in tests that use the event period to estimate the standard deviation, the sample size necessary to yield an equivalent rejection rate level changes directly with the degree to which the event affects the estimated standard deviation. If the percentage impact on the estimated standard deviation is similar for return and volume metrics, then this error will not materially affect our inferences about differential performance across metrics.

A third limitation is that the analysis assumes that earnings announcements cause population-wide price and trading responses. However, Kandel and Pearson (1995) find that approximately 10 percent of their sample of earnings announcements stimulate no significant price movement in the three days around the earnings announcement date. Bamber et al. (2000) suggest that prior research's conclusion that the return response is associated with earnings surprises is largely attributable to a relatively small subpopulation of earnings announcements. Such findings suggest that the earnings announcement population may consist of a mixture of unresponsive and responsive groups.

If the percentage of earnings announcements that stimulate price responses differs substantially from the percentage of earnings announcements that stimulate trading, then, *ceteris paribus*, the likelihood of rejecting the null hypothesis should be greater for the more broadly responsive metric than for the less broadly responsive metric.

At a purely unconditional level, the fact that differential responsiveness can explain any observed difference in rejection rates is irrelevant to our tests. Our analysis allows mean shifts to differ between the returns *vis-à-vis* volume metrics, so any responsiveness differences affect these mean shifts. That is, the overall population mean shift (U) is a function of *both*: (1) the percentage of the population that is responsive with respect to a given metric, q, and (2) the mean of the responsive subpopulation, U^r, because $U = qU^r$. Thus, although an increase in response rate q necessarily increases U, an increase in U could be attributable to *either*: (1) a stronger reaction in the responsive subpopulation, *or* to (2) an increase in the response rate, q. Hence, our analysis does not provide direct evidence on relative cross-metric response rates (i.e., q's).

Differential responsiveness might matter, however, when researchers screen out likely zero response observations to increase the power of their tests, as Bamber et al. (2000) suggest occurs in Beaver (1968). In such cases the underlying mean for the post-screen subpopulation approaches U^r, which necessarily exceeds U. Effective screens should lead to higher rejection rates than reported here.[5] The generalizability of our cross-metric inferences depends on whether the research design differentially screens out zero response observations across return vs. volume metrics. If a screen eliminates similar numbers of zero (or near-zero) response observations for both metrics, then the power associated with each metric increases, but any differences in power across the two remain intact.

Response Metrics

We consider four distinct event-period response metrics: (1) percentage of outstanding shares traded, (2) number of transactions, (3) absolute return, and (4) return conditioned on the direction of the earnings news.

Beaver (1968) introduces the percentage of outstanding shares traded as a measure of market response. The specific measure we use is:

$$VSHRS_{it} = (V_{it} - EV_{it})/\sigma_{Vi}$$

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[5] Since screens reduce power by reducing the sample size, a strict improvement in power is guaranteed only if the screen never erroneously eliminates a responsive observation.

where:

V_{it} = the log of (100 times shares traded in firm i's stock on day t divided by the outstanding shares for firm i on day t, plus 0.000255);[6]
σ_{Vi} = the standard deviation of the residuals from the market-volume regression used to determine EV;[7] and
EV_{it} = the predicted level of V_{it} from a first-order serial correlation regression of V_{it} on V_{MKT} where V_{MKT} is the percentage of outstanding shares traded for all NYSE-listed firms in CRSP, estimated over days −55 to −6 and +6 to +55 relative to the announcement date.[8]

Cready and Ramanan (1991) indicate that market-adjusted volume outperforms simple mean-adjusted volume in detecting seeded trading increases. The serial correlation regression is similar to the methods Pincus (1983) and Cready and Mynatt (1991) used to correct for the strong positive serial correlation in daily volume. In addition, following Cready and Ramanan (1995), we use only the structural portion of the model to generate EV values in non-initial event test period days (e.g., days 0 and +1 when the test period is days −1 to +1).

Cready and Ramanan (1995) find that tests based on the number of trades, or transactions, outperform tests based on percentage of shares traded in detecting both seeded and announcement-period trading effects. Data on the number of transactions are now available in the ISSM and Trades and Quotes (TAQ) databases. Hence, we also consider a transactions-based metric:

$$VTRAN_{it} = (TR_{it} - PTR_{it})/\sigma_{TRi}$$

where:

TR_{it} = the log of (number of transactions in firm i's stock on day t plus 1);[9]
PTR_{it} = the predicted level of TR_{it} from a first-order serial correlation regression of TR on TR_{MKT} where TR_{MKT} is based on the number of transactions for all NYSE firms in the ISSM database on that day; and
σ_{TRi} = the standard deviation of the residuals from the market-transactions regression used to determine PTR.

May (1971) introduces absolute returns as a measure of market response to interim earnings announcements. Subsequent analysis by Rohrbach and Chandra (1989), Cready and Mynatt (1991), and Subramaniam (1995) indicate that absolute return is more powerful than Beaver's (1968) squared return metric in identifying unconditional price responses. In untabulated analyses, we confirm that absolute returns outperform squared returns in detecting announcement period price responses. The specific absolute-return response metric we employ is:[10]

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[6] The log-transformation mitigates the skewness in the volume distribution (Ajinkya and Jain 1989). To avoid taking the log of zero we add the constant (0.000255) following Cready and Mynatt (1991) and Richardson et al. (1986).

[7] We do not use the standard deviation of the predicted values here or in the subsequent transactions-based metric because the calculation of such deviations adds a great deal of computational complexity. Moreover, because the number of estimation-period observations is large and mean market-level trading is unlikely to differ much from the mean level of trading in the estimation period, the standard deviation of the residuals is unlikely to differ much from the standard deviation of the prediction errors.

[8] We use before- and after-event estimation periods because trading increases with time (Cready 1988). Results, not reported here, are very similar when we use a pre-announcement estimation period in conjunction with a trend variable.

[9] The log-transformation mitigates the skewness in the transaction distribution (Cready and Ramanan 1995) and we add 1 to avoid taking the log of zero.

$$\mathrm{ABRET}_{it} = (|R_{ikt} - R_{kt}| - \mathrm{MEAN\_ABRET}_{is})/\sigma_{ARiks}$$

where:

R_{ikt} = the return on firm i, a member of CRSP size-decile k, on day t;
R_{kt} = the CRSP size-decile k return on day t;
$\mathrm{MEAN\_ABRET}_{is}$ = the mean value of $|R_{ikt} - R_{kt}|$ over estimation periods (days −105 to −6 relative to the announcement date); and
σ_{ARiks} = the standard deviation of $|R_{ikt} - R_{kt}|$ estimated over estimation period s.

We calculate ABRET and then sum by day over the multiday response periods. For example, for a two-day period we determine ABRET values for each of the two days and then add these two values together to derive the two-day ABRET.[11]

Studies often incorporate the expected direction of the return effect into the research design. We incorporate expected direction into our analysis by conditioning returns on the sign of the difference between actual and expected earnings. Following Foster (1977), when actual earnings fall below expected earnings we multiply the return by negative one (i.e., a short sale) and perform statistical analysis using one-tailed tests on this signed return metric. If we have accurately measured the conditioning variable—in this case, unexpected earnings—then the observed return in direction-based designs incorporates additional information into the test procedure. In general, the more information upon which a test procedure is based, the greater is its power to reject false null hypotheses. However, if there is noise in the conditioning variable—that is, if "good news" announcements are misclassified as "bad news," and vice versa—then such conditioning can add noise to the design.

We examine the power of conditional return designs using the following metric:

$$\mathrm{URET}_{it} = I_i\,(R_{ikt} - R_{kt})/\sigma_{URiks}$$

where:

I_i = an indicator variable set to 1 if we predict the earnings announcement increases price, and is set to −1 if we predict the earnings announcement decreases price; and
σ_{URikj} = the standard deviation of $(R_{ikt} - R_{kt})$ estimated over estimation period s.

In earnings announcement research, the earnings forecast error most commonly determines the indicator variable's sign. We use two alternative measures of forecast error: random-walk error based on year-ago quarterly earnings (RW-URET) and analyst forecast error based on the I/B/E/S mean forecast from the month preceding the earnings-announcement release month (AF-URET).

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[10] Market-adjusted returns and abnormal returns relative to beta-matched portfolios led to similar inferences.

[11] Alternatively, we could recalculate ABRET using two-day returns, expectations, and standard errors. Preliminary analysis, however, indicated that such multiday accumulations did not attain as high rejection rates as the day-by-day accumulation. This may reflect that any price reversal effects over the two days are captured as separate positive influences in the daily absolute returns, but are netted in multiday accumulations.

Grand Sample

The study employs a grand sample of 49,274 earnings announcements between January 1, 1983 and December 31, 1992. We include earnings announcements in this sample if (1) the firm is listed on the NYSE; (2) the earnings announcement date is available from Compustat; (3) return data are available for at least 80 of the 100 days in the day −105 to −6 period relative to the announcement date; (4) transactions and volume data are available for at least 80 of the 100 days from the time periods spanning day −55 to −6 and day +6 to +55 relative to the announcement date. We obtain return and volume data from CRSP and transactions data from the ISSM database.[12]

Table 1 presents descriptive information on the grand sample, partitioned by earnings announcement date into three time periods: 1983–1986, 1987–1989, and 1990–1992. The increase in sample size from 14,063 announcements in the initial 1983–1986 period to 19,311 in the 1990–1992 period is attributable to (1) a general increase in the number of NYSE-listed firms; (2) better trading-data coverage in the ISSM database (early years suffer from a large number of missing trading data); and (3) more accurate cross-referencing of firms by ISSM.

Table 1 indicates that daily non-earnings-announcement-period percentage volume decreases slightly from 0.23 percent to 0.22 percent of outstanding shares per day. Numbers of transactions and absolute return, however, both increase over the ten-year sample period, with the number of transactions increasing from 48.9 to 74.0 per day and daily absolute return increasing from 1.48 percent to 1.88 percent per day.

Earnings-announcement-period increases in percentage volume, number of transactions, and absolute returns are greatest in the 1990–1992 time period. Daily means for all three metrics decrease when we extend the event period from days −1 to 0, to days −1 to +1. For the two trading measures this decrease is slight. However, the average daily absolute return effect decreases by 0.07 percentage points after including day +1.

Table 1 also reports descriptive statistics for random-walk and analyst-based earnings forecast errors. We calculate the random-walk error per share as the difference between the announced quarterly earnings per share (EPS) and year-ago EPS for that fiscal quarter. Requiring current- and prior-year EPS (from Compustat) reduced the number of firm announcements for which we have random-walk forecast error to 46,874. We used the I/B/E/S summary data set to compute analyst forecast error as the difference between I/B/E/S earnings and the mean I/B/E/S forecast from the month prior to the announcement month.[13] Because I/B/E/S covers fewer firms, we have analyst forecast errors for only 28,129 of the grand sample of earnings announcements.

Employing an investment strategy of buying stocks with positive forecast errors and selling short firms with negative forecast errors, based on foreknowledge of the sign of the

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[12] The Institute for the Study of Security Markets based at the University of Memphis produced the ISSM data. The data set provides transaction and quotation records from January 1, 1983, through December 31, 1992, for all NYSE-listed, AMEX-listed, and (beginning in 1990) OTC-listed securities, for trades occurring on the NYSE, AMEX, and regional exchanges as well as the NASDAQ OTC market.

[13] We also examine the most recent analyst forecasts and the mean of those forecasts updated within 45 days before the announcement. Untabulated analyses revealed that these alternative expectation measures lead to slightly lower rejection rates, on average.

TABLE 1
Descriptive Statistics for Grand Sample of Earnings Announcements between 1/1/83 and 12/31/92[a]

Descriptive Variable	*Announcement Date* *1983–86*	 *1987–89*	 *1990–92*	*Full Sample*
Number of earnings announcements	14,063	15,900	19,311	49,274
Mean market value of equity (in millions)[b]	$1,032	$1,358	$1,606	$1,359
Mean Estimation Period Statistics:				
Daily percentage volume[c]	0.23	0.22	0.22	0.22
Daily number of transactions[d]	48.9	55.8	74.0	61.0
Daily absolute size-adjusted return[e]	1.48%	1.57%	1.88%	1.66%
Earnings Announcement Period Statistics:				
Mean Increase in Percentage Volume:				
Day −1 to 0 average (in % points)	0.077	0.065	0.092	0.080
Day −1 to −1 average (in %.points)	0.065	0.071	0.092	0.078
Mean Increase in Number of Transactions				
Day −1 to 0 average	8.4	10.7	19.7	13.7
Day –1 to +1 average	7.2	9.5	19.4	13.5
Mean Increase in Absolute Size-Adjusted Return				
Day −1 to 0 average (in % points)	0.47	0.41	0.64	0.52
Day −1 to +1 average (in % points)	0.36	0.35	0.58	0.45
Mean Random-walk forecast error (per share)[f]	–$0.42	$0.10	–$0.06	–$0.11
Number of random-walk forecast errors	13,529	15,107	18,238	46,874
Mean Analyst forecast error (per share)[g]	–$0.16	$0.01	–$0.02	–$0.05
Number of analyst forecast errors	7,395	9,699	11,035	28,129
Mean Random-Walk Signed Return[h]				
Day –1 to 0 average (in % points)	0.47	0.37	0.49	0.45
Day –1 to +1 average (in % points)	0.34	0.30	0.42	0.36
Mean Analyst Signed Return[h]				
Day –1 to 0 average (in % points)	0.36	0.29	0.51	0.39
Day –1 to +1 average (in % points)	0.27	0.24	0.41	0.32

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[a] The grand sample includes 49,274 annual and quarterly earnings announcements by NYSE-listed firms.

[b] Market value of equity equals share price multiplied by the number of shares outstanding on trading day –106 relative to each announcement date.

[c] For each announcement, we calculate mean estimation period daily percentage volume as the percentage of outstanding shares traded each day per CRSP, averaged over trading days –55 to –6 and +6 to +55 relative to the announcement date.

[d] For each announcement, we calculate mean estimation period daily number of transactions as the number of transactions (i.e., trades) occurring in the firm's common stock, calculated from the ISSM data base, averaged over trading days –55 to -6 and +6 to +55 relative to the announcement date.

[e] For each announcement, we calculate mean estimation period daily absolute size-adjusted return as the absolute value of the difference in the announcing firm's return and the size-decile-matched CRSP portfolio return averaged over trading days –105 to –6 relative to the announcement date.

[f] Random-walk forecast error is the difference between earnings in quarter q and earnings for the same quarter of the previous year.

[g] Analyst forecast error is the difference between actual quarterly earnings (per I/B/E/S) and the mean I/B/E/S earnings forecast for the month preceding the announcement.

[h] We multiply returns for negative earnings forecast error announcements by –1.

random-walk forecast error on day −1, yields an average return of 0.45 percent per day over days −1 to 0 for the random-walk sample. Foreknowledge of the sign of the analyst forecast error on day −1 yields average daily returns of 0.39 percent over days −1 to 0. The lower return to foreknowledge of the analyst forecast error is attributable to selection bias in I/B/E/S coverage: the random-walk-based average return for only those firms for which I/B/E/S data are also available falls to 0.35 percent over days −1 to 0, which is less than the companion analyst forecast error return.

Statistical Test Procedures

To evaluate the statistical power of alternative metrics we use the grand sample to generate smaller test samples. We randomly generate (without replacement) 1,000 samples of 15 earnings announcements, 25 earnings announcements, and 40 earnings announcements from the grand sample. (Because of grand-sample-size restrictions, we generate only 500 samples of each of the three sample sizes when we compare the analyst forecast metrics.) We then test the null hypothesis that each metric has a zero mean response on each sample (j) using cross-sectional t-tests.[14]

Table 2 provides evidence that rejection rates for our test procedures and metrics are well-behaved. The table reports results of testing the hypotheses of a zero mean response using data from days −10 to −8 relative to the earnings announcement date, when the null hypothesis of no market response is likely true.[15] The rejection rates reported in Table 2 reveal no evidence of overrejection. Indeed, the tests appear somewhat conservative in that rejection rates tend to be too small rather than too large. In no instance does the rejection rate exceed its expected level under the null hypothesis (i.e., 5 percent in Panel A; 1 percent in Panel B).

III. RESULTS

Unconditional Response Metric Performance

Table 3 reports announcement period rejection rates for the VSHRS, VTRAN, and ABRET metrics by sample size using 5 percent (Panel A) and 1 percent (Panel B) significance levels. Rejection rates for the two trading metrics, VSHRS and VTRAN, significantly exceed their counterpart ABRET rates ($p < 0.01$).[16] The 5 percent significance level day −1 to 0 VSHRS metric rejection rates of 37.6 percent, 53.3 percent, and 72.0 percent for sample sizes of 15, 25, and 40, respectively, exceed their counterpart ABRET-based rejection rates by between 13.0 (sample size of 15) and 8.7 (sample size of 40) percentage points. The two-day 5 percent VTRAN rejection rates of 45.9 percent, 64.2 percent, and 84.8 percent exceed counterpart ABRET-based rejection rates by more than 20 percentage

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[14] A more powerful alternative is to use normal-distribution-based tests similar to those described in Patell (1976) and Marais (1984, 1985). Such tests, however, overreject the null hypothesis when the underlying distributions are kurtotic, which is the case for absolute returns. Hence, we follow Cready and Mynatt (1991) and use cross-sectional rather than normal-distribution-based tests. Moreover, in analyses not reported here, application of normal-distribution tests across all metrics yielded similar relative rejection rate differences, except that the differences between the VSHRS rejection rates and the ABRET rejection rates narrowed, while the differences between the VSHRS rejection rates and the VTRAN rejection rates widened, relative to those reported here.

[15] We adjust the estimation periods for these tests accordingly. Specifically, we use data from days −110 to −11 to estimate expected absolute return and standard deviation of both absolute and unexpected returns. We use data from days −60 to −11 and 11 to 60 to estimate expected volume, expected number of transactions, and standard deviations of unexpected volume and unexpected transactions.

[16] Consistent with Cready and Ramanan (1995), VTRAN rejection rates also exceed counterpart VSHRS rejection rates at the 1 percent level in all instances except for the three-day 1 percent level analysis, where the difference is significant at only the 5 percent level.

TABLE 2
Rejection Rates for the Null Hypothesis of No Increase in the Mean for Alternative Investor Response Metrics in Days −10 to −8 around Earnings Announcement Dates[a]

Panel A: Rejection Rates across 1,000 Non-Overlapping Samples Using 5 Percent, One-Tailed, Rejection Rule[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
Response Metric[e]	*15*	*25*	*40*	*15*	*25*	*40*
VSHRS	3.3%	2.8%	2.0%	2.6%	2.7%	2.0%
VTRAN	2.9	2.9	2.5	2.6	2.9	1.8
ABRET	2.7	3.3	2.8	2.2	2.9	3.0
URET	4.5	4.6	4.4	3.9	4.9	4.0

Panel B: Rejection Rates across 1,000 Non-Overlapping Samples Using 1 Percent, One-Tailed, Rejection Rule[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
Response Metric[e]	*15*	*25*	*40*	*15*	*25*	*40*
VSHRS	0.5%	0.7%	0.3%	0.4%	0.6%	0.4%
VTRAN	0.6	0.7	0.6	0.5	0.6	0.6
ABRET	0.2	0.1	0.4	0.3	0.5	0.5
URET	0.6	0.5	0.8	0.8	0.6	0.5

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[a] All samples are random without-replacement draws from the grand sample of 49,871 annual and quarterly earnings announcements by NYSE-listed firms between 1/1/83 and 12/31/92.

[b] We base rejection on a cross-sectional t-test of the null hypothesis that the response metric mean is less than or equal to 0.

[c] We accumulate each metric over days −10 and −9 relative to the earnings announcement date.

[d] We accumulate each metric over days −10 through −8 relative to the earnings announcement date.

[e] Response metric definitions:

VSHRS = the actual percentage of outstanding shares traded, less the predicted percentage of outstanding shares traded (based on a time-series first-order serial correlation regression of daily percentage volume over days −60 to −11 and +11 to +60 relative to the announcement date volume, on the percentage of shares traded for all NYSE-listed firms), where this difference is divided by the standard deviation of the residuals from the regression (volumes are log-transformed);

VTRAN = the actual number of transactions, less the predicted number of transactions (based on a time-series first-order serial correlation regression of the daily number of transactions over days −60 to −11 and +11 to +60 relative to the announcement date, on the total number of all NYSE-listed firm transactions), where this difference is divided by the standard deviation of the residuals from the regression (the number of transactions is log-transformed);

ABRET = the absolute value of the difference between actual daily return and return on the CRSP size-matched decile portfolio for the announcing firm, less the mean of this same absolute difference over days −110 to −11 relative to the announcement date, where this difference is divided by the standard deviation of the absolute differences over days −110 to −11; and

URET = the difference between actual daily return and the return on the CRSP size-matched decile portfolio for the announcing firm, divided by the standard deviation of this difference over days −110 to −11 relative to the announcement date.

TABLE 3
Rejection Rates for the Null Hypothesis of No Increase in the Mean for Alternative Investor Response Metrics in Days −1 to +1 Relative to Earnings Announcement Dates[a]

Panel A: Rejection Rates across 1,000 Non-Overlapping Samples Using 5 Percent, One-Tailed, Rejection Rule[b]

Response Metric[e]	*Two-Day Accumulation Period*[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
	15	*25*	*40*	*15*	*25*	*40*
VSHRS	37.6%	53.3%	72.0%	41.2%	57.1%	77.7%
VTRAN	45.9	64.2	84.8	48.7	68.6	87.1
ABRET	24.6	43.5	63.3	27.8	47.5	68.1

All VSHRS and VTRAN rejection rates significantly exceed their counterpart ABRET rejection percentages at the 1 percent significance level using McNemar's χ^2 test.

Panel B: Rejection Rates across 1,000 Non-Overlapping Samples Using 1 Percent, One-Tailed, Rejection Rule[b]

Response Metric[e]	*Two-Day Accumulation Period*[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
	15	*25*	*40*	*15*	*25*	*40*
VSHRS	14.0%	25.9%	43.2%	17.3%	30.0%	51.2%
VTRAN	17.3	33.5	59.5	20.3	37.6	66.1
ABRET	3.9	12.7	30.1	5.9	14.5	34.2

All VSHRS and VTRAN rejection rates significantly exceed their counterpart ABRET rejection percentages at the 1 percent significance level using McNemar's χ^2 test.

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[a] All samples are random without-replacement draws from the grand sample of 49,274 annual and quarterly earnings announcements by NYSE-listed firms occurring between 1/1/83 and 12/31/92.

[b] We base rejection on a cross-sectional t-test of the null hypothesis that the response metric mean is less than or equal to 0.

[c] We accumulate each metric over days −1 and 0 relative to the earnings announcement date.

[d] We accumulate each metric over days −1 through +1 relative to the earnings announcement date.

[e] Response metric definitions:

VSHRS = the actual percentage of outstanding shares traded, less the predicted percentage of outstanding shares traded (based on a time-series first-order serial correlation regression of daily percentage volume over days −55 to −6 and +6 to +55 relative to the announcement date volume, on the percentage of shares traded for all NYSE-listed firms), where this difference is divided by the standard deviation of the residuals from the regression (volumes are log-transformed);

VTRAN = the actual number of transactions, less the predicted number of transactions (based on a time-series first-order serial correlation regression of the daily number of transactions over days −55 to −6 and +6 to +55 relative to the announcement date, on the total number of all NYSE-listed firm transactions), where this difference is divided by the standard deviation of the residuals from the regression (the number of transactions is log-transformed); and

ABRET = the absolute value of the difference between actual daily return and return on the CRSP size-matched decile portfolio for the announcing firm, less the mean of this same absolute difference over days −105 to −6 relative to the announcement date, where this difference is divided by the standard deviation of the absolute differences over days −105 to −6.

points. Table 3 shows similar rejection-rate patterns for tests based on 1 percent significance levels and three-day event periods.[17]

One primary means researchers can use to increase the power of their statistical tests is to increase the sample size. Therefore, one way of evaluating the implications of the differential cross-metric rejection percentages reported in Table 3 is to determine how much the sample size would have to increase for the ABRET-based rejection levels to equal their volume-based counterparts. The results reported in Table 3 suggest that researchers would need, for example, samples of 40 ABRETs around earnings announcements to attain rejection rates comparable to the rejection rates achieved by just 25 announcement-associated VTRANs. That is, replacing the ABRET metric with the VTRAN metric increases power to roughly the same extent as increasing sample size by 60 percent.

We also explored the extent to which these sample size equivalencies hold in larger samples sizes by sampling with replacement. In these untabulated analyses, we reach a 95 percent rejection rate (5 percent probability of a Type II error) for sample sizes of 70, 55, and 85 for the VSHRS, VTRAN, and ABRET metrics, respectively. Hence, a 21 percent increase in sample size brings the ABRET-based test's power in line with the power of the VSHRS-based test, while a 51 percent increase in sample size brings it in line with the power of the VTRAN-based test. These percentage increases are somewhat smaller than those implied by the results reported in Table 3 for smaller sample sizes, but nonetheless reveal that the substantial power advantage of the VSHRS and VTRAN metrics extends beyond the range of sample sizes considered in Table 3.

Conditional (Signed) Return Analysis

Table 4 reports rejection rates for tests based on the RW-URET (Panel A) and AF-URET (Panel B) metrics for sample sizes of 15, 25, and 40 announcements. These conditional (signed) return metrics require earnings from the same quarter of the prior year (RW-URET) and analyst earnings forecasts (AF-URET). Data unavailability reduces the grand sample from which we form subsamples. For example, we can compute AF-URET measures for only 28,129 announcements. This makes it impossible to form 1,000 40-announcement subsamples without replacement, so we base AF-URET rejection rates on only 500 subsamples. Since data availability may be associated with systematic differences in announcement-period return and volume magnitudes, we also calculate rejection rates for VSHRS, VTRAN, and ABRET for the 500 RW-URET subsamples (Panel A) and for the 500 AF-URET subsamples (Panel B). Panels A and B of Table 4 show that rejection rates for the RW-URET and AF-URET conditional return metrics are considerably smaller than those for either of the counterpart trading-response metrics ($p < 0.01$). Indeed, rejection rates for the VTRAN metrics are at least twice as large as counterpart rejection rates for the RW-URET and AF-URET metrics.

Table 4 also reports that the rejection rates for the unconditional ABRET metric are significantly higher ($p < 0.01$) than the counterpart rates for the RW-URET and AF-URET metrics. Indeed, for the largest sample size of 40 announcements, the ABRET rejection rates exceed counterpart: (1) RW-URET rates by 27.2 (two-day period) and 31.7 (three-day period) percentage points, and (2) AF-URET rates by 31.6 (two-day period) and 32.4 (three-day period) percentage points. Thus, error in signing the earnings surprise outweighs

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[17] Rejection rates are smaller, but follow a similar cross-metric pattern when we use one-day (i.e., day −1, day 0) event periods.

TABLE 4
Rejection Rates (5 Percent p-level) for the Null Hypothesis of No Increase in the Mean for Conditional (Signed) Unexpected Return vs. Unconditional Trading and Absolute Return Response Metrics in Days −1 to +1 Relative to Earnings Announcement Dates[a]

Panel A: Rejection Rates by Sample Size for Unexpected Return Conditioned on Sign of Change in Earnings Per Share (RW-URET), Transactions-Based (VTRAN), Percentage Volume-Based (VSHRS), and Absolute Return-Based (ABRET) Metrics for 500 Non-Overlapping Samples[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
Response Metric[e]	*15*	*25*	*40*	*15*	*25*	*40*
VSHRS	37.9%	53.8%	72.6%	40.2%	57.7%	78.0%
VTRAN	44.9	65.2	85.2	47.7	70.1	87.7
ABRET	23.7	43.4	63.1	26.9	47.7	67.7
RW-URET	18.8	25.4	35.9	18.5	25.3	36.0

All VTRAN, VSHRS, and ABRET rejection rates significantly exceed their counterpart RW-URET rejection percentages at the 1 percent significance level using McNemar's χ^2 test.

Panel B: Rejection Rates by Sample Size for Unexpected Return Conditioned on Analyst Earnings Forecast Error (AF-URET), VTRAN, VSHRS, and ABRET Metrics for 500 Non-Overlapping Samples[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period*[d]		
	Sample Size			*Sample Size*		
Response Metric[e]	*15*	*25*	*40*	*15*	*25*	*40*
VSHRS	38.9%	54.6%	76.8%	41.3%	61.0%	80.8%
VTRAN	45.2	67.0	86.6	48.2	71.4	89.0
ABRET	26.9	46.5	71.0	32.8	51.9	72.4
AF-URET	21.8	28.3	39.4	20.7	26.9	38.0

All VTRAN, VSHRS, and ABRET rejection rates significantly exceed their counterpart UR-W rejection percentages at the 1 percent significance level using McNemar's χ^2 test.

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[a] All samples are random draws from the grand sample of 28,129 annual and quarterly earnings announcements for which analysts forecasts are available, by NYSE-listed firms occurring between 1/1/83 and 12/31/92.

[b] We base rejection on a cross-sectional t-test of the null hypothesis that the response metric mean is less than or equal to 0.

[c] We accumulate each metric over days −1 and 0 relative to the earnings announcement date.

[d] We accumulate each metric over days −1 through 1 relative to the earnings announcement date.

[e] The VSHRS, VTRAN, and ABRET response metrics are described in the notes to Table 3. RW-URET is URET, as described in Table 3, multiplied by −1 if actual quarterly earnings is less than earnings for the same quarter of the previous year, and multiplied by 1 otherwise. AF-URET is URET multiplied by −1 if actual quarterly earnings (per I/B/E/S) is less than the mean I/B/E/S earnings forecast for the month preceding the announcement, and multiplied by 1 otherwise.

the direction-based return metrics' potential to provide more powerful tests of market response to earnings announcements.[18]

Joint Price/Volume Tests

Bamber and Cheon's (1995) finding that volume and return are only modestly correlated at announcement dates suggests that a joint test of volume *and* return response may be more powerful than a simple examination of either volume or return exclusively. We investigate this possibility by conducting tests based on the joint outcome of volume and return response tests. In these joint volume and return tests we use a nominal 2.5 percent significance level rejection rule to independently evaluate whether each metric-specific test (i.e., volume or return) leads to rejection of the nonresponse null. We then reject the general hypothesis of no investor response to the information release if *either* the null of no volume response *or* the null of no return response is rejected. Based on Bonferroni inequalities, the bounded overall p-level for our two-test analysis of a single hypothesis is 5 percent.[19]

Panel A of Table 5 reports rejection rates when we employ ABRET in combination with VSHRS (ABRET&VSHRS). Panel B reports these rates for the ABRET and VTRAN (ABRET&VTRAN) combination. To facilitate comparisons, these panels also re-report the stand-alone 5 percent significance level rejection rates for VSHRS, VTRAN, and ABRET originally reported in Table 3. The ABRET&VSHRS rejection rates reported in Panel A exceed counterpart ABRET-only rejection rates for all three samples sizes, by between 7.3 and 9.8 percentage points. All differences are significant at the 1 percent level. The negligible differences between the ABRET&VSHRS rejection rates and the VSHRS-only rates suggest that their performances are comparable.

Panel B of Table 5 indicates that two-day accumulation period rejection rates for the ABRET&VTRAN test exceed counterpart ABRET-only rejection rates (based on a 5 percent rejection rule) by 12.0, 15.2, and 18.1 percentage points for the 15, 25, and 40 announcement samples, respectively. The three-day accumulation period rejection rates exhibit a similar pattern. All differences are significant at the 1 percent level, indicating that ABRET&VTRAN-based tests are more powerful than tests based on ABRET alone. However, Panel B also shows that VTRAN-only rejection rates are significantly higher (0.05 level or better) than ABRET&VTRAN-based tests. The difference in rejection rates narrows considerably as sample size increases, dropping to only 2.1 percentage points for the three-day-window, 40-announcement samples. In sum, our results suggest that although it is advantageous to supplement the absolute return response analyses with volume-based metrics, there is no obvious advantage to supplementing a volume-based test with a return-based test.

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[18] Brown and Warner (1985) find that tests based on time-series standard errors are more powerful than the cross-sectional tests we employ. In further analyses, we determined conditional return metric rejection rates using a time-series standard error test like the method Dyckman et al. (1984) used. As expected, rejection rates increase using this test. Moreover, for 15 announcement samples the conditional returns' rejection rates exceed counterpart ABRET-based rejection rates (significant at the 0.01 level). However, as sample size increases, the advantage of time-series standard errors dissipates rapidly. For samples of 25 announcements, rejection rates for the conditional returns time-series based tests are statistically indistinguishable from the ABRET-based rates reported in Table 4. For 40 announcement samples the conditional returns' time-series rejection rates are substantially smaller than the reported ABRET-based rates.

[19] The parametric test of choice here is a Hotelling T^2 test of the hypothesis that the vector of means is zero. In untabulated analyses we found that rejection rates based on the Hotelling test were consistently lower than those reported in Table 5. The Hotelling tests are less powerful than the Bonferroni approach because in the Hotelling test the alternative hypothesis is inherently two-tailed. Here, however, the alternative hypothesis is one-tailed (that the means exceed zero). The experiment-wide Bonferroni approach exploits these directional alternative hypotheses.

TABLE 5
Power of Joint Return/Volume Tests to Reject the Null Hypothesis of No Investor Response in Days −1 to +1 around Earnings Announcement Dates[a]

Panel A: Rejection Rates for Joint Return and Volume Test of the Null Hypotheses of No Investor Response (ABRET&VSHRS) across 1,000 Non-Overlapping Samples Using 1 Percent, One-Tailed, Rejection Rule[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period[d]*		
	Sample Size			*Sample Size*		
Test Metric/ Procedure[e]	*15*	*25*	*40*	*15*	*25*	*40*
ABRET&VSHRS	32.1%	50.8%	73.2%	36.1%	57.3%	77.9%
VSHRS only	37.6**	53.3	72.0	41.2**	57.1	77.7
ABRET only	24.6**	43.5**	63.3**	27.8**	47.5**	68.1**

Panel B: Rejection Rates for Joint Return and Volume Test of the Null Hypotheses of No Investor Response (ABRET&VTRAN) across 1,000 Non-Overlapping Samples Using 5 Percent, One-Tailed, Rejection Rule[b]

	Two-Day Accumulation Period[c]			*Three-Day Accumulation Period[d]*		
	Sample Size			*Sample Size*		
Test Metric/ Procedure[e]	*15*	*25*	*40*	*15*	*25*	*40*
ABRET&VTRAN	36.6%	58.7%	81.4%	41.4%	64.2%	85.0%
VTRAN only	45.9**	64.2**	84.8**	48.7**	68.6**	87.1*
ABRET only	24.6**	43.5**	63.3**	27.8**	47.5**	68.1**

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*, ** Rejection rate differs from ABRET&VSHRS (Panel A) or ABRET&VTRAN (Panel B) rejection rate at the 0.05 and 0.01 levels, respectively, using McNemar's χ^2 test.

[a] All samples are random without-replacement draws from the grand sample of 49,274 annual and quarterly earnings announcements by NYSE-listed firms occurring between 1/1/83 and 12/31/92.

[b] We base rejection on a cross-sectional t-test of the null hypothesis that the response metric mean is less than or equal to 0.

[c] We accumulate each metric over days −1 and 0 relative to the earnings announcement date.

[d] We accumulate each metric over days −1 through +1 relative to the earnings announcement date.

[e] Table 3 defines the response metrics.

IV. CONCLUSION

Bamber and Cheon (1995, 440) conclude that because there is considerable independence between the market's return and trading responses at earnings announcement dates, "information content-oriented research should continue to examine both price and volume reactions." Our primary finding, that tests of investor response to earnings announcements are more likely to correctly reject the null hypothesis of no investor response based on measures of trading than are comparable tests based on returns, reinforces their conclusion. Our results further suggest that not only is volume a nonredundant measure of investor response to information events, but it is also a more powerful measure. Our analysis indicates that volume's power advantage can be sizable—in the settings we examine, the best-performing volume metric, the metric based on the number of transactions, increases power beyond that of the return metric equal to a 60-plus percent increase in sample size.

The importance of employing powerful test designs depends on the specific research context. When sample size is very large or the investor response is sizable, any reasonable design and metric is likely to reject a false null. However, when sample size is small or return responses are small, our analysis suggests that volume-based response metrics are more likely to detect the presence of an investor response than are return-based metrics. In addition, and as a general rule, in any setting where return-based analyses fail to reveal evidence of investor response, it is important to conduct powerful tests before concluding that no response is present. Hence, before concluding that investors do not respond to a public disclosure based on a returns analysis, we recommend that researchers confirm the nonresponse inference using a trading-based analysis.

Our findings, although consistent with the notion that trading response is a more widespread phenomenon than price response (Kandel and Pearson 1995), do not provide direct evidence on the relative incidence of price *vis-à-vis* trading response. Rejection-rate levels reflect (relative) mean shift magnitudes, and these magnitudes increase with both response frequency and response magnitude. Hence, our findings are attributable to either: (1) more earnings announcements stimulate trading responses than return responses, or (2) trading response magnitudes are simply larger (in a statistically detectable sense) than return response magnitudes.

Finally, two significant caveats temper our conclusion that tests based on volume metrics outperform tests based on return metrics. First, our analysis applies to questions of the existence or nonexistence of some sort of investor response to information. If, however, a researcher is investigating a more subtle question, such as a theoretically expected valuation effect, or a specific form of investor behavior that is most directly revealed by their trading decisions, then the research should of course use the metric that is most appropriate. The second caveat is that the generalizability of our inferences to other information events depends on whether the relative magnitudes of volume and return response to earnings announcements typifies the response to other information events. While we have no *ex ante* reason to believe this is not the case, if other information events stimulate larger return response relative to trading response, then our conclusion that volume-based metrics provide more powerful tests may no longer apply.

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How Informative Are Value-at-Risk Disclosures?

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ABSTRACT: Value at Risk (VAR), a measure of the dollar amount of potential loss from adverse market moves, has become a standard benchmark for measuring financial risk. Spurred by regulators and competitive pressures, more institutions are reporting VAR numbers in annual and quarterly financial reports. To provide preliminary evidence on the informativeness of these new disclosures, I investigate the relation between the trading VAR disclosed by a small sample of U.S. commercial banks and the subsequent variability of their trading revenues. The empirical results suggest that VAR disclosures are informative in that they predict the variability of trading revenues. Thus, analysts and investors can use VAR disclosures to compare the risk profiles of banks' trading portfolios.

Keywords: *derivatives; disclosure regulation; market risk disclosures; value at risk; Basel Committee; SEC.*

Data Availability: *The data used in this study can be obtained from public sources.*

I. INTRODUCTION

Value at Risk (VAR) has become a standard benchmark for measuring financial risk. The Group of Thirty (1993) report on derivatives stated that "market risk is best measured as value at risk." Specifically, VAR provides a summary statistic of the order of magnitude of potential losses due to market risk. VAR is the maximum loss over a target horizon such that there is a low, prespecified probability that the actual loss will be larger. A company disclosing, for instance, that its daily VAR is $30 million at the 95 percent level, means that there is only a 5 percent chance the firm will incur more than a $30 million loss over the next day.

VAR summarizes the effects of leverage, diversification, and probabilities of adverse price movements in a single dollar amount that is easy to communicate. VAR disclosure also improves the governance of derivatives as it forces the firm to develop a systematic process of risk measurement.

This paper has benefited from comments of seminar participants at the Federal Reserve Board and the Bank of England. I also appreciate the feedback from Daniel Thornton and the referees.

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This paper investigates the relation between publicly disclosed VAR measures and the volatility of trading revenues for a group of eight major U.S. commercial banks. I collected close to six years of historical data on VAR measures and associated trading revenues from quarterly and annual financial reports. Because only a few banks began disclosing VAR in 1995, only eight banks with adequate disclosure history were available for study. I matched these banks' quarterly trading revenues with VAR measures they reported on an annual or quarterly basis. My results suggest that VAR disclosures are informative measures of risks in that they can be used to predict the variability of banks' trading revenues.

Before the advent of VAR, shareholders could not assess the total trading risks financial institutions assumed. Banks reported notional amounts of their derivatives positions, but these are not meaningful if positions hedge each other.

Few banks disclosed VAR before 1995. Since then, bank regulators, in particular the Basel Committee on Banking Supervision (BCBS), have prodded the industry to disclose more information. This started with the market risk amendment to the Basel Accord (BCBS 1995), and continues with the latest revision of the Basel Accord (BCBS 2001), which "strongly recommends" that banks disclose their VAR.[1] By 1999, 66 of the 71 financial institutions the Basel Committee surveyed disclosed VAR numbers in their publicly available financial reports (BCBS 1999). Most major financial institutions have implemented risk management systems and now publish their VARs on an annual or quarterly basis.

This study investigates whether the VAR numbers banks publicly disclose in their financial statements are related, cross-sectionally and over time, to subsequent fluctuations in banks' trading revenues. Such evidence is relevant beyond the financial institution context. The Securities and Exchange Commission (SEC) now requires *all* large U.S. publicly traded corporations to report *quantitative* information about their market risk in financial reports filed with the SEC.[2] Registrants choose among three methods: (1) a tabular presentation describing fair value and contract terms, (2) a sensitivity analysis describing potential changes in fair values under market fluctuations, and (3) VAR numbers. The SEC's (1997) market risk disclosure rule FRR No. 48, reviewed in Linsmeier and Pearson (1997), became effective in June 1998.

The SEC intended its market risk disclosure requirements to address security analysts' and accountants' general concern that "users are confused."[3] Prior reporting guidelines provided insufficient detail on the scope of firms' involvement in financial instruments and the potential effect of derivatives activity on corporate profits. The SEC reviewed U.S. public corporations' *qualitative* disclosure statements, and found that management discussion was typically uninformative. Nearly all companies explain that they use derivatives to "hedge." Few admit to outright speculation, even though the losses some corporations incurred are *prima facie* evidence to the contrary.[4]

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[1] The Basel Accord, established in 1988 to set minimum capital requirements for commercial banks against credit risks, was amended in 1996 to include a charge for market risk (BCBS 1995, 1996a). Banks now have a choice between using a "standardized" method, using predefined rules, or their own internal VAR measure as the basis for their capital charge for market risk. In practice, using internal VAR numbers leads to lower capital charges than the standardized model. This likely explains the industry-wide move to VAR measurement.

[2] The requirements cover derivatives and other financial instruments. The SEC also encourages, but does not require, inclusion of market risk due to nonfinancial assets, liabilities, or transactions, such as inventories or sales commitments to provide a more complete picture of market risk, although nonfinancial items may be more difficult to measure (Linsmeier and Pearson 1997).

[3] This phrase is ascribed to the AICPA (SEC 1997).

[4] As an example, Nick Leeson told his superiors at Barings that his positions on Japanese stock index futures hedged each other. However, he was taking large speculative positions that led to a loss of $1.3 billion (Bank of England 1995).

Because the line between selective hedging and speculation is thin, such general qualitative statements shed little light on the extent and effectiveness of corporate derivatives activities. This explains why the SEC mandated quantitative disclosures of market risk. Some observers, however, question whether such disclosures can be informative. For instance, Logan and Montgomery (1997, 3), representing the Treasury Management Association, testified that the SEC rules "are unlikely to enable investors to better understand a public company's use of these (derivatives) instruments and its aggregate risk exposure."[5]

No previous work investigates the informativeness of firms' publicly available VAR disclosures for the firms' risk profiles. Most prior research on the informativeness of derivatives-related disclosure focuses on the extent to which these disclosures are informative about the firm's exposure, or the linear sensitivity of share prices, to changes in a particular financial risk factor.[6] Exposure is a component of financial risk but focuses on one risk factor only. Thus, most prior research focuses on industries that are exposed to one major source of financial risk. For instance, Schrand (1997) examines whether derivatives disclosures provide useful information for assessing the interest rate exposure of a sample of savings and loan associations. She reports that derivatives activities lower savings and loans' interest rate exposure, measured by the sensitivity of stock prices to movements in interest rates. Similarly, Rajgopal (1999) finds that information about derivatives presented in a tabular and sensitivity analysis format is related to the share price exposure of oil and gas firms to oil and gas prices. Wong (2000) examines the foreign exchange rate exposure for a sample of U.S. manufacturing firms and finds that derivatives notionals partially explain exposure.

In contrast, a variety of risk factors affect large banks' trading portfolios. VAR disclosures summarize the banks' maximum potential losses after allowing for diversification across these different risk factors. This paper provides the first evaluation of the informativeness of publicly disclosed VAR measures for the risk of trading portfolios.

In a concurrent and independently developed working paper, Berkowitz and O'Brien (2001) investigate the association between daily VAR data that banks report *privately* to regulators, and subsequent daily trading revenues, for a sample of six large dealer banks over a two-year period. They provide a detailed analysis of the forecasting performance of VAR models. Their results, however, do not address the informativeness of public financial reports, because daily VAR and trading revenue data are available only to bank regulators (e.g., the Federal Reserve Board), and not to the public. While we would expect short-horizon, daily VAR data to provide useful forecasts of daily volatility of trading revenues, this relation may not necessarily hold with quarterly or annual data. This study fills this void by analyzing the informativeness of VAR disclosures at quarterly frequencies.

The rest of the paper proceeds as follows. Section II presents the testing framework. Section III describes the data and descriptive statistics. Section IV presents results, and Section V contains some concluding observations.

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[5] The compliance costs these requirements impose on registrants are not clear. However, corporations that trade derivatives likely invest in risk management systems, and the SEC (1998, 2) reports that the market risk disclosures are "not terribly costly," with a range of $10,000 to $50,000.

[6] Other accounting literature has studied the informativeness of derivatives disclosures for explaining the level of market values, as opposed to risk exposures. Venkatachalam (1996), for instance, finds that fair values for derivatives help explain the cross-sectional variation in bank share prices better than notional amounts. More recently, Linsmeier et al. (2002) conclude that increased market risk disclosures reduce investors' diversity of opinion about the effect of market risk on firm value.

II. TESTING THE PREDICTIVE POWER OF VAR

Denote the *daily* trading revenue a bank earned during day t + 1 as r_{t+1} and its associated VAR as VAR_t, measured as of the close of day t. VAR is the cutoff loss level where the probability that the bank incurs a loss beyond VAR equals 1 minus the confidence level (e.g., 1 percent for the typical confidence level for c = 99 percent):[7]

$$P[r_{t+1} - E(r_{t+1}) < -VAR_t] = 1 - c. \qquad (1)$$

To investigate whether the VAR number adequately estimates future losses, one could match the realized distribution of daily trading revenues with the daily VAR forecast by counting the number of "exceptions," or realizations of losses beyond VAR. This is in fact the "backtesting" framework bank regulators use.[8] Bank daily trading data, unfortunately, are proprietary. As such, they are unavailable to financial analysts and investors, who only have access to the bank's quarterly or annual VAR numbers and the bank's quarterly trading revenues.

I therefore use an alternative approach that transforms the VAR number into a measure of dispersion. Tests based on dispersion measures are more powerful than tests based on the number of exceptions because dispersion measures incorporate data from the whole distribution. The dispersion measure is based on the assumption that the distribution of trading revenues is symmetric. My approach accommodates the fat tails commonly observed in financial data.

This approach, however, is less appropriate when the distribution is highly skewed; for instance, for a trader dealing with short-term options, which are nonlinear instruments with skewed payoffs.[9] This is a lesser concern for a larger commercial bank, whose trading portfolio typically includes a range of instruments exposed to a wide variety of risk factors.[10]

As an illustration, assume that r_{t+1} has a conditional normal distribution with mean $\mu = 0$ and forecasted volatility of s_t, $r_{t+1} \sim N(0, s_t^2)$. This trading revenue r_{t+1} is the effect of fixed positions w_t combined with movements in normally distributed market risk factors x_{t+1} over day t + 1. The portfolio variance is then $s_t^2 = w_t'\Sigma_t w_t$, where Σ_t is the forecasted covariance matrix for the market risk factors as of the close of day t. The VAR measure is the forecasted volatility s_t multiplied by the standard normal deviate α for the selected confidence level (e.g., $\alpha = 2.33$ for a one-tailed confidence level of 99 percent). Hence, we have:

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[7] Another definition would exclude the expectation term. The definition given here, however, is consistent with deriving VAR from a multiple of the standard deviation. Some banks, like J.P. Morgan, explicitly define their "earnings at risk" as the deviation from the mean. In most applications with daily data, the expectation term is immaterial because it is dominated by the daily volatility. With the quarterly horizons used in this paper, however, the expectation term is more important. As a result, I include the expectation term to abstract from other items (e.g. fees, interest income, and expected profits) that do not represent unexpected trading revenues.

[8] See the BCBS (1996b). Christoffersen et al. (2001) have developed other frameworks for evaluating risk forecasts.

[9] The mere presence of options does not imply strongly asymmetric distributions if the risk horizon is short relative to the maturity of the option, however. The distribution of changes in values of long-term options, for instance, over the next day, is typically indistinguishable from a normal approximation. Consider an option on an asset worth $S = \$100$ with 10 percent annual volatility and zero dividend; interest rates are at 3 percent. Assuming 250 trading days in a year, the daily volatility is $0.10/\sqrt{250}$. For a six-month option, the Black-Scholes model gives a delta of 0.60 and gamma of 0.055. As a first approximation, I compute the 95 percent VAR from the delta of the option and the 95th percentile of the underlying asset, for which the associated standard normal deviate is $\alpha = 1.64$. This gives a linear VAR $= \Delta(\alpha\sigma S) = 0.60(1.64(0.10/\sqrt{250})\$100) = \$0.62$. For more precision, one could add a quadratic correction, which is $0.5\Gamma(\alpha\sigma S)^2 = \$0.03$. Because this is a small fraction of the linear VAR for this option, the option distribution is close to a normal distribution over this one-day risk horizon. As the risk horizon extends, however, the nonlinearity in options induces skewness in the distribution of option returns. Using the same option over a one-month risk horizon, the linear VAR is \$2.84 and the quadratic correction is \$0.62. Because the correction is a greater fraction of the linear VAR, the distribution is more asymmetrical.

[10] That is, unless the bank "games" the VAR number by altering the distribution of profits and losses to satisfy a fixed VAR at the expense of a small probability of large losses. Ju and Pearson (1999) have analyzed such strategies, which are more likely to occur at the level of traders' desks than at the aggregate level of large institutions such as the commercial banks in the sample.

$$\mathrm{VAR}_t = \alpha s_t. \tag{2}$$

With normally distributed market risk factors, the expectation of the absolute value $|r_{t+1}|$ is:

$$E(|r_{t+1}|) = \sqrt{\frac{2}{\pi}} \times s_t = 0.80 \times s_t. \tag{3}$$

Thus, the expected absolute value of the trading revenue is linearly increasing in the forecasted volatility of trading revenues, s_t, which is proportional to VAR.

This setup is valid for a wide class of distributions including the normal distribution. As long as the conditional distribution of trading revenues is fixed and symmetrical, the expected absolute value of trading revenues increases linearly with the VAR-based volatility. With distributions other than the normal, the coefficient will in general differ from $\sqrt{2/\pi}$, but the VAR measures should still be linearly related to the expected absolute value of trading revenues. For instance, when the conditional distribution is a Student-t with six degrees of freedom, which is typical of daily financial series such as exchange rates, the coefficient is 0.74 instead of 0.80.

Thus, I test the predictive power of banks' *quarterly* VAR disclosures by estimating the following equation:

$$|R_{t+1}| = a + b\sigma_t + \epsilon_{t+1}, \tag{4}$$

where R_{t+1} is the trading revenue for quarter t + 1, and σ_t is the forecasted volatility of *quarterly* trading revenue inferred from the bank's public disclosure of its trading VAR as of the end of quarter t.

Banks publicly disclose R_{t+1}, the sum of daily revenues for each day of the quarter, $R_{t+1} = \Sigma_{i=1}^{N} r_i$, where N is the number of trading days in a quarter, taken as N = 63 (assuming 21 trading days in a month). Banks also publicly disclose VAR numbers every quarter, but these disclosed VAR numbers correspond to a horizon of one day, which I must extend to match the quarterly horizon of banks' reported trading revenues. I extrapolate the banks' reported daily VAR numbers to a quarterly horizon, estimating the volatility in quarterly trading revenues from the daily volatility using the square root of time rule:

$$\sigma_t = s_t\sqrt{N}. \tag{5}$$

VAR is informative about the risks of future trading revenues if b estimated in Equation (4) is significantly positive. The value of b will equal its theoretical value of 0.80 only if several assumptions are met.

The first assumption is that σ_t is measured without error. Measurement error in σ_t will downward bias the slope coefficient due to the errors-in-variables problem. One source of error in σ_t is the extrapolation of the bank's reported daily VAR, s_t, to a quarterly horizon,

σ_t. This transformation assumes that the variance in the bank's trading revenues is constant throughout the quarter. This assumption may be reasonable if VAR moves slowly over the quarter. Figure 1 displays the time variation in the daily VAR for J.P. Morgan for the years 1997–1998. The graph shows some variation within the quarter, but major changes occur over longer horizons. Other sources of error in σ_t include that the forecasted volatility in trading revenues is only an estimate, subject to sampling variability. Furthermore, banks must make simplifying assumptions in estimating VAR, such as limiting the number of risk factors considered.[11]

A second assumption is that trading revenues, R_{t+1}, are measured without error. However, trading revenues are based on real portfolios with changing positions in which traders adapt their portfolios to market conditions, whereas VAR assumes that positions w_t are fixed over the horizon (i.e., fixed over the quarter, in this study's case).

The third assumption is that R_{t+1} has mean zero and is normally distributed. Thus, R_{t+1} must be the *unexpected* component of trading revenues. Unfortunately, trading revenues include other items such as fees and interest income, which banks exclude from the construction of VAR. In addition, we would expect trading revenues to generate sufficient profits to compensate for the immobilized capital and the operating expenses. This expected component of trading revenues may grow over time due to the increase in trading activities or change systematically as banks alter their risk profile. To correct for the expected component of the trading revenue, I measure the unexpected component as the difference between the quarterly trading revenue and its moving average over the previous four quarters. (I use the previous four quarters as the benchmark because trading revenues are not seasonal.) The "unexpected" trading revenue is then defined as:

$$R_{t+1} - E[R_{t+1}] = R_{t+1} - (1/4)\sum_{i=1}^{4} R_{t+1-i}. \tag{6}$$

This transformation implies that $|R_{t+1} - E[R_{t+1}]|$ should have an expected value of zero.[12] These estimates of unexpected revenues are statistically indistinguishable from zero for my sample of banks. Also, as expected, forecasts of expected trading revenues were positive for every bank in every single quarter.

I then implement Equation (4) as:

$$|R_{t+1} - E[R_{t+1}]| = a + b\sigma_t + \epsilon_{t+1}, \tag{7}$$

where the trading revenue has been replaced by its unexpected component. If the banks' VAR disclosures are informative about the risk of next quarter's trading revenues, then $b > 0$. Ideally, the regression R^2 should be high, implying that σ_t captures much of the

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[11] Banks must make additional simplifications in mapping financial instruments on the selected risk factors. These approximations introduce additional errors, or "model risk." Marshall and Siegel (1997) find that, as financial instruments become more complicated, there is less agreement in VAR numbers produced by 10 different VAR software packages.

[12] One minor drawback of this approach is that the four-quarter moving average measure of expected trading revenues induces a slight negative autocorrelation in the unexpected trading revenues ($\rho = -0.05$). A quarter with, say, a large negative shock pulls down the estimate of $E[R_{t+1}]$ for the next three quarters, which is more likely to generate positive values for UR_{t+1} to UR_{t+3}.

The results are robust to an alternative moving average based on all previous data (not just the last four quarters), and to a measure of unexpected trading revenues defined as the deviation from a time trend fitted over the whole period.

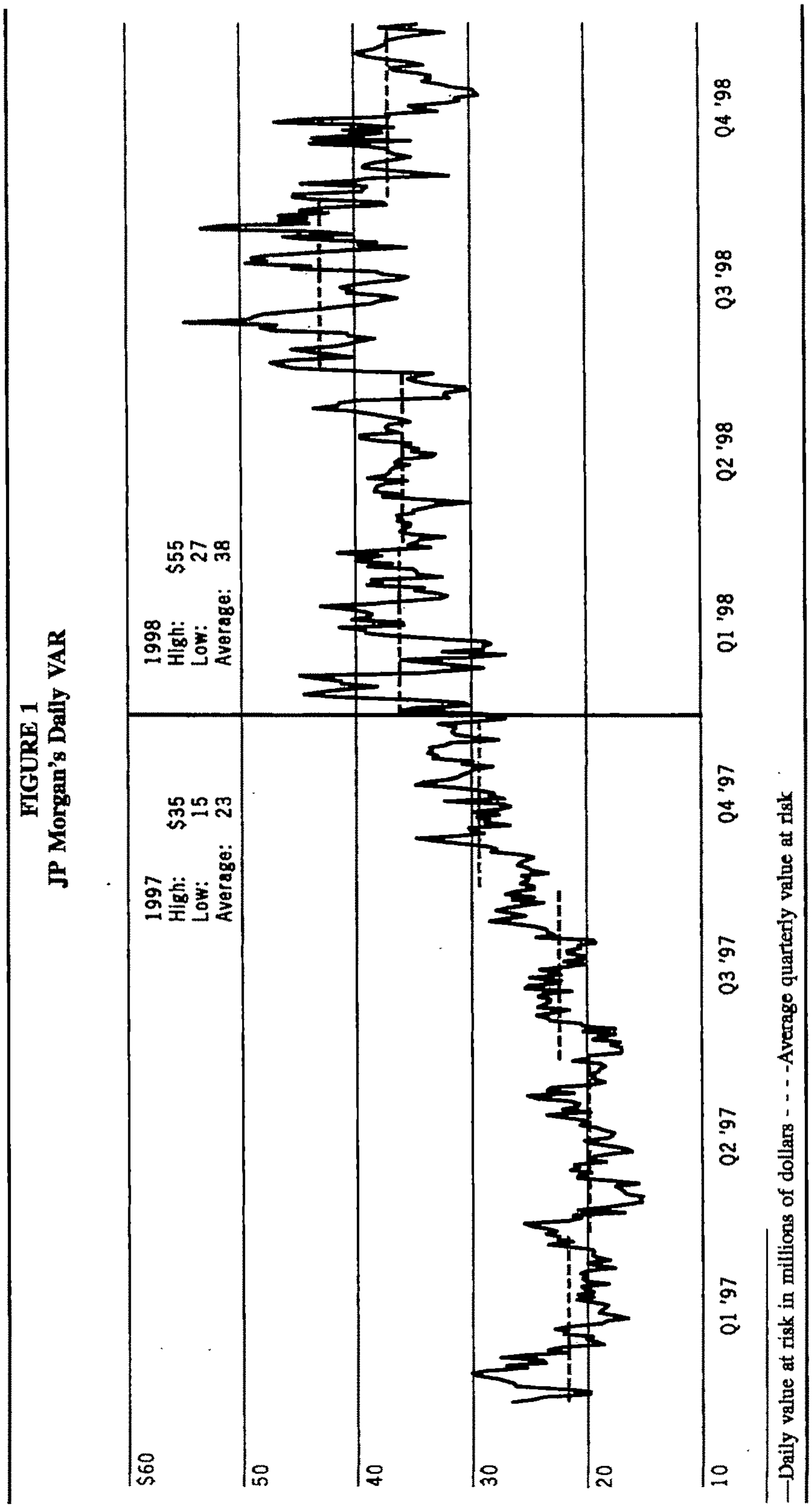
FIGURE 1
JP Morgan's Daily VAR

variation in $|R_{t+1} - E[R_{t+1}]|$. The regression will be meaningful only if σ_t varies substantially across quarters. If a bank maintains a roughly constant risk profile throughout the six-year period, then a simple time-series regression will yield an insignificant estimate for b, with the constant capturing the average value of $|R_{t+1} - E[R_{t+1}]|$ instead.

III. DATA AND DESCRIPTIVE STATISTICS

The Bank Sample

The key criterion for including banks in this sample is the availability of publicly disclosed VAR data. I initially focused on the U.S. commercial banks with the largest derivatives positions, as reported by the Office of the Comptroller of the Currency over the period 1995 to 1999. Twelve banks appeared in the top ten ranking at some point over the 1995–1999 period. These banks have large proprietary trading operations and are more likely to have established, centralized risk-management systems that enable them to report VAR numbers. Banks with large trading operations are exposed to a variety of risk, including interest rate, currency, equity, and commodity risk. This makes it necessary for them to measure risk on an aggregate basis.

Of these 12 banks, the eight in the final sample issued VAR disclosures on a quarterly or annual basis beginning in December 1994.[13] To avoid survivorship bias, I keep NationsBank, which merged into BankAmerica during the sample period. Panel A of Table 1 provides more details on the type and frequency of the banks' VAR disclosures, including the method the bank used to compute VAR, and the confidence level.[14]

Figure 2 shows an example of Chase Manhattan Bank's VAR disclosure. Chase measures VAR at the 99 percent level of confidence over one day. The bank also gives general information about how it estimates VAR. Chase states that it is exposed to interest rate, foreign exchange, equity, and commodity market risk in its trading portfolio. The breakdown by type of risk indicates that this is a diversified portfolio. Chase reports the average, high, low, and end-of-period VAR across categories of market risk and for the portfolio as a whole. This is one of the more extensive VAR disclosures. Other banks' disclosures are typically less informative, although the quality of information has improved over time.

Panel B in Table 1 reports the book value of total assets and equity, which can be compared to the banks' trading risk, at the beginning and end of the sample period. The banks' positions in off-balance-sheet derivatives provide an indication of their potential trading risk. The table shows that the banks' derivatives notional positions grew by about 112 percent on average, which is triple the 40 percent growth in assets and equity. Most of these banks' notional positions exceed \$1 trillion, which is far larger than book equity, which is on the order of tens of billions of dollars. Notional amounts, however, do not reveal the risks banks actually face. Market risk could be minimal if the banks hedged all positions back-to-back. If unhedged, however, these banks could be assuming large trading risks. The VAR measure is designed to account for hedging and diversification effects.[15]

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[13] For an overview of different VAR methods, see, e.g., Linsmeier and Pearson (2000) or Jorion (2000).

[14] Smaller banks reported less information, if any, about VAR. Regional banks are mainly exposed to U.S. interest rate risk and instead report a sensitivity analysis to shocks in interest rates. FleetBoston and First Union did not begin providing VAR disclosures until 1996 and 1997, respectively. Neither Republic nor Wells Fargo report VAR.

[15] To some extent, one could assess a bank's market risk profile from the historical volatility of its reported trading revenues. However, historical volatility is a backward measure of risk because exposures can change over time. Banks can change their derivatives strategies, exiting some markets or entering new ones. Thus, VAR is designed to provide a forward-looking gauge of market risk.

TABLE 1
Bank Disclosures

Panel A: Description of VAR Disclosures for the 8 U.S. Commercial Banks with the Largest Derivatives Positions and That Report VAR Beginning in 1994

Bank	*Description of Method as of Dec. 1999*	*Reporting Details as of Dec. 1999*	*First VAR Disclosure*	*Reported Average VAR Since*	*Reported End-quarter Var Since*	*Confidence Level*
Bank of America	"Sophisticated techniques"	Average, min, max	Dec-94	1998-Q3	NA	95%, 97.5%, 99% since Sept.-98
Bank of New York	Monte Carlo simulations	Average, end, min, max	Dec-94	1996-Q4	1997-Q4	95%, 99% since Mar.-98
Bankers Trust	"Proprietary simulations"	Average, end, min, max	Dec-94	1998-Q3	1998-Q3	99%
Chase Manhattan	Historical simulation	Average, end, min, max	Dec-94	1994-Q4	1997-Q4	95%, 97.5%, 99% since Dec.-97
Citicorp	Covariance matrix	Average, end, min, max	Dec-94	1997-Q4	1997-Q4	97.7%, 99% since Dec.-97
First Chicago	Not described	Average, end, min, max	Dec-94	1994-Q1	1997-Q4	99.87%, 99% since Dec.-97
J.P. Morgan	Historical simulation	Average, end, min, max	Dec-94	1995-Q1	1997-Q4	95%
NationsBank	Scenario simulation	End	Dec-94	NA	1996-Q1	99%

Panel B: Banks' Book Value of Assets and Equity, and Derivatives Notional in 1994 and 1999 ($ Billions)

	Book Value of Assets			*Book Value of Equity*			*Derivatives Notional*		
Bank	*1994*	*1999*	*Growth*	*1994*	*1999*	*Growth*	*1994*	*1999*	*Growth*
Bank of America	$170	$633	272%	$11.0	$44.4	304%	$1,333	$5,672	326%
Bank of New York	$49	$75	53	$4.3	$5.1	19	$80	$318	298
Bankers Trust	$97	$68	−30	$4.9	$4.3	−12	$1,982	$323	−84
Chase Manhattan	$285	$406	42	$18.9	$23.6	25	$4,566	$12,720	179
Citicorp	$250	$389	56	$17.8	$26.0	46	$2,265	$3,712	64
First Chicago/BankOne	$113	$269	138	$7.8	$20.1	158	$622	$1,034	66
J.P. Morgan	$155	$261	68	$9.6	$11.4	19	$2,489	$8,839	255
NationsBank	$158	$308	95	$11.0	$26.7	143	$485	$2,355	386
Total	$1,277	$1,776	39	$85	$117	37	$13,822	$29,301	122

(*Continued on next page*)

TABLE 1 (Continued)

Panel C: Banks' Quarterly Trading Revenues ($ Millions) and VAR-Based Quarterly Volatility from 1995 to 2000 ($ Millions)

	Trading-Related Revenue		*Total Revenue*		*Unexpected Trading Revenue*			*VAR-Based Quarterly Volatility*	
Bank	*Average*	*Standard Deviation*	*Average*	*Percent Trading*	*Average*	*Standard Deviation*	*Rho(1)*	*Average*	*Standard Deviation*
Bank of America	$253	$219	$8,695	2.9	$38	$226	0.15	$94	$45
Bank of New York	$35	$19	$1,524	2.3	$6	$8	0.10	$16	$7
Bankers Trust	$188	$235	$2,265	8.3	−$18	$229	−0.03	$70	$30
Chase Manhattan	$564	$218	$7,703	7.3	$54	$186	−0.19	$74	$13
Citicorp	$591	$150	$9,456	6.3	$44	$125	−0.27	$67	$16
First Chicago/BankOne	$64	$29	$3,847	1.7	$4	$31	−0.10	$45	$7
J.P. Morgan	$718	$297	$4,348	16.5	$75	$258	−0.04	$129	$36
NationsBank	$75	$23	$4,959	1.5	$4	$27	−0.17	$67	$15

In Panel B, values in 1994 and 1999 are taken from annual reports as of December. "Growth" refers to the total growth rate of these items over the period. Deutsche Bank acquired Bankers Trust and made Bankers Trust a wholly owned subsidiary in June 1999. Chase Manhattan merged with Chemical Bank on March 31, 1996; prior data are restated for the merged entity. First Chicago NBD merged with BancOne on October 2, 1998 to become Bank One; data prior to the merger is that of First Chicago NBD, which had relatively large derivatives positions. NationsBank merged with Bank of America in September 1998 so it disappeared from the sample after June 1998. The "total" line excludes NationsBank in 1999 because another bank in the sample (Bank of America) absorbed its assets.

Panel C presents averages and standard deviations from the first quarter of 1995 to the third quarter of 2000, except for NationsBank, for which data end in June 1998. "Unexpected trading revenues" is defined as the difference between the latest trading revenue and the average over the previous year. Rho(1) is the first-order autocorrelation in the unexpected trading revenues, for which the standard error is 0.21.

FIGURE 2

Excerpts from the Annual Report of Chase Manhattan Bank, 1999 Dealing with Value at Risk

Value-at-Risk

VAR is a measure of the dollar amount of potential loss from adverse market moves in an everyday market environment. The VAR looks forward one trading day and is the loss expected to be exceeded with a 1 in 100 chance.

The VAR methodology used at Chase is historical simulation, which assumes that actual observed historical changes in market indices, such as interest rates, foreign exchange rates, and equity and commodity prices, reflect future possible changes. In its daily VAR calculations, Chase uses the most recent one-year historical changes in market prices and rates. Chase's historical simulation is applied to end-of-day positions, and it is shown by individual position and by aggregated positions by business, geography, currency, and type of risk.

Trading Activities

Chase is exposed to interest rate, foreign exchange, equity, and commodity market risk in its trading portfolios. No single risk statistic can reflect all aspects of market risk; in addition, market risk exposures change continuously through daily trading activities. Nonetheless, the tables that follow provide a meaningful overview of Chase's market risk exposure arising from its trading activities.

The table that follows represents Chase's average and period-end VARs for its total trading portfolio and for each of the major components constituting that portfolio.

	Year Ended December 31, 1999			*At Dec. 31, 1999*	*At Dec. 31, 1998*
(in millions)	*Average VAR*	*Minimum VAR*	*Maximum VAR*	*VAR*	*VAR*
Interest Rate	$20.2	$10.7	$36.5	$20.0	$20.1
Foreign Exchange	7.0	2.3	21.3	3.0	2.3
Equities	6.3	3.4	10.1	7.2	4.6
Commodities	3.5	1.9	9.0	3.4	2.6
Hedge Fund Investments	4.1	3.1	4.6	3.3	NA
Less: Portfolio Diversification	(17.0)	NM	NM	(13.7)	(8.9)
Total VAR	$24.1	$12.3	$41.8	$23.2	$20.7

NA = not available.
NM = not meaningful.
Chase stated reporting in 1999 its market risk exposure to hedge fund investments as a separate VAR category. Prior to that, the market risk of hedge funds was in other categories.

Trading Revenues

I collected quarterly trading revenues from banks' quarterly reports (10-Q) and annual reports (10-K), along with the banks' VAR disclosures. Because banks begin disclosing VAR data in 1994, I collect trading revenue data from 1994 to the third quarter of 2000.

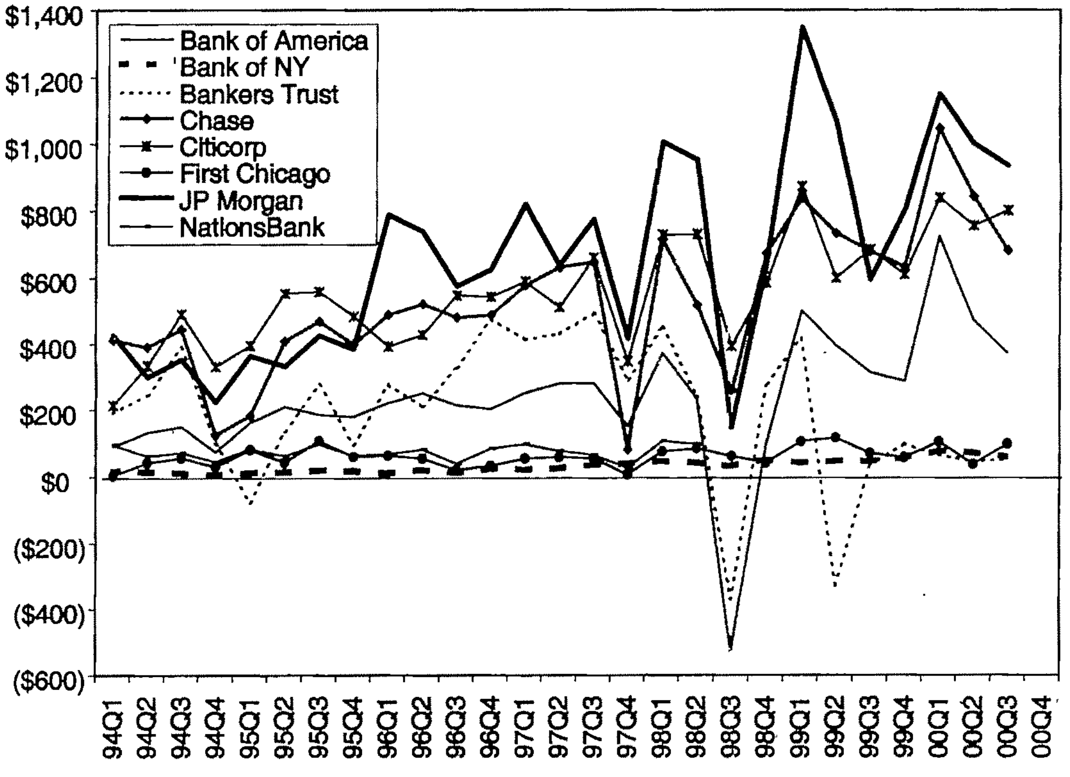
Most banks report "total trading-related venues," which includes (1) direct trading revenues, (2) fees, and (3) interest revenues on trading assets net of the costs to fund trading positions. Fees and net interest earned on assets and liabilities are more stable over time than are trading revenues. As explained earlier, I therefore abstract from the expected component of trading revenues to construct a measure of unexpected trading revenues.

Panel C in Table 1 reports the average and standard deviation of quarterly trading-related revenues, measured in millions of dollars, as well as statistics on the measure of unexpected trading revenues. Average trading revenues differ widely across banks, ranging from a low of $35 million for Bank of New York to $718 million for J.P. Morgan. The third column reports the banks' total revenues, one component of which is trading revenues. For some banks, trading revenues are an insignificant proportion of total revenues. For Bank of New York, for instance, the ratio is only 2.3 percent. For other banks, the ratio is much higher, reaching 16.5 percent of revenues for J.P. Morgan.

Figure 3 shows an upward trend in the banks' quarterly trading-related revenues. Volatility is also high, with some banks losing money during the third quarter of 1998. This was a period of turmoil in financial markets, beginning with the Russian default and leading to the debacle of the hedge fund Long-Term Capital Management (LTCM). Financial markets were severely disrupted when it appeared that LTCM might collapse, due to the sheer size of LTCM's positions.

Panel C in Table 1 shows that average unexpected trading revenues are much closer to zero than are raw trading revenues. Consistent with the assumption underlying my computation of unexpected trading revenues, I am unable to reject the hypothesis that the banks' mean unexpected trading revenues equal zero, except for Bank of New York. Also, unexpected trading revenues have no detectable first-order autocorrelation.

FIGURE 3
Bank's Quarterly Trading Revenues, 1994–2000



Value at Risk

Firms normally measure VAR over a one-day horizon, assuming the current positions are fixed over the daily horizon. In practice, of course, traders change positions actively during the trading horizon. Moreover, the actual risk may be less than indicated by VAR if management takes corrective actions when losses begin to develop. Using a quarterly horizon exacerbates the noise created by assuming positions are fixed throughout the entire horizon. As a result, I cannot identify rapid changes in banks' risk profiles. Rather, my goal is to identify slow changes in risk profiles across time, as well as to compare the magnitude of risk profiles across banks.

Ideally, I would match end-of-quarter VAR data with subsequent unexpected trading revenue. I collected VAR data for trading activities from the banks' annual (10-K) and quarterly (10-Q) reports. Banks began disclosing annual VAR numbers in 10-K forms in 1994. Because the SEC's market risk disclosure rule FFR-48 was not effective until January 1, 1998, quarterly VAR disclosures are unavailable for some banks until 1998, as Panel A of Table 1 indicates. In some cases, I recovered quarterly VAR from daily VAR graphs published in annual reports. In addition, many banks report VAR only as an average or an end-of-period value, but not both. As a result, I use the following decision rule in collecting VAR measures:

1) End-of-quarter VAR data for the previous quarter when available.
2) Average VAR for the previous quarter.

If quarterly data are not available, I resort to annual data.

3) VAR reported at the end of the previous year.
4) Average VAR for the previous year.

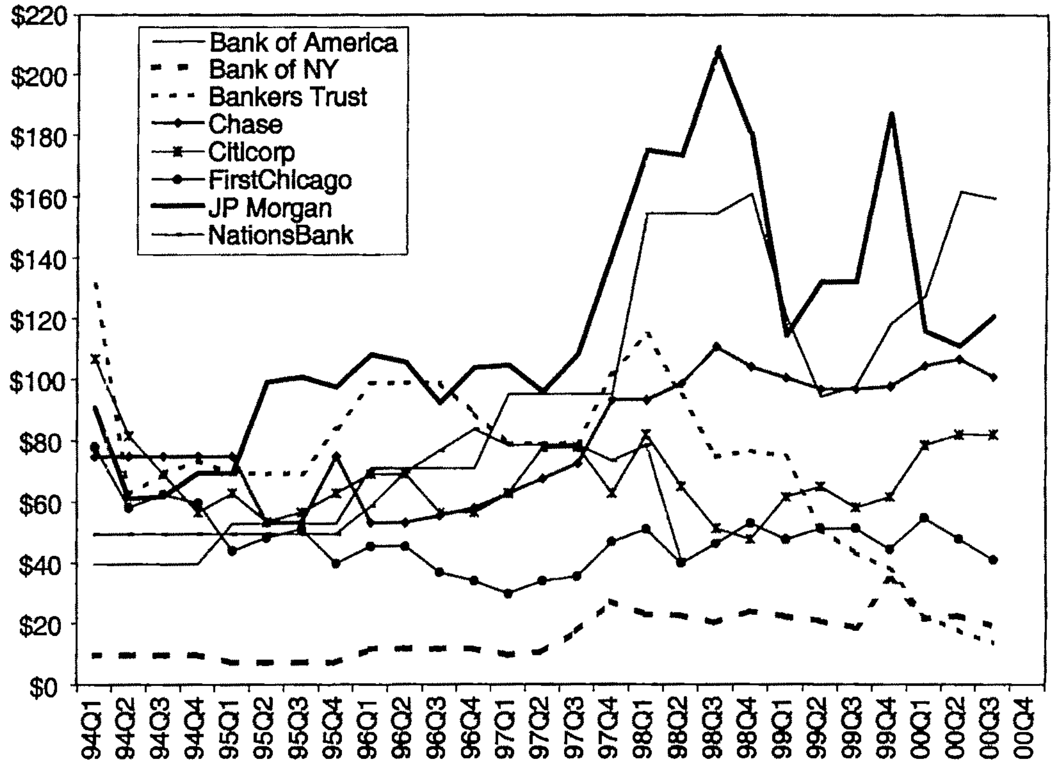
This approach selects the VAR number that is the closest to the beginning of the subsequent quarter over which I measure trading revenue. Using annual VAR data instead of quarterly data, however, should reduce the accuracy of the risk forecast because in the regressions (Equation 7) four quarterly trading revenue observations correspond to the same value of the VAR-based independent variable.

Banks report VAR data in different formats, with different reporting horizons and different confidence levels. I converted these into a quarterly standard deviation assuming normal distributions and identically and independently distributed returns using Equations (2) and (5). (As stated previously, the relation between the absolute value of trading revenues and the measure of dispersion holds for a large class of distributions, not just the normal distribution.)

The two right-most columns in Panel C of Table 1 describe average risk measures (i.e., the VAR-based forecasted volatility of trading revenues) and the standard deviations of these measures over the sample period. Risk profiles differ widely across banks. The average quarterly measure of VAR-based volatility ranges from a low of $16 million for Bank of New York to a high of $129 million for J.P. Morgan.

Figure 4 compares the risk measures for the banks in the sample over 1995 to 2000. The figure shows substantial variation across banks and, for some banks, over time. The VAR-based risk measures of J.P. Morgan and Bank of America have tripled over the period. In contrast, Chase's VAR-based risk measure has only increased from $80 million to $100 million, although its notional derivatives position has doubled. For some banks, there is limited time-series variation in the VAR measure, which will decrease the power of time-series regressions for these individual banks.

FIGURE 4
VAR-Based Quarterly Volatility in Banks' Trading Revenues, 1994–2000



IV. RESULTS

I test the informativeness of VAR measures by estimating the relation between the VAR-based quarterly volatility and the absolute value of the unexpected trading revenue in the subsequent quarter. I first estimate the regression in Equation (7) individually for each bank i.

Table 2 reports the results of the eight bank-specific time-series regressions of unexpected trading revenue on VAR-based quarterly volatility. The table shows results based on univariate Ordinary Least Squares (OLS) and Seemingly Unrelated Regression (SUR) regressions. The SUR method leads to slightly lower standard errors, but can be applied to only seven banks. I excluded NationsBank from the SUR analysis because it requires the same sample period for all series used to compute the covariance matrix.

The individual bank-specific regressions reveal some evidence that forecasts of volatility in unexpected trading revenues derived from VAR disclosures are positively associated with the magnitude of the next quarter's unexpected trading revenues. The relation is significant for the two banks with the greatest time-series variation in VAR-based volatility: Bank of America and J.P. Morgan. The relation is also significant for First Chicago, which was one of the earliest disclosures of quarterly VAR data. In contrast, Figure 4 shows that other banks, such as Bank of New York and Citicorp, have little time-series variation in VAR-based volatility, which makes it difficult to identify a reliable relation.

TABLE 2
Bank-Specific Regressions of Absolute Value of Quarter t + 1 Unexpected Trading Revenues on Quarter t VAR-Based Volatility

$$|R_{i,t+1} - E[R_{i,t+1}]| = a_i + b_i\sigma_{i,t} + \epsilon_{i,t+1}$$

	Period	*OLS*			*SUR*	
Bank	*(Number of Observations)*	*Constant* *(t-statistic)*	*Slope*	R^2	*Constant* *(t-statistic)*	*Slope*
Bank of America	1995,Q1–2000,Q3	−73.48	2.24*	28.0%	13.48	1.29*
	(23)	(−0.94)	(2.86)		(0.24)	(2.71)
Bank of New York	1995,Q1–2000,Q3	4.16	0.25	6.9%	4.00	0.26
	(23)	(1.21)	(1.24)		(1.26)	(1.43)
Bankers Trust	1995,Q1–2000,Q3	47.59	1.38	4.9%	56.15	1.26
	(23)	(0.46)	(1.03)		(0.74)	(1.38)
Chase	1995,Q1–2000,Q3	−80.29	3.12*	12.1%	−34.68	2.51*
	(23)	(0.58)	(1.70)		(−0.31)	(1.67)
Citicorp	1995,Q1–2000,Q3	118.84	−0.12	0.0%	66.22	0.67
	(23)	(1.74)	(−0.12)		(1.41)	(1.00)
First Chicago/ Bank One	1995,Q1–2000,Q3	−17.68	0.97*	19.5%	−22.17	1.07*
	(23)	(−0.89)	(2.25)		(−1.31)	(2.91)
J.P. Morgan	1995,Q1–2000,Q3	−65.15	1.99*	15.1%	20.02	1.32*
	(23)	(−0.48)	(1.95)		(1.32)	(2.08)
NationsBank	1995,Q1–1998,Q2	−2.44	0.36	10.7%	—	—
	(14)	(−0.12)	(1.20)			
Joint test of zero slopes						24.26*
						prob = 0.001

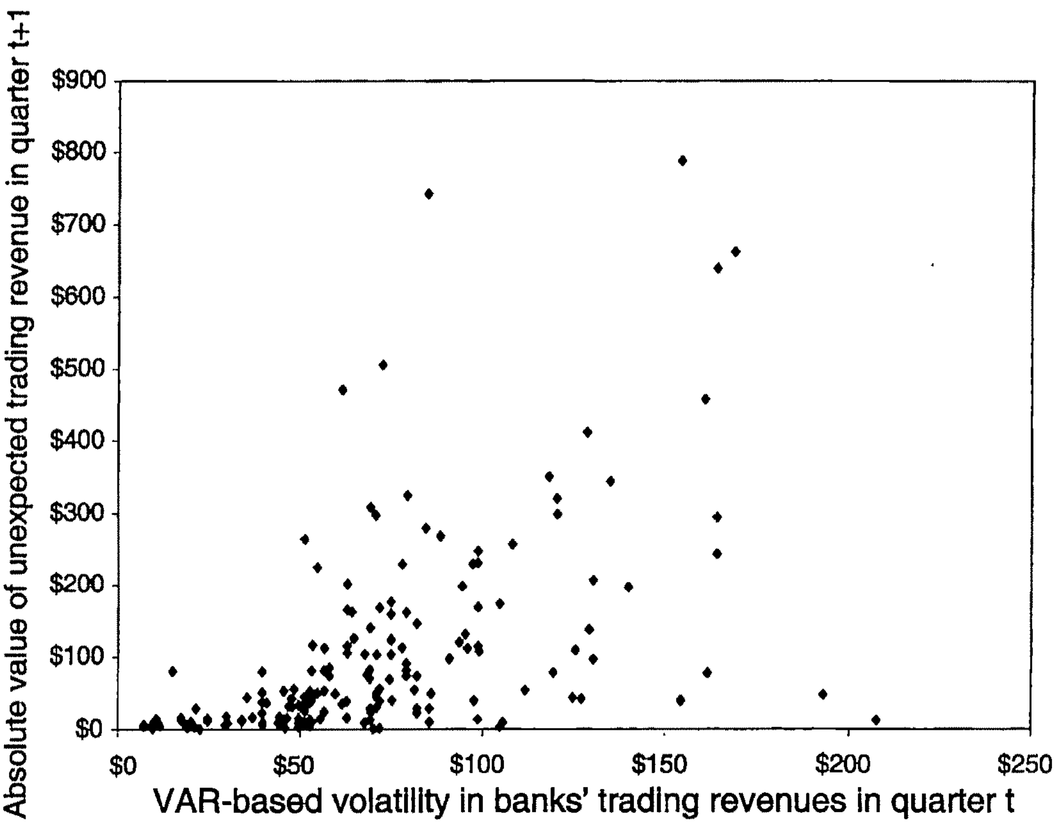
$R_{i,t+1}$ is the quarterly trading revenue, $E[R_{i,t+1}]$ is the expected trading revenue in quarter t + 1 based on a moving average of the last four quarters, and $\sigma_{i,t}$ is the VAR-based volatility in unexpected trading revenue for bank i in quarter t. The table reports OLS and Seemingly Unrelated Regressions (SUR) regressions for each bank; NationsBank is not included in the SUR regressions due to its shorter sample period. The joint test of zero slope coefficients is distributed asymptotically as Chi-square with 7 degrees of freedom.
One-tailed significance at the 5 percent level denoted by *.

The bottom of Table 2 reports the result of a joint test of significance of all seven banks' SUR slope coefficients. The null hypothesis of zero slope coefficients is strongly rejected, at $p < 0.001$. As a whole, I find that VAR-based volatility is positively associated with the variation in future unexpected trading revenues.

Figure 5 displays the pooled sample, which is similar to an analyst comparing risk profiles across firms. Higher VAR-based volatility is associated with greater variation in unexpected trading revenues.

Table 3 reports the results of a cross-sectional estimation of Equation (7). Panel A first reports the results of a "pooled sample-OLS" regression with 175 observations (seven banks with 23 quarters of data plus NationsBank with 14 quarters). The slope coefficient on the VAR-based volatility is 1.80, which is significant at $p < 0.001$. The regression R^2 is 26 percent.

FIGURE 5
Absolute Value of Banks' Unexpected Trading Revenues in Quarter t + 1 and VAR-Based Volatility in Quarter t: Pooled Sample



To correct for heteroskedasticity (evident from the increasing variance as the VAR-based volatility increases in Figure 5) and cross-sectional correlations (e.g., Figure 3 shows that many banks suffered drops in trading revenues during the third quarter of 1998), I use a two-step GLS approach. First, I estimate the covariance matrix of the error terms from the individual regressions. Call this covariance matrix $\hat{\Sigma}$. I construct the Cholesky factorization as $\hat{\Sigma}^{-1} = P'P$, where P is a lower triangular matrix. Next, I transform the observations by pre-multiplying Equation (7) by P so that the error term has unit variance. This approach corrects for estimated correlations and also puts greater relative weight on banks with lower variability in trading revenues. I run the regression on the transformed variables, and report the GLS results on the second line in Table 3. The estimated coefficient on the VAR-based volatility is 0.57, highly significant, and in line with the slope of 0.80 expected from Equation (3), allowing for measurement errors in the variables. The regression R^2 in the transformed space is 38 percent.

I also implemented a two-step time-series approach as in Fama and MacBeth (1973), which is more intuitive and does not require estimating the covariance matrix. I first estimate a cross-sectional regression every quarter, and then aggregate the coefficients $\hat{b}_t$ over time.

TABLE 3
Pooled Regressions of Absolute Value of Quarter t + 1 Unexpected Trading Revenues on Quarter t VAR-Based Volatility

$$|R_{i,t+1} - E[R_{i,t+1}]| = a + b\sigma_{i,t} + \epsilon_{i,t+1}$$

Sample	Period (Number of Observations)	Constant (t-statistic)	Slope (t-statistic)	R^2
Panel A: Total Sample				
Pooled Sample-OLS	1995,Q1–2000,Q3	−23.97	1.80*	26.4%
(8 banks)	(175)	(−1.31)	(7.89)	
Pooled Sample-GLS	1995,Q1–2000,Q3	−1.88	0.57*	37.7%
(7 banks)	(161)	(−0.90)	(6.24)	
Time-Series	1995,Q1–2000,Q3		1.67*	
(Fama-MacBeth)	(19)		(4.92)	
Panel B: Sample without 1998, Q3				
Pooled Sample-OLS	1995,Q1–2000,Q3	−4.56	1.32*	21.4%
(8 banks)	(168)	(−0.29)	(6.72)	
Pooled Sample-GLS	1995,Q1–2000,Q3	1.82	0.50*	43.0%
(7 banks)	(154)	(0.08)	(6.79)	
Time-Series	1995,Q1–2000,Q3		1.44*	
(Fama-MacBeth)	(18)		(5.32)	
Panel C: Sample without 1998,Q3 and 1999,Q1				
Pooled Sample-OLS	1995,Q1–2000,Q3	−3.11	1.31*	19.5%
(8 banks)	(154)	(−0.19)	(6.08)	
Pooled Sample-GLS	1995,Q1–2000,Q3	−0.09	0.57*	36.0%
(7 banks)	(140)	(−0.56)	(7.35)	
Time-Series	1995,Q1–2000,Q3		1.45*	
(Fama-MacBeth)	(16)		(5.64)	

$R_{i,t+1}$ is the quarterly trading revenue, $E[R_{i,t+1}]$ is the expected trading revenue in quarter t + 1 based on a moving average of the last four quarters, and $\sigma_{i,t}$ is the VAR-based volatility in unexpected trading revenue for bank i in quarter t. The OLS regression is a simple pooled time series and cross-sectional regression. The pooled GLS regression transforms the variables by the estimated covariance matrix; it accounts for heteroskedasticity and correlation in the series. The Fama and MacBeth (1973) approach consists of running cross-sectional regressions for each quarter and then taking the time-series average of estimated coefficients.

Panel A uses the total sample. Panel B drops the third quarter of 1998 from the sample. Panel C drops the third quarter of 1998 and the first quarter of 1999 from the sample.

One-tailed significance at the 5 percent level denoted by *.

This approach corrects for correlations across banks. Table 3 reports an estimated coefficient on the VAR-based volatility of 1.67, similar to the OLS value and still highly significant. Overall, the results indicate that VAR-based volatility is significantly positively associated with the magnitude of future unexpected trading revenues.

Next, Panels B and C in Table 3 show that the inferences are not attributable to extreme observations in the third quarter of 1998. Panel B shows that dropping the quarter entirely from the sample decreases the estimated coefficients on the VAR-based volatility forecast, but they remain significant at $p < 0.001$. Panel C shows that dropping the third quarter of 1998 to the first quarter of 1999 does not affect the inferences.

The strongest results arise in the cross-sectional analysis. This raises the question of whether the results could be due to a scale effect, i.e., simply due to the fact that both VAR-based volatility and the volatility of trading revenues are measured in dollars. Moreover, market participants and researchers should be interested in whether quantitative VAR disclosures contain useful information for predicting the volatility of unexpected trading revenues, above and beyond the information in traditional notional amounts. To address these questions, I estimate the following regression:

$$|R_{i,t+1} - E[R_{i,t+1}]| = a + b\sigma_{i,t} + cNOT_{i,t} + \epsilon_{i,t+1}, \tag{8}$$

where $NOT_{i,t}$ is the outstanding notional value of bank i's derivatives contracts at the end of the previous quarter.[16] If VAR provides incremental information for predicting the volatility of unexpected trading revenues, then we would expect the coefficient b to remain significantly positive.

Table 4 reports the results from pooled cross-sectional regression on the notional positions alone, as well as combined with the VAR-based volatility forecast. Panel A shows that the magnitude of the banks' derivatives positions is significantly positively related to the volatility in unexpected trading revenues. More interestingly, however, Panel B shows

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[16] Including in the regression total assets or trading assets rather than the notional amounts led to similar inferences, except that the estimated coefficients on assets were even weaker than those obtained for notional amounts.

TABLE 4

Pooled Regressions of Absolute Value of Quarter t + 1 Unexpected Trading Revenues on Quarter t VAR-Based Volatility and Notionals

Sample	*Period (Number of Observations)*	*Constant*	$\sigma_{i,t}$ *Slope (t-statistic)*	*Notional Slope*	R^2
Panel A: $\lvert R_{i,t+1} - E[R_{i,t+1}]\rvert = a + cNOT_{i,t} + \varepsilon_{i,t+1}$					
Pooled Sample-OLS	1995,Q1–2000,Q3	46.63		0.0176*	14.9%
(n = 8)	(175)	(3.34)		(5.51)	
Pooled Sample-GLS	1995,Q1–2000,Q3	4.26		0.0187*	40.5%
(n = 7)	(161)	(3.59)		(7.51)	
Time Series	1995,Q1–2000,Q3			0.0210*	
(Fama-MacBeth)	(19)			(4.62)	
Panel B: $\lvert R_{i,t+1} - E[R_{i,t+1}]\rvert = a + b\sigma_{i,t} + cNOT_{i,t} + \epsilon_{i,t+1}$					
Pooled Sample-OLS	1995,Q1–2000,Q3	−23.48	1.54*	0.0055	27.4%
(n = 8)	(175)	(−1.29)	(5.43)	(1.49)	
Pooled Sample-GLS	1995,Q1–2000,Q3	−1.17	0.36*	0.0117*	43.0%
(n = 7)	(161)	(−0.54)	(3.42)	(3.94)	
Time Series	1995,Q1–2000,Q3		1.09*	0.0110*	
(Fama-MacBeth)	(19)		(2.38)	(1.93)	

(*Continued on next page*)

TABLE 4 (Continued)

Sample	*Period (Number of Observations)*	*Constant*	$\sigma_{i,t}$ *Slope* *(t-statistic)*	*Notional Slope*	R^2
Panel C: $\lvert R_{i,t+1} - E[R_{i,t+1}]\rvert/A_{i,t} = a + b(\sigma_{i,t}/A_{i,t}) + \epsilon_{i,t+1}$					
Pooled Sample-OLS	1995,Q1–2000,Q3	3.25	1.32*		13.9%
(n = 8)	(175)	(0.31)	(5.29)		
Pooled Sample-GLS	1995,Q1–2000,Q3	3.21	0.44*		41.0%
(n = 7)	(161)	(0.87)	(3.45)		
Time Series	1995,Q1–2000,Q3		1.52*		
(Fama-MacBeth)	(19)		(2.94)		
Panel D: $\lvert R_{i,t+1} - E[R_{i,t+1}]\rvert/A_{i,t} = a + b(\sigma_{i,t}/A_{i,t}) + c(NOT_{i,t}/A_{i,t}) + \epsilon_{i,t+1}$					
Pooled Sample-OLS	1995,Q1–2000,Q3	−13.68	1.09*	0.0192*	19.3%
(n = 8)	(175)	(−1.22)	(4.31)	(3.39)	
Pooled Sample-GLS	1995,Q1–2000,Q3	4.46	0.32*	0.0213*	41.6%
(n = 7)	(161)	(1.18)	(2.30)	(5.10)	
Time Series	1995,Q1–2000,Q3		1.35*	0.0140*	
(Fama-MacBeth)	(19)		(2.59)	(2.31)	

$R_{i,t+1}$ is the quarterly trading revenue, $E[R_{i,t+1}]$ is the expected trading revenue in quarter t + 1 based on a moving average of the last four quarters, $\sigma_{i,t}$ is the VAR-based volatility in unexpected trading revenue for tank i in quarter t, and $NOT_{i,t}$ is the notional derivatives amount for bank i in quarter t. In Panels C and D, the variables are scaleld by the previous quarter's assets $A_{i,t}$. See Table 3 for explanations of pooled OLS, GLS, and time-series regressions. One-tailed significance at the 5 percent level denoted by *.

that notional amounts have little explanatory power beyond the information contained in the VAR-based volatility forecast. The R^2s are not much higher than those in Panel A in Table 3. In contrast, VAR-based volatility forecasts provide incremental new information about the volatility in unexpected trading revenues, after controlling for derivatives notional amounts.

As a further control for scale effects, Panels C and D report the results of estimating the regressions after scaling all variables by the previous quarter's assets. Panel C in Table 4 confirms that, even after scaling, there is a significantly positive relation between the magnitude of quarter t + 1 unexpected trading revenues and the VAR-based measure of volatility in quarter t. Panel D indicates that the estimated coefficient on VAR-based volatility is still significant after controlling for the banks' notional amounts. Thus this relation is not simply a scale effect.

V. CONCLUSIONS

This study investigates the relation between value-at-risk (VAR) measures for trading activities commercial banks disclose in their financial reports and the subsequent variability of their unexpected trading revenues. The informativeness of VAR disclosures in firms' financial reports is of considerable interest, given the SEC's recent requirement that corporations must disclose quantitative measures of their market risks.

After conducting informal surveys, the SEC (1998) concluded that these disclosures provide investors and analysts with "new and useful information." Until now, however, there has been no formal attempt to assess whether VAR measures banks disclose in their financial reports can predict future volatility in their unexpected trading revenues.

In addressing this question, I find that VAR-based volatility forecasts based on banks' publicly available VAR disclosures are significantly related to future market risk, especially in cross-sections. This suggests that analysts can meaningfully compare the risk profiles of different banks using their disclosed VARs.

Banks with low VAR measures appear to be exposed to limited downside risk, whether they report large or small notional derivatives positions. Banks with large VAR measures experience much greater fluctuations in unexpected trading revenues. Thus VAR measures published in banks' financial reports offer a useful predictor of the market risk of the banks' trading activities. Shareholders need to assess whether average trading revenues compensate for these risks.

However, even if I had found that VAR disclosures are not informative, the imposition of such disclosures could still improve the governance of derivatives. Calculation and public reporting of VAR numbers imposes a discipline on derivatives trading. It can also reassure investors that the bank has in place a process to measure and manage risk.

The quality of VAR disclosures should improve over time as methodologies become more consistent. Thus, we would expect VAR-based volatility forecasts to become increasingly accurate indicators of the variability of banks' future trading revenues. In the meantime, the most reassuring aspect of VAR reporting is that banks, finally forced to reveal the full extent of their market risk, now must consider the delicate trade-off between risk and return.

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The Effect of Shareholder-Level Dividend Taxes on Stock Prices: Evidence from the Revenue Reconciliation Act of 1993

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ABSTRACT: We investigate the effect of an increase in the individual (shareholder-level) income tax rate on share values. We regress cumulative daily abnormal stock returns surrounding the passage of the Revenue Reconciliation Act of 1993 on firm dividend yield, tax status of the investor as represented by level of institutional ownership, the interaction of these two variables, and control variables. Consistent with our expectations, we find that (1) the higher the firm's dividend yield, the more negative the firm's stock price reaction to the increase in the individual income tax rate (i.e., the dividend tax rate) enacted in the Revenue Reconciliation Act of 1993, and (2) institutional holdings mitigate this negative reaction. Our results suggest that both the dividend policy of the firm and the tax status of the marginal investor influence the extent to which dividend taxes are reflected in share values. Our evidence is consistent with the traditional view that firm dividend policy influences the extent to which tax rate changes affect share values.

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I. INTRODUCTION

The effect of shareholder-level taxes on stock prices is a fundamental issue for both tax policy and stock valuation. Shareholder-level taxes may affect share prices because these taxes are imposed on distributions of earnings (dividend taxes) and on the sale of shares through trades, share repurchases, and liquidating distributions (capital gains taxes), thus reducing after-tax cash flows to investors. The effect of shareholder-level taxes on share prices, however, is complicated because the amount of explicit shareholder-level taxes depends on the type of income realized (dividend or capital gain) and the timing of income recognition. Furthermore, the tax rate also depends on whether the shareholder is an individual, a corporation, or a tax-exempt entity (e.g., a pension fund). Not surprisingly, the extent to which shareholder-level taxes affect stock prices is a difficult empirical question that has generated decades of research without reaching consensus (e.g., Zodrow 1991; Erickson and Maydew 1998).

This study investigates the effects of shareholder-level dividend taxes on share value by analyzing stock price reactions to the increase in the maximum individual income tax rate (i.e., the dividend tax rate) from 31.0 to 39.6 percent enacted in the Revenue Reconciliation Act of 1993 (Public Law 103-66; hereafter RRA93). In particular, we investigate whether firm dividend policy and the tax status of the marginal investor influence the stock price reaction to an increase in the dividend tax rate. We expect that the higher the firm's dividend yield, the more negative the firm's stock price reaction to this tax increase. However, because RRA93 primarily increased the tax rates of individual investors, we expect the negative effect of the firm's dividend yield on event-period returns to be mitigated by the level of institutional ownership, a proxy for the likelihood that a stock's marginal investor is *not* an individual taxpayer. We test these hypotheses by regressing cumulative daily abnormal stock returns surrounding the passage of RRA93 on firm dividend yield, institutional ownership, the interaction of these two variables, and control variables. Consistent with our expectations, we find that the higher the firm's dividend yield, the more negative the stock price reaction over the five-day period that Congress enacted the increase in individual income tax rates in August 1993. In addition, we find that these returns are more negative for stocks in which the marginal investor is likely to be an individual taxpayer. Our results are robust to controls for risk, profitability, leverage, size, industry, and stock return differences related to dividend policy in nonevent weeks.

Our evidence that RRA93 adversely affected stock prices is consistent with earlier research reporting investor-level tax effects on the pretax returns of assets such as preferred stock (Erickson and Maydew 1998; Engel et al. 1999), municipal bonds (Poterba 1986; Mankiw and Poterba 1996), and Treasury bills (Guenther 1994). Our findings extend this literature by demonstrating that the tax-induced price effect on common stocks depends on both the firm's dividend policy and shareholder-level tax characteristics. Specifically, this study makes two contributions to the literature investigating the stock price effects of shareholder-level taxes in general, and of dividend taxes in particular.

First, our finding that the magnitude of the adverse price effect of RRA93 increases in dividend yield suggests that the extent to which changes in shareholder-level dividend taxes are impounded into stock prices depends on the market's expectations of a firm's dividend policy. This helps distinguish between the two competing theories of shareholder-level tax effects on share prices.[1] In brief, the "tax-capitalization" view (e.g., Auerbach 1979) predicts that a firm's dividend policy has no effect on the relation between shareholder tax rates and stock price because firm value is eventually distributed as taxable dividends. In contrast, the traditional or "tax-penalty" view posits that a firm's dividend policy affects the relation between shareholder tax rates and stock price because dividend distributions trigger shareholder taxes at ordinary income tax rates, whereas other distributions (such as share repurchases) are taxed at capital-gain tax rates. Although both views predict that increases in dividend tax rates will decrease share prices, they differ with respect to whether the firm's dividend policy affects the magnitude of the price decrease. To date, empirical evidence regarding the relation among tax rate changes, dividend policy, and share values has been inconsistent. Our finding that the adverse price effect of RRA93 increased in dividend yield supports the traditional view.

Second, our evidence demonstrates that the effect of shareholder-level dividend taxation on the value of a particular stock also depends on the tax status of the marginal investor in that stock. Although theoretical models of tax effects on stock prices recognize the importance of the marginal investor, empirical studies generally do not investigate or control for this factor because of the lack of shareholder-level information. For example, although Lang and Shackelford (2000) speculate that the price effects of shareholder-level capital gains taxes on common stocks will depend, in part, upon whether the marginal investor for a particular stock is an individual taxpayer, they do not provide evidence on this point. Indeed, tax research commonly assumes that the marginal investor is subject to the highest applicable tax rate (e.g., Collins and Kemsley 2000). To our knowledge, this is the first empirical study to use an exogenous proxy for the tax status of the marginal investor to demonstrate that a dividend tax rate change affects prices in a predictable manner.[2]

Our study is closely related to a concurrent working paper, Dhaliwal et al. (2002) (hereafter DLT), which examines long-window stock returns to test whether returns are higher for dividend-paying firms, consistent with the tax-penalty (i.e., traditional) view. Like our study, DLT use the level of institutional ownership as a proxy for the tax rate or tax status of the marginal investor, reasoning that institutional owners face, on average, lower tax rates on dividend income than do individual investors. Consistent with the tax-penalty view (and our findings in the control period), DLT find that dividend yield is positively associated with long-term stock returns and that this effect decreases in the level of institutional ownership. Nonetheless, a potential weakness of long-window returns studies (such as DLT) and recent price-level studies (e.g., Harris and Kemsley 1999) is that the results are susceptible to confounding factors such as risk. Our event-study methodology isolates the price effects of dividend policy and institutional ownership during a short event window around an unexpected increase in tax rates, thereby minimizing the effects of nontax factors. Hence, our research design triangulates both existing event studies (e.g., Lang and Shackelford 2000) and long-window return studies (e.g., DLT) by simultaneously incorporating a proxy for the tax status of the marginal investor and demonstrating the effect of dividend yield in a setting less susceptible to confounding factors.

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[1] Poterba and Summers (1985) and Zodrow (1991) provide thorough comparisons of the tax-capitalization and tax-penalty perspectives.

[2] Prior studies investigate whether the yield spread for municipal bonds reflects the tax rate for an individual or corporate marginal investor (Poterba 1986; Fortune 1988; Mankiw and Poterba 1996) and whether there is an institutional tax clientele for dividend-paying stocks (Dhaliwal et al. 1999; Seida 2001).

We organize the remainder of this study as follows. Section II describes our hypotheses and research method. Section III presents results. In the final section we explain the implications of our results for the competing theories that explain how shareholder-level taxes influence share values and describe the limitations of our study.

II. RESEARCH METHOD

Although both the tax-capitalization and traditional views predict that increases in dividend tax rates will decrease share prices, only the traditional view predicts that the firm's dividend policy affects the magnitude of the price decrease. A series of studies by Harris and Kemsley (1999), Collins and Kemsley (2000), and Harris et al. (2001) analyze the market values (price levels) of pooled cross-sectional samples and conclude that dividend taxes on retained earnings are fully impounded into share values, irrespective of a firm's dividend policy. Other studies criticize this conclusion (Shackelford and Shevlin 2001; Dhaliwal et al. 2001). A potential weakness of studies using price levels is that the observed effects of dividend policy might reflect differential returns for factors associated with dividends such as risk or earnings persistence (Hanlon et al. 2001). To minimize these potential confounding factors, we test for price effects associated with dividend policy during a short event window when tax rates unexpectedly increase.

If the traditional view is descriptive of how dividend tax rate increases influence stock prices, we would expect the market to react more negatively to the tax rate increase, the higher the firm's dividend yield. Because RRA93 primarily increased the tax rates of individual investors, we expect the negative association between dividend yield and returns to be concentrated in those firms owned primarily by individuals.[3] We use the level of institutional ownership as a proxy for the likelihood that a stock's marginal investor is *not* an individual taxpayer. Hence, we test the following hypothesis (stated in the alternative):

H1: The share price reaction to the dividend tax rate increase enacted in RRA93 is negatively related to dividend yield for stocks with low institutional ownership.

To test whether high institutional ownership mitigates the effect of the tax rate change, we posit the following conditional hypothesis (stated in the alternative):

H2: The negative relation between dividend yield and the share price reaction to the dividend tax rate increase enacted in RRA93 is mitigated by the level of institutional ownership.

We estimate the following regression model to test our hypotheses:

$$\begin{aligned} CAR_{it} = {} & \gamma_0 + \gamma_1 EVENT_t + \gamma_2 DIV_i + \gamma_3 INST_i + \gamma_4 DIV_i \times INST_i \\ & + \gamma_5 DIV_i \times EVENT_t + \gamma_6 DIV_i \times INST_i \times EVENT_t + \gamma_k X_{ki} + \varepsilon_{it} \qquad (1) \end{aligned}$$

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[3] RRA93 also increased the top corporate tax rate from 34 to 35 percent. After accounting for the standard 70 percent corporate-dividends-received deduction, this 1 percent tax rate increase corresponds to a maximum increase in tax rates on dividends of 0.3 percent for corporate investors. This change would tend to bias our tests against H2.

where:

CAR_{it} = cumulative abnormal returns for sample firm *i* determined using a standard market model cumulated over the nine five-day periods beginning four weeks prior to August 3, 1993, and ending four weeks after August 9, 1993;[4]

EVENT_t = an indicator equal to 1 if period *t* is August 3–9, 1993 (i.e., the RRA93-event period), and 0 for the control periods (four weeks preceding August 3, 1993, and four weeks following August 9, 1993);

DIV_i = an indicator equal to 1 for high dividend-yield firms and 0 for no-dividend firms or, in an alternative specification, a continuous dividend-yield variable calculated as a firm's common stock dividends for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992;[5]

INST_i = the percentage of firm *i's* common stock owned by institutional investors; and

X_{ki} = a vector of other variables controlling for firm attributes (e.g., firm size, profitability, leverage, and book-to-market ratio) that may be associated with stock returns during the sample period.

To test whether the change in the dividend tax rate affected stock prices, we first estimate daily abnormal returns for each sample firm using a standard market model and stock returns obtained from the Center for Research in Security Prices (CRSP). We then cumulate abnormal returns, CAR_{it}, over the five-day event period commencing on August 3, 1993, and ending August 9, 1993. This five-day period begins the day before the tax rate increase cleared the Joint Conference Committee and includes the days both houses of Congress narrowly approved the legislation (by a House vote of 218-216 on August 5 and a Senate vote of 51-50 on August 6). Although Congress debated this legislation for some time prior to the enactment of RRA93, ample anecdotal evidence indicates substantial doubt of passage.[6] Nonetheless, market anticipation in advance of our estimation period would tend to bias our tests against finding a significant effect of the tax rate change on stock prices. We control for the relation between dividend policy and stock returns outside our event period by cumulating abnormal returns over four five-day control periods on both sides of the event period. In other words, our regression includes nine cumulative abnormal return estimates for each sample firm, with four control observations preceding and another four control observations following the RRA93-event observation.

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[4] We estimate the market model parameters for each sample firm by regressing firm daily returns for calendar year 1992 on the value-weighted market index. We use market-adjusted abnormal returns despite the inclusion of many dividend-paying firms in the market return. Consequently, our tests should be biased against rejection because the market adjustment could remove part of the price effect we seek to estimate.

[5] Investors might expect firms to change their dividend policies as a result of the higher tax rate on dividends paid to individual investors. We also estimated our regressions using dividends paid during the fiscal year ending after December 1993 as a proxy for a firm's expected dividend yield. Results are essentially identical to those presented later in Table 2.

[6] Prior to the Joint Conference Committee, the Senate version of RRA93 passed by the narrowest of margins (i.e., 51-50) on June 25, 1993, relying on the tie-breaking vote of Vice President Gore. Passage of the Joint Conference Committee version remained doubtful until the final Senate vote (e.g., Calmes 1993). In sensitivity analysis, we repeated our analyses considering legislative dates prior to August 3, 1993, as event dates. Consistent with speculation that the passage of the tax rate increase was uncertain prior to the congressional votes in August 1993, we find no evidence of abnormal returns surrounding the legislative events prior to August 3, 1993. Nonetheless, it is possible that the market gradually impounded the promised tax rate increase during the legislative process or during the preceding presidential campaign. Indeed, in their investigations of political campaign rhetoric, Slemrod and Greimel (1999) and Ayers et al. (2002) present evidence that market prices reflect the tax rhetoric of opposing presidential candidates.

We use two alternative specifications of DIV to represent the dividend policy of sample firms. In the first specification DIV is an indicator variable equal to 1 if firm *i* is ranked in the top three deciles of dividend yield for the fiscal year ending prior to January 1993, and 0 for non-dividend-paying firms. To increase the distinction between the two groups identified by this indicator and thereby increase the power of our tests, we exclude from this analysis dividend-paying firms that are not ranked in the top three deciles of dividend yield.[7] Our alternative specification includes all sample firms and DIV is a continuous dividend-yield variable that we calculate as a firm's common stock dividends for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992.

INST represents the likelihood that the marginal investor in a firm's stock is not an individual. We obtain institutional ownership data as of December 31, 1992, for each sample firm using the CDA Spectrum database. This database reports institutions' common stock ownership at the end of each calendar quarter, based on SEC form 13(f) filings. Institutions include banks, broker-dealers and other investment advisers, college endowments, mutual funds, corporate investors, insurance companies, and pension and retirement funds. In general, institutional investors face lower tax rates on dividend income than individual investors because they are either tax exempt (e.g., college endowments, pension and retirement funds) or eligible for the corporate-dividends-received deduction (e.g., any corporate investor, including banks, broker-dealers, and insurance companies organized as corporations). INST is an imperfect proxy for the likely tax status of the firm's marginal investor because it includes (1) shares owned by individuals but held in the street name of their brokers and (2) shares owned by mutual funds that are, in turn, owned by individual investors in taxable accounts (as opposed to tax-deferred pension accounts). Noise in this measure would tend to bias our tests against the expected results.

Equation (1) allows us to estimate the effects of DIV and DIV × INST during the control weeks when expectations of future tax rates are presumably constant. Our analysis controls for these differences because, according to the traditional view described above, dividend-paying firms should earn higher pretax returns than non-dividend-paying firms during the control weeks to compensate taxable investors for the tax penalty created by dividend payments. The estimated coefficient for DIV represents the average difference in abnormal returns between dividend-paying and non-dividend-paying firms with no institutional ownership throughout the control period. The coefficient on DIV × INST represents the effect of increasing institutional ownership on the overall relation between DIV and abnormal returns during the control period. Consistent with DLT, we expect a positive coefficient on DIV and a negative coefficient on DIV × INST.

We test our first hypothesis with the estimated coefficient on DIV × EVENT, which we expect to be negative. This coefficient represents the incremental difference in abnormal returns between dividend- and non-dividend-paying firms with no institutional ownership during the RRA93-event period compared to the control periods. We test our second hypothesis with the estimated coefficient for DIV × INST × EVENT. This coefficient represents the effect of institutional ownership on the incremental difference in abnormal returns for dividend- vs. non-dividend-paying firms during the RRA93-event period compared to the control periods. If the level of institutional ownership represents the likelihood that the marginal investor will *not* be subject to the tax rate increase, then the estimated coefficient on this three-way interaction should be positive.

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[7] In sensitivity analysis, we include all sample firms in our analysis and define DIV as an indicator variable equal to 1 if firm *i* pays dividends during the fiscal year ending prior to January 1993, and 0 for non-dividend-paying firms. Although the results of this analysis are less pronounced than those presented later in Table 2, Column (3), conclusions are the same.

This research design controls for DIV and DIV × INST during the nonevent period (when expected tax rates are constant), so the coefficients on DIV × EVENT and DIV × INST × EVENT reflect the incremental effects of the RRA93 tax rate increase. This reduces the likelihood that our tests are confounded by an omitted variable that is correlated with dividend policy or institutional ownership (e.g., risk). Although we cannot completely eliminate the possibility that a nontax factor confounds our results, we know of no plausible *nontax* explanation for the predicted pattern of coefficients.

Our design includes several control variables. The regression includes EVENT to capture any difference in abnormal returns during the event period that is unrelated to dividend policy. We include INST as a separate variable to capture any differential abnormal returns associated with the level of institutional ownership. Following Lang and Shackelford (2000), we include additional control variables to verify that differences in firm profitability, leverage, risk, size, or industry membership do not influence our results. We represent firm profitability as the firm's return on value (ROV), defined as the firm's net income before extraordinary items for the fiscal year ended prior to January 1993, deflated by firm market value at December 31, 1992. We represent firm leverage (LEV) as the firm's total liabilities for the fiscal year ended prior to January 1993, deflated by firm market value at December 31, 1992. We use firm book-to-market ratios (BK/MKT) to control for risk and expected growth differences across sample firms, and define BK/MKT as the firm's book value of equity for the fiscal year ended prior to January 1993, deflated by firm market value at December 31, 1992. We use the log of firm market value at December 31, 1992 (SIZE), to control for size differences across sample firms. Finally, we control for industry membership using indicator variables for firm one-digit SIC code industry membership.

III. RESULTS

We estimate Equation (1) using a sample of firms with financial and stock price information available on Compustat and CRSP for the estimation period. Of the 1,755 firms in our initial sample, we eliminate 109 firms that report both losses and negative retained earnings for the fiscal year ending prior to January 1993. We impose this restriction because shareholder distributions not paid out of current or accumulated earnings and profits are not subject to the dividend tax rate. We eliminate an additional 334 firms for which institutional ownership data are not available on the CDA Spectrum database. The final sample of 1,312 firms is distributed across nine (one-digit SIC code) industries, with firms in the rubber, metal, and machines industry representing the largest portion of the sample, approximately 27.0 percent. Firms in the agriculture, forestry, and fishing industry represent the smallest portion of the sample, approximately 0.2 percent.

Table 1 presents descriptive statistics for sample firms in the aggregate, as well as partitioned by dividend policy using the dichotomous definition of DIV. Approximately 71 percent of sample firms paid some amount of dividends to common shareholders during the fiscal year ending prior to January 1993.[8] The 29 percent of firms that paid no dividends comprise the "no-dividend" (DIV = 0) group (n = 385). The top 30 percent of firms ranked on dividend yield comprise the "high-dividend" (DIV = 1) group (n = 393).

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[8] In comparison, 65 percent of the 1,755 firms with available Compustat and CRSP data paid dividends in the fiscal year ending prior to January 1993.

TABLE 1
Sample Descriptive Statistics and Tests for Differences in Means across No-Dividend and High-Dividend Firms[a]

Variables	*Significant Difference*[b]	*Mean*	*Standard Deviation*	*25% Quartile*	*Median*	*75% Quartile*
DIV YIELD						
Overall sample		0.021	0.033	0.000	0.016	0.031
No-Dividend Firms		0.000	0.000	0.000	0.000	0.000
High-Dividend Firms		0.049	0.048	0.033	0.041	0.056
INST						
Overall sample		0.404	0.238	0.203	0.407	0.591
No-Dividend Firms	HD > ND	0.342	0.240	0.134	0.305	0.543
High-Dividend Firms		0.390	0.233	0.184	0.375	0.572
ROV						
Overall sample		0.043	0.095	0.033	0.053	0.074
No-Dividend Firms	HD > ND	0.023	0.150	0.017	0.043	0.070
High-Dividend Firms		0.052	0.064	0.044	0.064	0.079
LEV						
Overall sample		1.738	3.163	0.288	0.727	1.525
No-Dividend Firms	HD ≈ ND	1.684	3.498	0.219	0.571	1.467
High-Dividend Firms		2.115	3.340	0.530	1.066	1.805
BK/MKT						
Overall sample		0.624	0.465	0.332	0.556	0.793
No-Dividend Firms	HD ≈ ND	0.688	0.665	0.276	0.544	0.927
High-Dividend Firms		0.698	0.352	0.520	0.645	0.828
SIZE						
Overall sample		5.988	1.913	4.653	5.957	7.318
No-Dividend Firms	HD > ND	4.855	1.677	3.743	4.736	5.940
High-Dividend Firms		6.294	1.883	5.092	6.271	7.535

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[a] Our overall sample consists of 1,312 firms. The "no-dividend firms" subsample consists of the 385 firms that did not pay dividends in the fiscal year ending prior to January 1993. The "high-dividend firms" subsample consists of the 393 firms ranked in the top 3 deciles of dividend yield for the fiscal year ending prior to January 1993.

[b] Significant differences between the no-dividend and high-dividend subsamples are based on t-tests of means ($p < 0.05$).

DIV YIELD = common stock dividends for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992;
INST = the percentage of common stock at December 31, 1992, held by institutional investors;
ROV = net income before extraordinary items for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992;
LEV = total liabilities for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992;
BK/MKT = book value for the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992; and
SIZE = log of the firm's market value at December 31, 1992.

Univariate comparisons in Table 1 indicate that high-dividend firms have a higher percentage of institutional ownership than firms that do not pay dividends. Although there could be other explanations for differences in institutional ownership levels, this finding is consistent with a clientele effect for dividend-paying firms (Allen et al. 2000). The existence of tax-induced clienteles, however, biases our tests against finding support for our predictions because high-tax-rate individuals are not the natural clientele for high-dividend-yielding stocks (Dhaliwal et al. 1999; Seida 2001). Table 1 also indicates that high-dividend firms are more profitable (ROV) and larger (SIZE) than no-dividend firms. These differences highlight the importance of controlling for these firm characteristics in our regression analyses.

Table 2 presents our regression results using both the dichotomous (Column [3]) and continuous (Column [4]) specifications of DIV.[9] Although the F-statistics for both regressions are statistically significant ($p = 0.00$ in Column [3] and $p = 0.00$ in Column [4]), we caution that the overall explanatory power of the estimated regressions is quite modest (adjusted R^2 of 0.015 in Column [3] and 0.007 in Column [4]). Because the results are consistent across both specifications, we discuss only the results using the dichotomous DIV measure. To aid in the interpretation of key effects, Figure 1 plots fitted values of the dependent variable, CAR, during the control (Panel A) and RRA93-event (Panel B) periods, as a function of dividend policy and institutional ownership. Consistent with DLT, we find that abnormal returns of firms with no institutional ownership are, on average, positively related to dividend yield during the control period (i.e., the coefficient on DIV is significantly positive), and institutional ownership mitigates this positive effect (i.e., the coefficient on DIV × INST is significantly negative). Panel A of Figure 1 illustrates that during the control period with constant tax rates on dividend income, high-dividend firms with low institutional ownership earn higher abnormal returns than no-dividend firms with low institutional ownership, on average, but this difference decreases as the level of institutional ownership rises. We determine the range (in INST) over which this difference in CAR across high-dividend vs. no-dividend firms remains statistically significant. Specifically, we test whether the sum of the coefficients for DIV and DIV × INST is significantly greater than zero at varying levels of INST. The predicted CAR for a high-dividend firm is significantly higher than the predicted CAR for a no-dividend firm ($p < 0.05$, one-tailed test) for any value of institutional ownership less than 60 percent. These results support the view that when expected tax rates are constant, positive abnormal returns compensate individual investors for the dividend tax penalty.

Our main results concern the incremental effects of DIV and DIV × INST during the RRA93-event period vs. the control period, as indicated by the interaction of these variables with EVENT. Consistent with our expectations, the coefficient for DIV × EVENT is negative and statistically significant. Also as expected, the coefficient for DIV × INST × EVENT is positive and statistically significant. These results indicate that the share price reaction to the dividend tax rate increase implemented by RRA93 is negatively related to dividend yield, and that this negative effect is mitigated by institutional ownership (i.e., the likelihood that the marginal investor was not subject to the increased individual tax rate). In particular, for any value of institutional ownership less than 56 percent, high-dividend firms experienced significantly lower abnormal returns than no-dividend firms during the event period (*relative to the control period*)—i.e., the sum of the coefficients for

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[9] Statistical tests are based on Froot (1989) adjusted standard errors to control for cross-sectional dependence arising for sample observations sharing the same event date.

TABLE 2
Pooled, Cross-Sectional OLS Regressions of Cumulative Abnormal Returns on Independent and Control Variables Using Dichotomous and Continuous Measures of Dividend Yield[a]

Variables	*Prediction*	*Dichotomous DIV*[b]	*Continuous DIV*[c]
		Parameter Estimates (t-statistics)[d]	
(1)	*(2)*	*(3)*	*(4)*
Intercept	?	−0.0156 (−4.77)	−0.0118 (−4.97)
DIV	+	0.0133 (6.58)	0.1379 (4.70)
DIV × INST	−	−0.0184 (−4.47)	−0.3292 (−4.57)
DIV × EVENT	−	−0.0216 (−5.48)	−0.2540 (−4.61)
DIV × INST × EVENT	+	0.0294 (4.14)	0.5040 (3.70)
EVENT	?	0.0075 (3.09)	0.0022 (1.75)
INST	?	0.0093 (2.47)	0.0054 (2.21)
ROV	?	−0.0102 (−1.91)	−0.0054 (−0.78)
LEV	?	0.0001 (0.01)	0.0001 (0.14)
BK/MKT	?	0.0017 (1.54)	0.0030 (3.01)
SIZE	?	0.0002 (0.65)	0.0004 (1.83)
Adjusted R^2		0.015	0.007
Sample Size		6,706	11,177

[a] Cumulative daily abnormal returns (CAR) are the sum of each sample firm's daily market model abnormal returns for each five-day event or control period. We estimate the market model parameters for each sample firm by regressing firm daily returns for calendar year 1992 on the CRSP value-weighted market index. EVENT is equal to 1 for the week of 8/3/93–8/9/93, and 0 otherwise (i.e., 0 for the four control weeks preceding and following the event week). Table 1 defines all other variables.

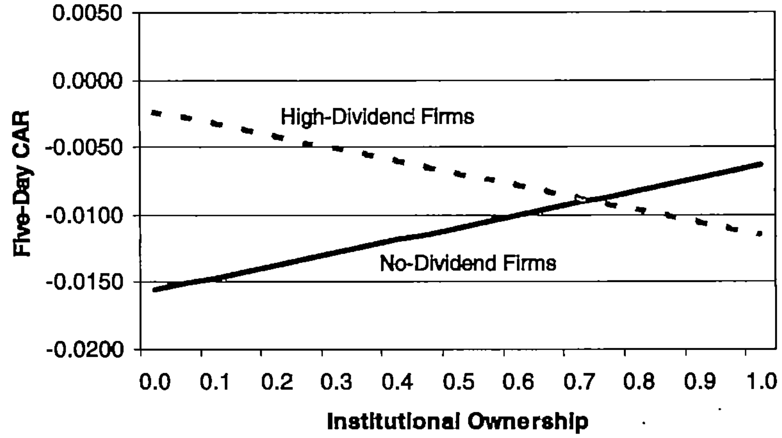
[b] In the dichotomous specification, DIV is equal to 1 for firms ranked in the top three deciles of dividend yield for the fiscal year ending prior to January 1993, and 0 for non-dividend-paying firms. We exclude dividend-paying firms not ranked in the top three deciles of dividend yield from this specification.

[c] In the continuous specification, we include all sample firms in the regression and define DIV as the dividends the firm paid to common shareholders during the fiscal year ending prior to January 1993, deflated by firm market value at December 31, 1992.

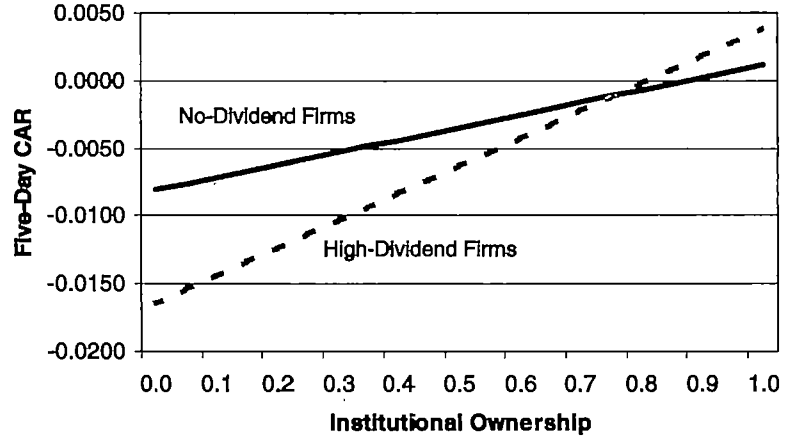
[d] Reported results are after removing the effects of influential observations identified by Belsley et al. (1980) procedures (i.e., absolute R-student value exceeding 2). We calculate t-statistics using the Froot (1989) adjustment that controls for cross-sectional dependence across sample observations. We estimate both regressions with indicator variables (coefficient estimates suppressed) to control for (one-digit SIC code) industry effects.

FIGURE 1
Interactive Effect of Dividend Policy and Institutional Ownership on Abnormal Returns during the Control and RRA93-Event Periods[a]

Panel A: During the Control Periods[b]



Panel B: During the Event Period (August 3–9, 1993)[c]



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[a] The graphs show predicted values of CAR based on the regression estimates reported in Table 2 using the dichotomous DIV variable (Column [3]). As defined in Table 2, "No-Dividend Firms" (DIV = 0) are the 385 sample firms that did not pay dividends, and "High-Dividend Firms" (DIV = 1) are the 393 sample firms in the top 3 deciles of dividend yield. The eight control periods consist of the four five-trading-day periods before and after the RRA93-event period, August 3–9, 1993.

[b] In Panel A, the high-dividend firm values for CAR are the sum of the estimated coefficients for the intercept, DIV, DIV × INST, and INST. The no-dividend firm values for CAR are the sum of the estimated coefficients for the intercept and INST.

[c] In Panel B, the high-dividend firm values for CAR are the sum of the estimated coefficients for the intercept, EVENT, DIV, DIV × EVENT, DIV × INST, DIV × INST × EVENT, and INST. The no-dividend firm values for CAR are the sum of the estimated coefficients for the intercept, EVENT, and INST.

DIV × EVENT and DIV × INST × EVENT for any value of INST less than 56 percent is significantly negative ($p < 0.05$, one-tailed test).

Panel B of Figure 1 illustrates the interactive effect of dividend yield and institutional ownership during the RRA93-event period when the tax rate on dividend income for individual taxpayers increased. Consistent with the three-way interaction, a comparison of Panels A and B indicates that the interactive effect of dividend policy and institutional ownership differs across the control and RRA93-event periods. Because share prices of dividend-paying firms are adjusting to higher shareholder-level tax rates during the RRA93-event period, high-dividend firms with low institutional ownership earned lower abnormal returns during this period, on average, than did no-dividend firms with low institutional ownership.[10] Consistent with this difference in abnormal returns being attributable to the tax penalty on individual investors' dividend income, the difference in abnormal returns decreases as the level of institutional ownership rises (just as during the control period).

To assess the reasonableness of our regression estimates, we first derive a theoretical prediction of the abnormal returns that should occur during the event period, and then compare this predicted effect to the magnitude of the effect implied by our regression results. For simplicity, we illustrate this analysis for a stock with zero institutional ownership. We assume that a share's value equals the present value of an annuity of its annual dividend amount received in perpetuity, plus some liquidation value. Because RRA93 should affect only the portion of share value attributable to expected dividends, we do not attempt to explain how liquidation value is determined. Consequently, we compute the predicted share price effect of RRA93, ΔPRICE, as follows:

$$\Delta\text{PRICE} = \left(\frac{\text{AnnualDividendAmount}}{\text{DiscountRate}}\right) \times \left(\frac{td_{\text{before}} - td_{\text{after}}}{1 - tg}\right) \times \Delta\text{PROB}. \quad (2)$$

The first factor on the right-hand side is simply the present value of the dividend stream, which varies with the assumed discount rate. We use the average annual dividend payment of high-dividend firms ($1.43) for the numerator of this term, and assume a discount rate of 10 percent. The second factor recognizes that the shareholder tax discount on dividend income is a function of both the dividend tax rate (td) and the capital gains tax rate (tg) (Poterba and Summers 1985, 232). The numerator of this factor is the increase in the dividend tax rate. Using $td_{\text{before}} = 31.0$ percent, $td_{\text{after}} = 39.6$ percent, and $tg = 28.0$ percent (i.e., the maximum long-term capital gains tax rate in 1993), this factor implies that the portion of share value attributable to expected dividends should decline in value by 11.94 percent as a result of RRA93. The third factor on the right-hand side recognizes that, in expectation, some part of the RRA93 tax rate increase had already been impounded into share values prior to the event period. Because RRA93 passed by such a narrow margin, we assume a prior probability of 0.5 such that the change in probability during the event period (i.e., ΔPROB) is also 0.5.

Under these assumptions, the predicted mean price decrease (ΔPRICE) for high-dividend firms is $0.85 per share [($1.43/0.10) × 11.94% × 0.5], and the predicted event-period abnormal return for these firms is −2.77 percent (−$0.85/$30.85 average share price). In comparison, the estimated coefficient on DIV × EVENT in the dichotomous specification is −2.16 percent. Given these basic assumptions, this analysis suggests that the estimated coefficient is reasonable.[11]

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[10] Event period CARs for high-dividend firms are significantly lower than those for no-dividend firms at all levels of institutional ownership below 34 percent ($p < 0.05$, one-tailed test). We determine this range of significance by testing whether the sum of the coefficients for DIV, DIV × INST, DIV × EVENT, and DIV × INST × EVENT is significantly less than zero at varying levels of INST.

With respect to our control variables, the coefficient for INST is significantly positive. Unlike the interaction terms involving dividend policy, we do not interpret this direct effect of INST as tax related. Possible nontax explanations of the positive effect of INST on abnormal returns are that institutional investors are better able to pick stocks that outperform the market due to informational advantages (Ali et al. 2002), or that management oversight by institutional investors enables firms to outperform the market. The significantly positive coefficient on EVENT indicates that, on average, during the RRA93-event period no-dividend firms realized abnormal returns that were more positive than those earned during the control period. The positive effect of EVENT could reflect other anticipated economic consequences of RRA93, or other economic events that occurred during the same week. The estimated coefficients for book-to-market ratio (BK/MKT) (in Column [4]) and the transportation and utilities industry (SIC 4000) indicator (not reported in Table 2) are both significantly positive (p = 0.00), suggesting that it is important to control for these firm characteristics when analyzing stock returns over this event period.[12]

IV. CONCLUSIONS

This study investigates the effect on share prices of an increase in the individual income tax rate for dividend income. Specifically, we examine abnormal returns on common stocks during the five-day period in 1993 when Congress enacted the Revenue Reconciliation Act of 1993, which increased the top tax rate for individuals from 31.0 percent to 39.6 percent. We predict that the tax-related effect on share prices is a joint function of firm dividend policy and the tax status of the firm's marginal investor. Consistent with our predictions, we find that the higher the firm's dividend yield, the more negative the firm's abnormal returns during the RRA93-event period. Moreover, because the 1993 tax rate increase affected only individuals, we predict and find that the level of institutional holdings, which represents the likelihood that the marginal investor in a particular stock is not an individual taxpayer, mitigates the negative effect of the firm's dividend yield on RRA93-event-period abnormal returns. Importantly, we find that the tax-related share price effects of dividend yield and institutional ownership during the RRA93-event period (in which expected future tax rates are increasing) are exactly the opposite of these effects during the control period (in which tax rates are constant).

Our study makes two contributions to the literature investigating the extent to which share values impound shareholder-level dividend taxes. First, we find that the effect of shareholder dividend taxes on the firm's share value depends, in part, on the tax status of the firm's marginal investor. Although both the tax-capitalization and traditional perspectives recognize that the relevant shareholder tax rate is that of the marginal investor, there is scant empirical evidence that the marginal investor's tax status varies across stocks. We argue that institutional ownership is a reasonable proxy for the likelihood that the marginal investor in a particular firm is *not* an individual taxpayer. Our conclusions conflict with those of Harris and Kemsley (1999) and the assumption of Collins and Kemsley (2000, 415) that the marginal investor for all stocks is a top-tax-rate individual taxpayer. In turn, our results reinforce the need to control for differences in the tax status of marginal investors across firms in future research investigating shareholder-level tax effects on share values.

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[11] The −2.16 percent value for the DIV × EVENT coefficient implies that this coefficient captures approximately 78 percent of the predicted effect. Using a similar approach, the coefficient estimate based on the continuous DIV measure (Table 2, Column [4]) explains approximately 43 percent of the predicted effect.

[12] Collinearity diagnostics verify that the correlation between firms' dividend policy and institutional ownership does not significantly affect our results. The highest condition indices for each analysis range from 18 to 20, well below the upper bound of 30 that Belsley et al. (1980) suggest as problematic. In sensitivity analyses we included an INST × EVENT interaction term in each of the Table 2 regression analyses. Although including INST × EVENT induces high levels of multicollinearity, conclusions are unaffected.

Second, we find that firm dividend policy matters. The negative effect of dividend yield on abnormal returns during the event period in which RRA93 increased dividend tax rates is consistent with the traditional view that dividend policy determines the extent to which dividend taxes are reflected in share prices. Likewise, the positive effect of dividend yield during the control period when expectations of future tax rates were constant is also consistent with the traditional view that individual shareholders of dividend-paying firms require higher returns to compensate for the dividend tax penalty. Both results are inconsistent with a strict interpretation of the tax-capitalization view that dividend policy is irrelevant (e.g., Harris and Kemsley 1999, 281).

Finally, our results are also important from a tax-policy perspective because they demonstrate that the effect of changes in shareholder-level taxes on stock prices depends on both the firm's dividend policy and the tax status of its shareholders. Our evidence enables policymakers to better anticipate the consequences (whether intentional or not) of tax rate changes for firm market values.

Readers should interpret the results of this study in light of three limitations. First, the fluidity of dividend tax rates over time makes it difficult to know whether investors view a tax rate change as temporary or permanent, and this has implications for distinguishing between the tax-capitalization and traditional perspectives. In particular, both the traditional and tax-capitalization views predict that dividend policy affects the extent to which share values impound *temporary* tax rate changes (Poterba and Summers 1985, 261). For example, although the tax-capitalization view assumes that all distributions are subject to dividend taxes, only those distributions occurring during the effective period of a temporary tax increase, which is a function of dividend policy, would be discounted at the temporarily higher tax rate. Thus, if investors viewed the RRA93 tax rate increase as temporary rather than permanent, then the significant negative effects of dividend policy we document during the event period would be consistent with both theories. Nonetheless, our research design also captures the significantly positive effect of dividend policy on returns during control periods on both sides of the event period, and this result is entirely inconsistent with the tax-capitalization perspective. On balance, therefore, we interpret our evidence as supporting the traditional view that the extent to which share values impound dividend taxes depends on the firm's dividend policy.

Second, because the tax legislative process spans a time frame longer than our event window, our design likely does not capture the complete price effect of the tax rate increase. Thus, our estimates of the price effects associated with the tax rate increase likely underestimate the true effects. Finally, although the magnitude of estimated coefficients appear in line with theoretical expectations, the model's overall explanatory power is quite modest.

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The Relation between Cost Shifting and Segment Profitability in the Defense-Contracting Industry

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ABSTRACT: We test the conjecture from prior research that defense contractors' excess profitability in the 1980s stemmed from their ability to shift common overhead costs to government contracts that typically allow cost reimbursement or price renegotiation (Rogerson 1992; Thomas and Tung 1992; Lichtenberg 1992). Although we confirm prior evidence that defense contractors enjoyed abnormally high profitability on their government work in the 1984–1989 period (a period of relatively low competition for defense contracts), we find no evidence that this excess profitability is attributable to cost shifting. In addition, we find no evidence that the Top 100 defense contractors (firms that likely wield above-average market power) are able to use cost shifting to exploit a lack of competition in the industry. Our results suggest that, contrary to the conjectures in prior research, the unusually high profitability reported on government contracts in 1984–1989 is more likely attributable to nonaccounting explanations than to cost shifting.

Keywords: *cost shifting; defense contracting; competition.*

Data Availability: *All data are available from public sources.*

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I. INTRODUCTION

In the mid-1980s, the General Accounting Office confirmed that defense contractors earned a higher return on assets (ROA) on their defense work than on their commercial business (Carrington 1986; Trueger 1991). Lichtenberg (1992) finds that the ROA of industry segments with government contracts is 68–82 percent higher than the ROA of commercial segments. Prior researchers argue that cost shifting is at least partly responsible for the defense-contracting industry's excess profits (Rogerson 1992; Thomas and Tung 1992). We test the hypothesized relation between cost shifting and defense-contractor excess profits by comparing the relative profitabilities (ROAs) of three segment types in defense-contracting firms: commercial segments (those with only commercial revenues), government segments (those with revenues derived predominantly from government contracts), and mixed segments (those with revenues from both commercial and government business). We also evaluate whether market power in the defense industry, and the level of competition for defense contracts, affect the relation between cost shifting and relative segment profitability.

The cost-shifting hypothesis assumes that revenues from defense contracts are more cost-sensitive than commercial revenues. Demski and Magee (1992, 736) describe cost sensitivity as "the responsiveness of revenue received to product costs reported." A classic example of a purely cost-sensitive contract is a cost-plus-fixed-fee contract, where the government reimburses the contractor for all incurred costs and pays the contractor a negotiated fixed profit (fee). For a purely cost-sensitive contract, an increase in cost creates a corresponding increase in revenue (price), rather than a decrease in profit.

Within a mixed segment, firms can choose legitimate cost-accounting methods that allocate overhead costs away from commercial work and toward defense contracts. This increases profits on the segment's commercial work without reducing profits on its government contracts. The ability to "pass-on" price increases to the government payer implies that cost shifting is more likely to occur when the level of competition for defense contracts is low. If defense contractors exploit a lack of competition by shifting costs, then we hypothesize that a firm's mixed segments should be more profitable than either its commercial or predominantly government segments, because managers have more opportunities to shift costs *within* these mixed segments than within "pure" segments or *across* segments.

Competition for defense contracts does not take place in a purely competitive market, because one buyer relies on a relatively small number of firms for a large proportion of its purchases. Firms ranked among the Top 100 defense contractors likely wield substantial market power in the Department of Defense (DOD). We investigate whether these ranked contractors exploit their market power to take advantage of low levels of competition. If mixed segments offer the greatest opportunities to shift costs when competition is low, then we expect ranked contractors' mixed segments to be more profitable than unranked contractors' mixed segments.

Decreased demand for defense products increases the level of competition for defense contracts and restricts defense contractors' ability to pass increased costs to the government. *Ceteris paribus*, any relative profitability advantage of defense contractors' mixed segments should then dissipate. Decreased demand for defense contracts could disproportionately affect ranked and unranked contractors' ability to shift costs. If ranked contractors' market power does not depend on the level of industry competition, then ranked contractors may continue to extract excess profits from cost shifting despite increased competition. Alternatively, increased competition could reduce the ranked contractors' market power and curtail their ability to extract additional profits through cost shifting.

We use 1984–1989 and 1994–1998 Compustat segment data to test the relation between cost shifting and excess profits by comparing the relative profitability (ROA) among three types of segments within defense-contracting firms: (1) commercial segments, (2) government segments, and (3) mixed segments. We evaluate whether the relation between excess profits and cost shifting is more pronounced for ranked contractors who likely wield above-average market power. We contrast the results from 1984–1989, a period of low competition for defense contracts, with those from 1994–1998, a period of reduced defense procurement outlays and increased competition for defense contracts.

Our findings indicate that managers' opportunities to manipulate cost allocations within mixed segments do not necessarily result in excess reported profits. Government segments (not mixed segments) significantly outperform their counterparts in the 1984–1989 period. In 1994–1998, profitability does not differ significantly across government, commercial, or mixed segments. We find no difference in the relative profitability of ranked and unranked contractors' mixed segments in either time period.

Our study makes two contributions to the literature. First, our results do not support prior research's conjecture that excess profits in the defense-contracting industry are attributable to within-segment cost shifting. Second, we provide evidence that low competition for defense contracts is associated with excess profits, even when there are limited opportunities for cost shifting. Thus, our results suggest that the excess profitability is consistent with nonaccounting explanations, including Leitzel's (1992) argument that the DOD is a "captured regulator" that allows its contractors to extract significant economic rents from government contracts.

The following section develops our hypotheses. Section III describes the sample, variables, and research design used to test the hypotheses, and Section IV reports our results. Section V concludes the paper.

II. HYPOTHESIS DEVELOPMENT

Cost Shifting in Low-Competition Environments for Defense Contracts

Prior studies examine defense-contractor cost shifting (Rogerson 1992; Thomas and Tung 1992) and excess profitability on government contracts (Lichtenberg 1992) in the 1980s, a relatively low-competition environment for defense contracts. Defense contractors are better able to derive excess profits from cost shifting when competition for contracts is low, because low competition allows defense contractors to pass excess costs to the government more easily.

Rogerson (1992) argues that a mix of cost-sensitive government contracts and cost-insensitive commercial work motivates defense contractors to shift overhead costs away from the firm's commercial work and toward its government contracts, thus increasing the profits on the firm's commercial work.[1] Rogerson (1992) defines cost sensitivity in terms of competition and funding levels. He argues that a well-funded, sole-sourced contract is more cost sensitive than either a poorly funded, competitively bid contract or commercial work.

Using 1980–1983 pension data, Thomas and Tung (1992) provide evidence consistent with cost shifting by evaluating incentives for shifting pension costs from a firm's nondefense to its defense business. They conclude that contractors' incentives to overfund pension costs exist only if a firm derives a portion of its revenues from cost-reimbursement contracts, but they do not provide evidence that cost shifting results in abnormal profits.

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[1] Prior research has documented cost shifting in the health care, government, and nonprofit sectors. For example, Eldenburg and Soderstrom (1996) find that hospitals shifted costs among various payers, which increased revenues. Eldenburg and Kallapur (1997) find that hospitals responded to Medicare's reduction in inpatient reimbursement rates by adjusting the patient mix to favor Medicare outpatients. Cavalluzzo et al. (1998) find that after the Federal Reserve began to compete directly with large commercial banks for some services, managers shifted overhead costs away from competitive services (e.g., check clearing) and toward noncompetitive services (e.g., managing the money supply). Yetman (2001) finds that, in response to tax incentives, medical and educational nonprofits shifted costs away from tax-exempt activities and toward taxable activities.

Lichtenberg (1992) examines the 1984–1989 profitability of 9,300 industry segments from government and nongovernment contractors. He finds that segments with the greatest percentage of revenues derived from government contracts also enjoy the highest ROA. Although Lichtenberg's (1992) findings suggest that government contracting results in excess profits, he does not investigate whether the excess profits result from cost shifting.

We primarily focus on ascertaining whether cost shifting can explain excess profits from defense contracts when competition for contracts is low. We compare the profitability of a firm's mixed segments to that of its commercial and government segments in a low-competition environment (1984–1989) to test our cost-shifting hypothesis. However, in addition to cost shifting there are other *nonaccounting* explanations for excess defense-contractor profits. For example, the DOD may wish to subsidize its contractors (Lichtenberg 1992). Similarly, Leitzel (1992) argues that the DOD is a "captured regulator" acting in the defense industry's best interests. Such economic explanations suggest that government segments would be more profitable than either commercial or mixed segments. Distinguishing among alternative *nonaccounting* explanations for government contracts' excess profitability requires examining economic or industrial organization issues that are beyond the scope of this study.

We base our analysis of the relation between cost shifting and excess profits on two assumptions. First, consistent with Rogerson (1992), we assume that government work is more cost sensitive than commercial work, providing defense contractors with the opportunity to shift costs when competition for defense contracts is low.[2] Second, we assume that managers can more easily shift costs within a segment than across segments. Work done within a segment is more likely to be similar in nature and to share both a common overhead allocation method and a common physical location, increasing the pool of allocable, shared costs.[3]

Segments with a mix of commercial work and government contracts (mixed segments) provide managers with the greatest opportunity to shift costs. If defense contractors use cost shifting to exploit a lack of competition in order to earn excess profits, then mixed segments should be more profitable than either commercial or government segments.

> **H1a:** A defense contractor's mixed segments will be more profitable than its commercial segments or its government segments when competition for contracts is low (1984–1989).

Cost shifting implies that the contractor passes additional overhead costs shifted from the firm's commercial work onto its government payer. Passing additional costs onto the

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[2] This also presumes that government officials' vigilance is insufficient to prevent overpricing.

[3] We tested our assumption that, when competition is low, firms can more easily shift costs within a segment than across segments. If government contractors shift costs from purely commercial segments to segments with sales to the government, then government contractors' commercial segments should be more profitable than similar segments of commercial firms. We matched the Compustat segment SIC code, year, and segment assets of a 1984–1989 sample of 420 commercial segments in defense-contracting firms to segments in purely commercial firms (firms with no revenues from government contracts). We identified only 140 matches for the defense contractors' commercial segments. The defense-contracting firms' commercial segments have an average ROA of 13.0 percent. The average ROA of segments from purely commercial firms is 10.8 percent. A matched-pair t-test ($t = 1.17$, $p = 0.12$, one-tailed) between the ROA of the two groups (defense contractors' commercial segments compared with matched segments from purely commercial firms) does not suggest that defense contractors engage in large-scale across-segment cost shifting.

DOD requires negotiation.[4] In the market for defense goods, a relatively small number of suppliers sell to a single customer. Some of these suppliers are more important to the DOD than others. Karpoff et al. (1999) argue that firms ranked among the Top 100 defense contractors exercise influence (or market power) within the DOD. Ranked contractors' market power would allow them to pass costs to the DOD more easily and to increase the profitability of their mixed segments. Ranked contractors should be in a stronger position in negotiating final overhead billing rates on cost-reimbursement contracts and in negotiating changes on fixed-price work, especially when overall competition is low. If defense contractors use cost shifting in their mixed segments to exploit a lack of competition, then ranked defense contractors' market power gives them an advantage over unranked contractors in extracting excess profits through cost shifting.

> **H1b:** Ranked defense contractors' mixed segments will be more profitable than unranked contractors' mixed segments when competition for contracts is low (1984–1989).

Effect of the Level of Competition on Cost Shifting

Increased competition for defense contracts decreases the contractor's ability to "pass-on" price increases to a government payer, which in turn reduces the benefits of cost shifting.[5] Two factors increased the competitiveness of the defense-contracting market in the 1990s. First, with the passage of the Federal Acquisition Streamlining Act in 1994 and the Clinger-Cohen Act of 1996, defense procurement took a major step away from cost-based negotiated contracts and toward price-based competition. In 1984, the DOD awarded 51.2 percent of its new prime contracts through full and open competition. By 1998, this percentage was 65.5, an increase of 14.3 percentage points.[6]

Second, defense spending fell significantly during the 1990s. Measured in 1992 constant dollars, federal defense procurement outlays (the amount paid to defense contractors) in 1998 were approximately 40 percent below the 1987 peak of $94.4 billion. Between 1984 and 1989, defense procurement spending averaged $88.1 billion, in 1992 constant dollars, representing 27.5 percent of total defense spending and 1.66 percent of the GDP. In contrast, between 1994 and 1998, defense procurement spending fell to an average of $47.2 billion in 1992 constant dollars, representing 19.0 percent of total defense spending and only 0.69 percent of GDP (U.S. Bureau of the Census 1998).

The precipitous drop in defense procurement spending, coupled with a move toward price-based contract solicitations, suggests that the defense-contracting environment was more competitive in the 1990s than in the 1980s. As competition increases, incentives to shift costs decrease as renegotiating fixed-priced contracts and bidding for new work with higher overhead rates becomes more difficult. If defense contractors used cost shifting to

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[4] Prior to the beginning of the fiscal year, defense contractors develop provisional indirect rates. The DOD approves the rates based upon actual costs incurred during the prior year and a formal budget submission. The contractor uses these preapproved provisional rates to accrue revenue, generate interim invoices to the government, and bid on new work. At the end of the fiscal year, the DOD conducts an incurred cost audit, in which auditors review all costs (on all types of contracts). At the conclusion of the audit, contractors meet with the DOD auditors to negotiate their final indirect rates. Contractors use the approved rates to adjust their billings on cost-reimbursement contracts. They could also negotiate contract change orders to obtain more funding on individual contracts or renegotiate fixed-price contracts.

[5] Dranove (1988) makes a similar argument in a health care context. He finds that as private-sector insurance became more competitive, hospitals were less apt to shift costs from Medicaid patients to private payers.

[6] We compiled these statistics using information published in the DOD report *Prime Contract Awards*, prepared annually by the Washington Headquarters Services Directorate for Information Operations and Reports.

exploit a relative lack of competition in 1984–1989, their mixed segments should lose any profit advantage in the more competitive 1994–1998 period, and these mixed segments should experience the greatest drop in profits in 1994–1998 relative to 1984–1989.

> **H2a:** Within firms, mixed segments will experience a greater reduction in profitability from 1984–1989 to 1994–1998 than either commercial segments or government segments.

Ceteris paribus, increased competition reduces ranked contractors' ability to pass increased costs on to the DOD. As demand for ranked defense contractors' products fell, their ability to negotiate higher prices would also decrease. If ranked contractors lost their market power advantage over unranked contractors, then the profit differential between ranked and unranked contractors' mixed segments would dissipate.

Alternatively, increased competition for defense contracts may affect ranked and unranked contractors differently. For example, Karpoff et al. (1999) demonstrate that ranked contractors procure subsequent government contracts, despite allegations of defense procurement fraud, whereas unranked firms do not. This ability to obtain additional work persisted into the more competitive 1990s time period.[7] Similarly, ranked contractors may retain or even increase their market power advantage over unranked firms despite increased levels of competition. Since the effect of increased competition on ranked contractors' ability to exploit market power through cost shifting in their mixed segments is not clear, we state the following hypothesis in the null form.

> **H2b:** There is no difference in the reduction in profitability from 1984–1989 to 1994–1998 between ranked and unranked contractors' mixed segments.

III. METHOD

Sample

To compile a sample of defense contractors, we searched the 1984–1989 NAARS database using the following keywords: *fixed-price*, *cost*, *long-term*, *military*, *defense*, *army*, *navy*, or *air force* in conjunction with the word *contract*. We reviewed portions of each firm's financial statements to eliminate any firms not engaged in defense contracting. For example, a firm might have long-term contracts, but the keyword *defense* flagged in the NAARS search referred to a legal defense rather than to defense contracting. We restricted the sample to 104 firms that appeared in the NAARS search in each of the six years, from 1984 to 1989. This increased the probability that government contracts were a normal part of a firm's operations.

We matched the 104 firms to the 1984–1989 and 1994–1998 Compustat segment files to determine the amount of government sales in each segment. We restricted the sample to defense contractors that had at least one segment with purely commercial revenues during either 1984–1989 or 1994–1998. This allows us to compare each firm's commercial segments' profitability with that of its segments with sales to the government. In any given year, we eliminated firms reporting only a single segment, firms reporting government sales in all segments, and firms reporting no segment information.[8]

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[7] The time period of the Karpoff et al. (1999) study is 1983–1995.

[8] We also eliminate firms that apparently report erroneous segment information. For example, several firms disclosed the same amount of sales to the government in each of their segments. The amount greatly exceeded total segment sales and often corresponded to the firm's total amount of sales to the government disclosed in the major customer footnote of a firm's financial statements.

The number of sample firms averages 35 in 1984–1989 and 27 in 1994–1998. The 23 percent decrease in the average number of firms reflects the mergers among defense contractors, and contractors' decisions to divest their defense-contracting segments. The firms report a total of 1,166 segments. We eliminate 9 segments with no primary SIC code, 17 reporting no segment assets, and 17 with segment income divided by segment assets greater than 1 or less than −1. The final sample totals 1,123 segments, with 701 in 1984–1989 and 422 in 1994–1998.

Variable Definitions and Research Design

We used the 1984–1989 and 1994–1998 Compustat segment files to obtain *segment total sales, segment income, segment assets, segment sales to the government,* and each segment's primary SIC code. We use ROA, defined as *segment income* divided by *segment assets,* to measure relative profitability.

We use the percentage of a segment's revenues derived from government contracts (*segment sales to the government* divided by *segment total sales*) to estimate the relative cost sensitivity of a segment's revenue stream. Although the sample firms all participate in defense contracting, *segment sales to the government* includes revenues derived from nondefense (civil) as well as from defense contracts. Procurement statistics support our assumption that segment sales to the government stem primarily from sales to the DOD. The DOD awards the largest percentage of total contract dollars of any government agency. In 1984, it awarded 80 percent of the federal government's contracting dollars. No other agency (except for the Department of Energy [DOE] at 7 percent) awarded more than 5 percent of federal contract dollars. In 1994, the DOD awarded 67 percent of all federal contract dollars. All other agencies accounted for less than 5 percent of the government's contract awards, except for the DOE (9 percent) and NASA (6 percent).

Segment sales to the government also includes revenues derived from fixed-priced contracts as well as from cost-reimbursement contracts, and from contracts awarded using various solicitation procedures. We evaluated data obtained from the U. S. General Services Administration's Federal Procurement Data System to compare the types of contracts and the solicitation procedures the DOD and civil agencies used in 1984 and 1994. Both the DOD and civil agencies award most of their contracts using either cost-reimbursement contracts or through negotiated, noncompetitive solicitations. This suggests that both civil government agencies' and the DOD's contract award methods produce cost-sensitive revenue streams.

We proxy for the ability to shift costs by defining the variable SEGMENT TYPE, based on the percentage of a segment's revenues derived from government contracts. We classify firm segments as commercial (segments with only commercial revenues), government (segments that derive more than 90 percent of revenues from government contracts), or mixed (segments that derive between 0 and 90 percent of revenues from government contracts). We proxy for the level of competition by comparing the results between the 1984–1989 period of low competition and the 1994–1998 period of increased competition.

Following Karpoff et al. (1999) we consider contractors ranked in the annual DOD report, *100 Companies Receiving the Largest Dollar Volume of Prime Contract Awards,* to wield market power. Procurement statistics also support our assumption that contractors that exert market power within the DOD also exercise power in civil federal government agencies. DOD-ranked contractors account for more than 70 percent of the firms included in the Federal Procurement Data System report, *Top 100 Federal Contractors,* in both 1984 and 1994.

We test all of the hypotheses concerning segment profitability with a repeated-measures analysis of variance (ANOVA) model that simultaneously adjusts for systematic differences across firms and years. The repeated-measures design allows for multiple measurements on firms across years, and partitions sources of variation into between-firm variation and within-firm variation. We use between-firm variation to test for effects due to the fixed factors SEGMENT TYPE and RANKED, and the interaction between SEGMENT TYPE and RANKED, while controlling for INDUSTRY. We use within-firm variation to test for the effect of the fixed factor YEAR and interactions among YEAR, SEGMENT TYPE, and RANKED. We assume the observations between years within a firm follow a first-order autocorrelation model.[9] We use annual data to estimate the following model (adapted from Neter et al. 1990, 827), and we use contrasts of the least squares mean ROA to test our hypotheses:

$$ROA_{ijklm} = \mu + S_i + R_j + (SR)_{ij} + I_k + F_{ijkm} + Y_l + (SY)_{il} + (RY)_{jl} + (SRY)_{ijl} + \epsilon_{ijklm} \qquad (1)$$

where:

ROA_{ijklm} = *segment income* divided by *segment assets*, a continuous variable ranging from −1 to 1;
μ = a constant;
S_i = the main effect of the fixed factor SEGMENT TYPE, which takes on one of three values: Commercial, Mixed, or Government;
R_j = the main effect of the fixed factor RANKED, which takes on one of two values: Ranked or Unranked to indicate whether the contractor is ranked among the Top 100 defense contractors;
I_k = the main effect of the fixed factor INDUSTRY, which takes on one of eight values corresponding to the first digit of the segment's Compustat SIC code to control for industry effects;
F_{ijkm} = a random firm effect distributed $N(0, \sigma^2_F)$;
Y_l = the main effect of the fixed factor YEAR, which takes on one of 11 values to distinguish each of the years 1984–1989 and 1994–1998; and
ϵ_{ijklm} = an error term with a structure assumed to be first-order autocorrelation between years within firms.

Because the repeated-measure design allows for multiple measurements on firms across years, we test for a time period effect (the factor of interest) by contrasting the least squares mean ROA from the six years 1984–1989 (the *early* time period) with the least squares mean ROA from the five years 1994–1998 (the *late* time period). Using contrasts to test for the time period effect (rather than pooling the annual data into two time periods prior to estimating the model) preserves degrees of freedom, resulting in more powerful tests.

To test H1a (a defense contractor's mixed segment ROA is higher than its government or commercial segment ROA when competition for contracts is low) and H2a (the decrease in ROA from the early to the late time period is steeper for mixed segments than for other segments), we construct appropriate contrasts of the least squares mean ROA by segment type for each time period. Specifically, for H1a, we hypothesize that $ROA_{\text{Mixed, Early}} > ROA_{\text{Commercial, Early}}$ and that $ROA_{\text{Mixed, Early}} > ROA_{\text{Government, Early}}$. For H2a, we hypothesize

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[9] The repeated-measures ANOVA is equivalent to a weighted least squares regression corrected for first-order autocorrelation between years within firms.

that $(\text{ROA}_{\text{Mixed, Early}} - \text{ROA}_{\text{Mixed, Late}}) > (\text{ROA}_{\text{Commercial, Early}} - \text{ROA}_{\text{Commercial, Late}})$ and that $(\text{ROA}_{\text{Mixed, Early}} - \text{ROA}_{\text{Mixed, Late}}) > (\text{ROA}_{\text{Government, Early}} - \text{ROA}_{\text{Government, Late}})$. Similarly, we test H1b (ranked defense contractors' mixed segment ROA is higher than unranked contractors' mixed segment ROA when competition for contracts is low) and H2b (no difference in the decrease in ROA from the early to the late period between ranked and unranked contractors' mixed segments) by constructing contrasts of the least squares mean ROA by segment type and contractor ranking for each time period. Specifically, for H1b, we hypothesize that $\text{ROA}_{\text{Mixed, Ranked, Early}} > \text{ROA}_{\text{Mixed, Unranked, Early}}$. For H2b, we hypothesize that $(\text{ROA}_{\text{Mixed, Ranked, Early}} - \text{ROA}_{\text{Mixed, Ranked, Late}}) = (\text{ROA}_{\text{Mixed, Unranked, Early}} - \text{ROA}_{\text{Mixed, Unranked, Late}})$.

Descriptive Statistics

Table 1 provides annual statistics for the sample of defense contractors' firm segments for the years 1984–1989, a period of relatively low competition, and the years 1994–1998, a period of increased competition for defense contracts. Segment ROA for the total sample drops from an average of 13.8 percent in 1984–1989 to 12.1 percent in 1994–1998, ranging from a high of 17.6 percent in 1984 to a low of 9.9 percent in 1994. The percentage of average segment revenues derived from government contracts drops from 27.8 percent in the late 1980s to 24.6 percent in the late 1990s. The percent of the segments from firms that rank in the DOD's Top 100 listing each year averages 58.1 percent in 1984–1989 and 59.2 percent in 1994–1998. Ranked contractors' segment ROA averages approximately 14 percent in both time periods, although the percentage of segment revenues derived from government contracts falls from 28.6 to 22.8 percent. In contrast, average unranked contractors' segment ROA decreases from 14.1 percent in 1984–1989 to 8.9 percent in 1994–1998, with the percentage of segment revenues derived from government contracts averaging 27 percent in both time periods. These descriptive statistics suggest that ranked firms maintain their average segment profitability but rely less on government contracts as a revenue source. Unranked firms display a steep drop in average segment profitability but continue to rely on government contracts for almost one-third of average segment revenues.

Table 2 presents average ROA grouped by segment SIC code and segment type for 1984–1989 (Panel A) and 1994–1998 (Panel B), respectively. In each time period, approximately 60 percent of the segments are purely commercial. In 1984–1989, the government segments (with an average of 97.6 percent of revenues derived from government contracts) report an average ROA of 19.4 percent. The mixed segments' ROA averages 14.1 percent and the commercial segments' ROA averages 12.4 percent. These unconditional means suggest that mixed segments, where managers have the opportunity to shift costs, do not report relatively higher profitability when competition for defense contracts is low. In 1994–1998, average ROA for each segment type is approximately 12 percent (government = 12.3 percent, mixed = 12.3 percent, and commercial = 12.0 percent). The government segments' profit rates experience the steepest decline (from 19.4 percent in 1984–1989 to 12.3 percent in 1994–1998). The mixed segments' ROA, expected to experience the largest decline in profitability, drops only 1.8 percent (from 14.1 percent to 12.3 percent), suggesting that within-segment cost shifting was not responsible for excess profits on government contracts in 1984–1989.

Table 2 also summarizes average ranked and unranked contractors' ROA for each segment type in each time period. In 1984–1989, the ranked firms' government segments report an average ROA of 18.1 percent. Ranked firms' mixed and commercial segments report an average ROA of 14.1 percent and 12.3 percent, respectively. The same pattern holds for the unranked contractors, whose government segments report an average ROA of 21.3

TABLE 1
Descriptive Statistics for Segments of Firms with Revenues from Defense Contracts (1984–1989) and (1994–1998)[a]

	Segments Belonging to Firms Ranked as a Top 100 Defense Contractor[b]			*Segments Belonging to Firms Not Ranked as a Top 100 Defense Contractor*[b]			*Total Sample of Segments*		
Year	*n*	*Percentage of Segment Revenues Derived from Government Contracts*[c]	*Mean Segment ROA*[d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*[c]	*Mean Segment ROA*[d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*[c]	*Mean Segment ROA*[d]
1984	62	25.6	17.6	61	31.7	17.7	123	28.6	17.6
1985	61	25.4	15.4	58	30.8	14.5	119	28.0	14.9
1986	69	29.5	11.9	52	25.7	15.6	121	27.8	13.5
1987	79	30.9	14.0	45	22.6	11.7	124	27.9	13.2
1988	69	30.5	12.3	38	23.2	12.7	107	27.9	12.4
1989	67	28.6	10.7	40	22.6	10.4	107	26.4	10.6
1984–89	407	28.6	13.6	294	26.7	14.1	701	27.8	13.8
1994	56	20.0	12.4	44	29.3	6.8	100	24.2	9.9
1995	61	24.3	14.5	41	24.4	8.9	102	24.3	12.3
1996	49	24.2	15.3	39	28.0	12.5	88	25.9	14.0
1997	46	20.4	15.5	31	28.4	6.8	77	23.6	12.0
1998	38	25.2	14.4	17	25.7	9.8	55	25.3	13.0
1994–98	250	22.8	14.4	172	27.3	8.9	422	24.6	12.1

[a] We compiled the sample of defense contractors using a keyword search of the 1984–1989 NAARS database. The keywords included: *fixed-price, cost, long-term, military, defense, army, navy,* or *air force* in conjunction with the word *contract*. We restricted the sample to firms that appeared in the NAARS search in each of the six years 1984–1989 and firms that had at least one segment with purely commercial revenues during either 1984–1989 or 1994–1998. The number of sample firms averages 35 in 1984–1989 and 27 in 1994–1998.

[b] Ranked contractors are included in the Department of Defense report, *100 Companies Receiving the Largest Dollar Volume of Prime Contract Awards.*

[c] Compiled from data reported in the Compustat segment files for our sample of defense contractors.

[d] Segment ROA is segment income divided by segment assets.

TABLE 2

Descriptive Statistics by Segment Type for Firms with Revenues from Defense Contracts (1984–1989) and (1994–1998)

Panel A: Descriptive Statistics, 1984–1989

	Commercial Segments[a]		*Mixed Segments*[a]			*Government Segments*[a]		
Industry[b]	*n*	*Mean Segment ROA*[c,d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*	*Mean Segment ROA*[c,d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*	*Mean Segment ROA*[c,d]
Metal Mining	30	1.5	1	19.1	3.3	—	—	—
Food and Kindred Products	25	16.5	—	—	—	—	—	—
Manufacturing	312	13.6	172	54.7	14.3	79	97.4	19.5
Transportation/Communications/Utilities	10	16.0	—	—	—	—	—	—
Wholesale/Retail	17	6.3	—	—	—	—	—	—
Business Services	11	1.6	—	—	—	—	—	—
Health Services	9	0.5	4	32.2	17.6	6	100.0	9.6
Engineering/Manufacturing Services	6	40.2	8	70.7	9.1	11	97.9	23.8
All	420	12.4	185	54.7	14.1	96	97.6	19.4
Contractor Ranking[e]								
Ranked	246	12.3	103	57.8	14.1	58	97.9	18.1
Unranked	174	12.6	82	50.8	14.1	38	97.1	21.3

(Continued on next page)

TABLE 2 (Continued)

Panel B: Descriptive Statistics 1994–1998

	Commercial Segments[a]		*Mixed Segments*[a]			*Government Segments*[a]		
Industry[b]	*n*	*Mean Segment ROA*[c,d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*	*Mean Segment ROA*[c,d]	*n*	*Percentage of Segment Revenues Derived from Government Contracts*	*Mean Segment ROA*[c,d]
Metal Mining	6	–0.8	—	—	—	—	—	—
Food and Kindred Products	21	19.5	—	—	—	—	—	—
Manufacturing	180	13.2	100	48.5	13.6	37	96.5	13.5
Transportation/Communications/Utilities	7	5.8	—	—	—	—	—	—
Wholesale/Retail	9	10.0	—	—	—	—	—	—
Business Services	13	6.3	—	—	—	—	—	—
Health Services	15	–1.9	3	78.9	4.3	9	99.3	5.8
Engineering/Manufacturing Services	11	17.3	6	55.8	–4.8	5	99.7	14.6
All	262	12.0	109	49.8	12.3	51	97.3	12.3
Contractor Ranking[e]								
Ranked	162	13.9	60	50.0	13.9	28	96.1	17.7
Unranked	100	8.9	49	49.5	10.3	23	98.8	5.6

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[a] Commercial segments have only commercial revenues; mixed segments derive between zero and 90 percent of revenues from government contracts; government segments derive more than 90 percent of revenues from government contracts.

[b] The first digit of each segment's Compustat SIC code determines industry membership.

[c] Segment ROA is segment income divided by segment assets.

[d] Mean Segment ROAs represent unconditional cell means.

[e] Ranked contractors are included in the Department of Defense report, *100 Companies Receiving the Largest Dollar Volume of Prime Contract Awards.*

percent, and whose mixed and commercial segments report an average ROA of 14.1 percent and 12.6 percent, respectively. These unconditional means suggest that ranked defense contractors do not have a market-power advantage over unranked contractors in extracting excess profits through within-segment cost shifting when competition is low. In 1994–1998, ranked firms report similar levels of profitability as in 1984–1989 (government = 17.7 percent, mixed = 13.9 percent, and commercial = 13.9 percent), but unranked contractors report a lower average ROA than in 1984–1989 in all segments (government = 5.6 percent, mixed = 10.3 percent, and commercial = 8.9 percent). This suggests that as competition for defense contracts increases, ranked contractors maintain their mixed-segment profitability while unranked firms' mixed-segment profitability decreases by 3.8 percentage points.

The industry breakdown reported in Table 2 reveals that in both 1984–1989 and 1994–1998 the commercial segments represent a wide range of industries, whereas the mixed and the government segments cluster in: manufacturing, health services, and engineering and manufacturing services. These descriptive statistics support our use of INDUSTRY as a control variable when testing our hypotheses.

IV. RESULTS

Preliminary Analysis

We first investigate whether Lichtenberg's (1992) inference that government contractors enjoy higher ROA than other segments generalizes to our sample. Specifically, we test the relation between the percentage of segment revenues derived from government contracts and segment ROA in 1984–1989. Consistent with Lichtenberg (1992), we find that in 1984–1989, segments with the greatest percent of revenues derived from government contracts report the highest ROAs. Untabulated results suggest that the relation is not linear. Only segments with more than 90 percent of revenues derived from government contracts are significantly more profitable than the commercial segments ($F = 8.97$, $p = 0.003$).[10]

Results of Hypothesis Tests

Panel A of Table 3 summarizes the results of the repeated-measures ANOVA for the full model. We find significant SEGMENT TYPE ($F = 2.33$, $p = 0.099$), YEAR ($F = 3.94$, $p = 0.001$), and RANKED ($F = 3.26$, $p = 0.072$) main fixed effects. Both the interactions SEGMENT TYPE × YEAR ($F = 1.81$, $p = 0.018$) and RANKED × YEAR ($F = 1.74$, $p = 0.071$) are significant. The control variable, INDUSTRY, is also significant ($F = 8.13$, $p = 0.001$). Untabulated results contrasting the least squares mean ROA for the six years 1984–1989 (the early time period) with the least squares mean ROA for the five years 1994–1998 (the late time period) confirm a time period effect ($F = 6.83$, $p = 0.009$).

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[10] We create five categories of segments with increasing percentages of segment revenues derived from government contracts. We tested the relation with 1984–1989 data and five segment categories in the following repeated-measures ANOVA model described in Section III:

$$ROA_{ijklm} = \mu + S_i + R_j + (SR)_{ij} + I_k + F_{ijkm} + Y_l + (SY)_{il} + (RY)_{jl} + (SRY)_{ijl} + \epsilon_{ijklm}. \qquad (2)$$

The only difference is that SEGMENT TYPE now takes on one of five values: (1) zero (*commercial*); (2) greater than zero and less than or equal to 30 percent (*low*); (3) greater than 30 percent and less than or equal to 60 percent (*medium*); (4) greater than 60 percent and less than or equal to 90 percent (*high*); and (5) greater than 90 percent (*government*). To probe the robustness of the inference from this repeated-measures ANOVA, we repeated the test after grouping the segments into five different categories: purely commercial segments and the four quartiles of the variable *percentage of segment revenues derived from government contracts*. The results confirm our inferences that the relation is not linear and that *government* segments outperform *commercial* segments.

TABLE 3

Summary Statistics, Estimated Conditional Cell Means and Planned Contrasts from a Repeated-Measures Analysis of Variance of Defense Contractors' Segment ROA[a]

$$ROA_{ijklm} = \mu + S_i + R_j + (SR)_{ij} + I_k + F_{ijkm} + Y_l + (SY)_{il} + (RY)_{jl} + (SRY)_{ijl} + \epsilon_{ijklm}$$

Panel A: Summary Statistics of Fixed Effects for the Full Model

	F-value	*p-value*
Fixed Effect of Between-Firm Factors		
SEGMENT TYPE	2.33	0.099
RANKED	3.26	0.072
SEGMENT TYPE × RANKED	0.39	0.681
INDUSTRY	8.13	0.001
Fixed Effect of Within-Firm Factors		
YEAR	3.94	0.001
SEGMENT TYPE × YEAR	1.81	0.018
RANKED × YEAR	1.74	0.071
SEGMENT TYPE × RANKED × YEAR	1.06	0.395
n = 1,123; R^2 = 25.9%; F = 2.82, p = 0.001		

Panel B: Test of Hypothesis 1a[b]

Planned Contrasts of Least Squares Mean Segment ROA by Segment Type[c,d]	*F-value*	*p-value*
H1a: Mixed (9.9%) > Commercial (9.5%) Segment ROA in 1984–1989	0.04	0.834
H1a: Mixed (9.9%) > Government (15.7%) Segment ROA in 1984–1989	8.27	0.004
Comparison of Commercial (9.5%) with Government (15.7%) Segment ROA in 1984–1989	10.56	0.001
Comparison of Mixed (8.0%) with Commercial (8.2%) Segment ROA in 1994–1998	0.23	0.634
Comparison of Mixed (8.0%) with Government (8.9%) Segment ROA in 1994–1998	0.10	0.757
Comparison of Commercial (8.2%) with Government (8.9%) Segment ROA in 1994–1998	0.15	0.704

(Continued on next page)

TABLE 3 (Continued)

$$ROA_{ijklm} = \mu + S_i + R_j + (SR)_{ij} + I_k + F_{ijlm} + Y_l + (SY)_{il} + (RY)_{jl} + (SRY)_{ijl} + \varepsilon_{ijklm}$$

Panel C: Test of Hypothesis 1b[b]

Planned Contrasts of Least Squares Mean Segment ROA by Segment Type and Contractor Ranking[c,d]	*F-value*	*p-value*
H1b: Ranked Contractors (10.1%) > Unranked (9.7%) Contractors' Mixed Segment ROA in 1984–1989	0.12	0.727
Comparison of Ranked (9.6%) with Unranked (9.4%) Contractors' Commercial Segment ROA in 1984–1989	0.02	0.890
Comparison of Ranked (13.8%) with Unranked (17.6%) Contractors' Government Segment ROA in 1984–1989	0.34	0.559
Comparison of Ranked (9.5%) with Unranked (6.5%) Contractors' Mixed Segment ROA in 1994–1998	0.14	0.705
Comparison of Ranked (11.1%) with Unranked (5.3%) Contractors' Commercial Segment ROA in 1994–1998	9.44	0.002
Comparison of Ranked (13.4%) with Unranked (4.4%) Contractors' Government Segment ROA in 1994–1998	3.42	0.065

Panel D: Test of Hypothesis 2a[b]

Planned Contrasts of Change across Time Periods in Least Squares Mean Segment ROA by Segment Type[c,d]	*F-value*	*p-value*
H2a: Change in Mixed (1.9%) > Change in Commercial (1.3%) Segment ROA	0.07	0.791
H2a: Change in Mixed (1.9%) > Change in Government (6.8%) Segment ROA	3.89	0.049
Comparison of Change in Commercial (1.3%) with Change in Government (6.8%) Segment ROA	3.97	0.047

Panel E: Test of Hypothesis 2b[b]

Planned Contrasts of Change across Time Periods in Least Squares Mean Segment ROA by Segment Type and Contractor Ranking[c,d]	*F-value*	*p-value*
H2b: Change in Ranked (0.6%) = Change in Unranked (3.2%) Contractors' Mixed Segment ROA	0.01	0.914
Comparison of Change in Ranked (−1.5%) with Change in Unranked (4.1%) Contractors' Commercial Segment ROA	5.61	0.019
Comparison of Change in Ranked (0.4%) with Change in Unranked (13.2%) Contractors' Government Segment ROA	3.56	0.060

(*Continued on next page*)

TABLE 3 (Continued)

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[a] ROA_{ijklm} = a continuous variable ranging from –1 to 1. μ = a constant. S_i = the main effect of the fixed factor SEGMENT TYPE, which takes on one of three values: Commercial, Mixed, or Government. R_j = the main effect of fixed factor RANKED, which takes on one of two values: Ranked or Unranked, to indicate whether the contractor is ranked among the Top 100 defense contractors. I_k = the main effect of the fixed factor INDUSTRY, which takes on one of eight values corresponding to the first digit of the segment's Compustat SIC code, to control for industry effects. F_{ijkm} = a random firm effect, distributed $N(0, \sigma^2_F)$. Y_l = the main effect of the fixed factor YEAR, which takes on one of 11 values, to distinguish each of the years 1984–1989 and 1994–1998. ϵ_{ijklm} = an error term with a structure assumed to be first-order autocorrelation between years within firms.

[b] Test statistics reflect contrasts of least squares means for the years 1984–1989 (early time period) and the years 1994–1998 (late time period).

[c] In contrast to the unconditional (descriptive) mean segment ROAs reported in Table 2, the mean segment ROAs reported in Table 3 are least squares mean estimates conditional on SEGMENT TYPE, RANKED, INDUSTRY, TIME PERIOD, the interaction between SEGMENT TYPE and RANKED and interactions among SEGMENT TYPE, RANKED, and TIME PERIOD. TIME PERIOD reflects the years 1984–1989 (early time period) or 1994–1998 (late time period).

[d] Comparisons without predicted directions do not represent formal hypotheses tests and are reported for completeness.

Hypothesis 1a

Hypothesis 1a posits that a defense contractor's mixed segment ROA is higher than its government or commercial segment ROA when competition for defense contracts is low. However, Panel B of Table 3 (which presents conditional cell means and the results of six contrasts that test for differences in segment ROA by segment type) shows that in the 1984–1989 period, mixed segment profitability is not significantly different from commercial segment profitability (F = 0.04, p = 0.834) and mixed segments are significantly *less* profitable than the government segments (F = 8.27, p = 0.004). These results do not support H1a: mixed segments, where managers have the opportunity to shift costs, do not enjoy relatively higher profitability even in the 1984–1989 low-competition period. Removing observations with extreme values (those with ROAs more than two standard deviations away from the mean) does not affect the inferences.

For completeness, Panel B also reports the results of analogous tests for the 1994–1998 period. Consistent with the 1984–1989 period, mixed segment ROA is not significantly different than that of the commercial segments (F = 0.23, p = 0.634), but the government segment ROA is no longer significantly different from either the mixed segment ROA (F = 0.10, p = 0.757) or the commercial segment ROA (F = 0.15, p = 0.704).[11]

Hypothesis 1b

Hypothesis 1b proposes that ranked defense contractors' mixed segment ROA is higher than unranked contractors' mixed segment ROA when competition for defense contracts is low. However, Panel C of Table 3, which presents the conditional cell means and the results of six contrasts that test for differences in segment ROA by segment type and contractor ranking, shows that in 1984–1989, the ranked firms' mixed segments do not significantly outperform unranked firms' mixed segments (F = 0.12, p = 0.727). These results are consistent with the insignificant interaction SEGMENT TYPE × RANKED for the full model reported in Table 3, Panel A (F = 0.39, p = 0.681) and do not support H1b. Again, removing observations with extreme values does not affect the results of testing H1b.

For completeness, Panel C of Table 3 also shows that in 1984–1989, there is no difference between ranked and unranked contractors' ROAs for either commercial or government segments. Panel C also reports that in 1994–1998, consistent with 1984–1989, ranked contractors' mixed segment ROAs are not significantly different than those of the unranked contractors' mixed segments. During the later time period, however, ranked contractors' commercial segment ROA and government segment ROA significantly exceed the unranked contractors' commercial segment ROA (F = 9.44, p = 0.002) and government segment ROA (F = 3.42, p = 0.065). The difference in ranked vs. unranked contractors' commercial segment ROA is due to extreme values, because the difference is not significant after we remove observations with ROAs more than two standard deviations from the mean. In contrast, the ranked contractors' government segments remain more profitable than those of unranked contractors (F = 7.28, p = 0.007), even after removal of extreme values.

Hypothesis 2a

Panel D of Table 3 presents conditional cell means and the results of three contrasts that test for differences in the change, across the two time periods, in ROA by segment

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[11] Untabulated tests reveal similar results within the subsamples of ranked contractors and unranked contractors. In 1984–1989, ranked and unranked contractors' government segment ROA significantly exceeds the ROAs of both their mixed and commercial segments. In 1994–1998, profitability does not vary significantly across segment types (government, commercial, or mixed) for either ranked or unranked contractors.

type. These contrasts allow us to test H2a, which posits that the decrease in mixed segment ROA across the two time periods is greater than the decline in either commercial or government segment ROA. Panel D shows that, contrary to expectations, defense contractors did not suffer significantly steeper declines in profitability in their mixed segments relative to their commercial segments (F = 0.07, p = 0.791) or their government segments. In fact, the difference between the mixed segments' and the government segments' change in ROA is in the direction *opposite* that predicted by H2a—the ROAs of mixed segments decline *less* than the ROAs of government segments, as is apparent from inspection of the conditional cell means reported in Panel D of Table 3 (F = 3.89, p = 0.049). These results do not support H2a, and removing extreme values does not affect the results of testing this hypothesis. For completeness, Panel D also reports that the drop in government segment ROA is also significantly steeper than the decline in commercial segment ROA (F = 3.97, p = 0.047).

Hypothesis 2b

Panel E of Table 3 presents conditional cell means and the results of three contrasts that test for differences in the change across the two time periods in ranked vs. unranked contractors' segment ROA by segment type. This allows us to test H2b, which posits that there is no difference in the reduction in profitability from 1984–1989 to 1994–1998 between ranked and unranked contractors' mixed segments. Panel E reports that ranked defense contractors' mixed segments did not experience significantly different changes in profitability compared to unranked defense contractors' mixed segments (F = 0.01, p = 0.914). These results are consistent with the insignificant interaction SEGMENT TYPE × RANKED × YEAR for the full sample reported in Panel A of Table 3 (F = 1.06, p = 0.395). Removal of extreme values does not affect the results of testing H2b. Thus, we are unable to reject the null hypothesis.

For completeness, Panel E reports contrasts revealing that the commercial and government segments of unranked firms experience significantly steeper declines in profitability than do commercial and government segments of ranked firms (F = 5.61, p = 0.019 and F = 3.56, p = 0.060, respectively). The difference in the change in ranked vs. unranked contractors' commercial segment ROA is again due to extreme values, because the difference is not significant after we remove observations with ROAs more than two standard deviations from the mean. In contrast, the government segments of unranked firms continue to suffer a significantly steeper decline in profitability than the government segments of ranked firms even after removal of extreme values (F = 8.47, p = 0.004).

Sensitivity Tests

We perform three types of sensitivity analysis on our data. First, our inferences remain the same using several alternative definitions of mixed segments, based on the following percentages of segment revenues derived from government contracts: between 0 and 80 percent; between 0 and 75 percent; between 0 and 66.67 percent; between 10 and 90 percent; between 20 and 90 percent; between 25 and 90 percent; between 33.33 percent and 90 percent; between 20 percent and 80 percent; between 25 percent and 75 percent; between 33.33 percent and 66.67 percent. Second, we run a model controlling for the natural log of segment assets, because segments that derive most of their revenues from government contracts may have the advantage of more fully utilizing government-owned assets, given that these segments are devoted to government work. Again, our inferences remain unchanged. Third, we restrict the 1994–1998 sample to be a subset of the 1984–1989 sample. (The full sample includes usable segments from the NAARS sample of 104 contractors in any year of either time period). This restriction, which reduces the sample to 469 segments in 1984–1989 and 294 segments in 1994–1998 from 27 contractors in both time periods, leads to identical inferences.[12]

Discussion of Potential Nonaccounting Explanations for Unusual Profitability on Government Contracts

We find no evidence that cost shifting is responsible for unusually high profits on government contracts in low-competition environments, despite managers' opportunities to shift costs within mixed segments. These findings are inconsistent with the cost-shifting explanation suggested by Rogerson (1992) and Thomas and Tung (1992). Our results suggest that low competition for defense contracts results in excess profits on government contracts, even without cost shifting. If accounting (cost shifting) does not explain the excess profitability of government contracts, then what else can explain these excess profits? Testing alternative, nonaccounting explanations for the unusual profitability of government contracts is beyond the scope of this study, but we briefly offer possible alternative explanations suitable for exploration in the economics or industrial organization literature.

Our results confirming excess profits on government contracts in the early, low-competition period (1984–1989) are consistent with Leitzel's (1992) "captured regulator" theory, in which the DOD is a captured regulator that acts in the best interests of the defense industry.[13] Our evidence from 1984–1989 is also consistent with the argument that market forces allowed both ranked and unranked contractors to extract excess profits on government work in the 1980s. Given the heightened defense spending in the 1980s, if the defense market were a seller's market, then sellers could have extracted higher profits on this trade even without cost shifting, simply because of market forces.

Our results for the later, higher competition period (1994–1998) suggest that reduced defense spending and increased competition in the 1990s affected ranked and unranked contractors differently. Specifically, unranked contractors' government segments experience steeper declines in ROA in the higher competition period than do ranked contractors' government segments. This disproportionate decline in ranked and unranked government-segment profitability is consistent with the Karpoff et al. (1999) argument that a few influential contractors enjoy preferential treatment. However, ranking among the Top 100 defense contractors could also indicate that these defense contractors have more managerial ability and stability, and ranked firms may have managed the reduction in the defense-contracting market better than unranked firms. Untabulated analyses are consistent with this second explanation. Ranked firms maintain the profitability of their government segments over time by shrinking government-segment assets in response to reductions in government business, whereas unranked firms do not.

V. CONCLUSIONS AND LIMITATIONS

We find that, in 1984–1989, defense contractors' business segments that derive their revenues predominantly from government contracts are significantly more profitable than

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[12] Specifically, in each sensitivity test we still find that (1) in 1984–1989, mixed segment ROA is not significantly higher than either government or commercial segment ROA (H1a); (2) in 1984–1989, the profitability of the ranked contractors' mixed segments is not significantly different from the profitability of unranked contractors' mixed segments (H1b); (3) mixed segments do not suffer significant decreases in profitability from the early (1984–1989) to the late (1994–1998) time period when compared with government or commercial segments (H2a); and (4) ranked defense contractors' mixed segments did not experience a significant change in profitability when compared with the change in unranked contractors' mixed segment ROA (H2b).

[13] See Rogerson (1989, 1994) for further background on the underlying economic incentives in defense contracting.

purely commercial segments or segments with a mix of commercial and government work. These results do not hold in the more competitive 1994–1998 period, when we observe no significant difference in the relative profitability (ROA) across government, commercial, and mixed segments. The equalization of profit rates in 1994–1998 results from a significant decrease in the ROA of the government segments, with the mixed segments showing no significant decrease in profitability over time.

Our primary result is that we find no evidence that the defense contractors' excess profits in the early time period (1984–1989) are attributable to cost shifting within segments. In contrast to the cost-shifting hypothesis, segments with a mix of government and commercial business (which present more opportunities for within-segment cost shifting) are *not* significantly more profitable than segments with predominantly government or only commercial business.

Further analysis provides no evidence that either ranked or unranked defense contractors use cost shifting to exploit a lack of competition. We find no significant difference between the ROAs of ranked and unranked contractors' mixed segments in the low-competition, 1984–1989 time period. Ranked and unranked contractors' highest average segment ROAs in 1984–1989 stem from their government segments, not their mixed segments as the cost-shifting hypothesis predicts. Both ranked and unranked contractors' mixed segment ROAs decline in the later, more competitive time period (1994–1998), but contrary to the cost-shifting hypothesis, the drop in mixed segments' ROAs is not significantly steeper than the drop in commercial or government segments' ROAs, for either ranked or unranked contractors.

In summary, our findings are inconsistent with an accounting (cost shifting) explanation for the excess profitability of defense contractors. Although defense contractors have the opportunity to shift costs (Rogerson 1992; Thomas and Tung 1992), we find no evidence that Lichtenberg's (1992) finding of excess profits on government contracts in the 1980s is attributable to cost shifting. As discussed in the results section, the unusually high profitability reported on government contracts when competition for contracts is low is more likely explained by nonaccounting factors, such as Leitzel's (1992) "captured regulator" hypothesis or that the market for defense goods in the 1980s was a seller's market.

Several limitations hinder our ability to use segment profitability to draw unequivocal inferences about cost shifting. First, we are unable to measure cost shifting directly. We test our hypotheses with the limited segment data that firms report in their financial statements. More definitive tests of the relation between cost shifting and excess profits would require access to firms' cost-accounting records. Second, we are unable to separate defense-contracting from nondefense-contracting revenues. It is possible that incentives and opportunities to shift costs depend on which federal government procurement agency awards the contract. It is also possible that ranked defense contractors are unable to use their market power when negotiating with non-DOD (civil) agencies. Third, we are unable to capture across-segment cost shifting. Although we find no evidence of significant across-segment cost shifting within our sample (see footnote 3), we cannot rule out this possibility.

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The Valuation Implications of Employee Stock Option Accounting for Profitable Computer Software Firms

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ABSTRACT: We use the Ohlson (1995, 1999) and Feltham and Ohlson (1999) valuation models to investigate the market's perception of the economic effect of employee stock options (ESOs) on firm value for a sample of 85 profitable computer software companies. Our results suggest that the market appears to value these firms' ESO expense not as an expense but as an intangible asset (even after controlling for the endogeneity bias arising from the mechanical relation between ESOs and the underlying stock prices). However, we also find a conflict between: (1) the positive manner in which investors appear to value ESO expense, and (2) the negative relation between current ESO expense and future abnormal earnings. This conflict not only could be an artifact of the restrictiveness of the abnormal earnings forecasting equation we estimate, but it also calls into question whether investors assess correctly the effect of ESOs on profitable software firm value.

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Data Availability: *Data are available from the authors.*

I. INTRODUCTION

Employee stock options (ESOs) comprise a sizable portion of executive compensation (Hall 2000, 121; *Business Week* 2002), but financial statement preparers, financial statement users, regulators, and standards setters disagree on how ESOs should be reflected in the financial statements. In June 1993, the Financial Accounting Standards Board (FASB) released *Exposure Draft: Accounting for Stock-Based Compensation* (FASB 1993) (hereafter *Exposure Draft*). The *Exposure Draft* would have required employers to recognize as an intangible asset the fair value of stock options at the grant date, to amortize this asset, and to record the asset's amortization as employee compensation expense.

Two years later, in October 1995, and after more than a decade of deliberations, the FASB issued Statement of Financial Accounting Standards No. 123, *Accounting for Stock-Based Compensation* (FASB 1995) (hereafter SFAS No. 123). SFAS No. 123 requires firms to *disclose* (in footnotes to the financial statements) the *pro forma* effects on earnings of employee compensation expense attributable to amortizing the fair value of employee stock options at the grant date. However, SFAS No. 123 does not generally require firms to *recognize* this ESO-related compensation expense in the income statement, although it encourages firms to do so. Instead, SFAS No. 123 permits firms to use Accounting Principles Board Opinion No. 25, *Accounting for Stock Issued to Employees* (AICPA 1972) (hereafter APB No. 25), which allows the firm not to recognize employee stock compensation expense if the grant meets two requirements at the grant date: the exercise price and the number of options are fixed, and the exercise price equals or exceeds the stock price. Most companies have been careful to meet these two requirements, presumably to avoid recognizing stock option-related expense in the income statement.

In this study, we use the Ohlson (1995, 1999) and Feltham and Ohlson (1999) valuation models to compare the extent to which the APB No. 25, SFAS No. 123, and the *Exposure Draft* approaches to accounting for employee stock options reflect the market's assessment of the effects of employee stock options on firm value for a sample of 85 profitable computer software firms. In particular, we focus on whether investors view ESO expense disclosures required by SFAS No. 123 differently from other components of income.[1] Investors may value ESO expense differently because stock options align the interests of employees and shareholders. Because employee stock options motivate employees to boost productivity, profits, and especially stock price, Stewart (1997, Chapter 6) suggests that employee stock options give rise to a valuable intangible asset. Furthermore, this incentive-aligning motivation extends beyond the vesting period (the period over which SFAS No. 123 requires firms to expense options) to the time that employees exercise their options. This raises the possibility that an intangible asset exists even beyond the vesting period. Thus, unlike most expenses, investors may view ESO expense as providing information about an unbooked intangible asset as well as an expense.

The motivational advantages of employee stock options are likely to be more important in knowledge-intensive industries with significant intellectual capital, such as the computer software industry. Engel (1997) reported that software firms had the highest ratio of options granted to shares outstanding of any industry. Thus, we focus our analysis on the software industry because it provides us with a powerful setting to explore the extent to which the three methods of accounting for employee stock options reflect the market's assessment of the effects of employee stock options on firm value. We restrict our sample to software firms with positive net income to ensure the valuation model is well specified.

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[1] Unless we state otherwise, "stock option expense" refers to stock option expense based on SFAS No. 123 disclosures, i.e., based on amortization of the fair value of employee stock options.

Using a valuation model following from SFAS No. 123—which includes the disclosed ESO expense but not the estimate of the ESO asset—we find that the market appears to value ESO expense not as an expense but as an asset. That is, the market appears to value the ESO expense as if it serves as a proxy for an intangible asset associated with issuance of employee stock options that we omit from the valuation model.

When we use a valuation model following from the *Exposure Draft*—which includes an estimate of the ESO asset as well as the ESO expense—we find that the ESO asset (measured as amortized fair value of employee stock options) is highly value-relevant, even more so than other assets of the firm. This result is consistent with employee stock options creating a valuable intangible asset, i.e., contributed intellectual capital. Surprisingly, our results suggest that investors continue to value ESO expense as an asset even when we include the ESO asset in the valuation model. This suggests that the amortized ESO asset may not capture fully the future net economic benefit that investors expect the firm to reap as a result of issuing the employee stock options. When we repeat this analysis including the unamortized (gross) ESO asset in the valuation model, ESO expense is no longer value-relevant. These results are consistent with investors believing that the intangible asset has a useful life longer than the option-vesting period.

In contrast with the positively valued ESO expense, we find that the market negatively values selling, general, and administrative expense (SG&A), which includes the compensation expense paid to employees engaged in administrative and marketing activities. Thus, our results suggest that for profitable software firms, investors value ESO expense differently from other components of income, and differently from other forms of compensation as reflected in SG&A.

Our results suggest that the approach proposed in the *Exposure Draft* on accounting for stock-based compensation better reflects the market's perception of the effect of employee stock options on profitable software firms' value. In particular, our results are consistent with investors in profitable computer software companies perceiving that employee stock option grants create a valuable asset or investment.

Our study extends the literature examining how investors assess the relation between ESO information and equity values. Aboody (1996) finds a significant negative relation between his estimate of the fair value of outstanding employee stock options and equity market value after controlling for the endogeneity bias induced by the mechanical relation between option values and share prices. Aboody (1996) must estimate ESO fair values because his sample period predates SFAS No. 123. Using data obtained from SFAS No. 123 disclosures, Aboody et al. (2001) and Chamberlain and Hseih (1999) find a negative association between ESO expense and equity value after controlling for the endogeneity bias in a manner similar to Aboody (1996). In contrast to these studies that document a negative effect of employee stock options on firm value, we find a positive association between ESO expense and equity value, even after controlling for endogeneity bias.

Differences between our findings and those of Aboody et al. (2001) and Chamberlain and Hseih (1999) are most likely attributable to sample differences. In particular, sample firms in the other two studies represent a broader set of industries and are not necessarily profitable. It is possible that employee stock options create a valuable intangible asset for profitable firms in knowledge intensive industries, but not for firms in other industries. The evidence in Keating et al. (2002), that their measure of the fair value of employee stock options that Internet firms granted in 1999 is significantly positively associated with equity value, supports our conjecture.

None of these other studies investigates the value-relevance of ESO assets measured in accordance with the *Exposure Draft*, yet we find that this approach to accounting for employee stock options better reflects the market's perception of the effect of employee stock options on profitable software firms' value. However, we cannot use our conclusions as a basis for a general policy recommendation. First, the generalizability of our inferences beyond knowledge-intensive industries is open to question. Second, we find a conflict between the positive manner in which investors appear to value ESO expense, and the negative relation between current ESO expense and future abnormal earnings. This conflict could be an artifact of the restrictiveness of the simple first-order autoregressive abnormal earnings model we estimate. However, it also calls into question whether investors correctly assess the effect of employee stock options on profitable software firms' value.

We organize the remainder of the paper as follows: Section II develops the research design and our predictions; Section III describes the sample and data; Section IV presents the primary findings; Section V presents findings from additional analyses; and Section VI summarizes and concludes the study.

II. RESEARCH DESIGN

Description of Alternative Accounting Methods

Prior to SFAS No. 123, APB No. 25 required firms to use the intrinsic value method to account for employee stock options. The intrinsic value of an option is the difference between its exercise price and the current price of the underlying stock. Intrinsic value excludes the value of the right to purchase the underlying stock at a fixed price for a specified future period. APB No. 25 requires employers to record stock option compensation cost as the difference between the market price of the stock and the amount an employee must pay to acquire the stock on the measurement date (i.e., when the employer knows the number of shares to which employees are entitled and their exercise price). For most ESOs, the measurement date and grant date are the same, although the measurement date may be later if there is uncertainty at grant date regarding the exercise price or the number of shares the employee may acquire.

APB No. 25 does not require employers to recognize or disclose compensation expense for fixed-award employee stock options (i.e., at the grant date, the employer knows the number of shares to which an individual employee is entitled to purchase and their exercise price) if the exercise price equals or exceeds the stock price at grant date. Paragraph 56 of SFAS No. 123 criticizes APB No. 25's use of the intrinsic value method because it allows such nonrecognition while treating performance options, which may be less valuable than fixed options at the grant date, as compensation costs that might have to be recognized. (The number and exercise price of performance options depend on the manager's achievement of a contractual measure of performance.) The business press criticized APB No. 25 as well (e.g., Morgenson 1998; *The Economist* 1999).

Because of these criticisms, coupled with the increasing use of stock options in lieu of salary and cash bonuses during the 1980s, the FASB began work on an alternative to APB No. 25. In 1993, the FASB issued the *Exposure Draft* that would have required employers: (1) to recognize the asset Prepaid compensation for the fair value of granted stock options at their grant date, and (2) to recognize the related compensation expense from amortizing that asset over the options' vesting period.[2] The offsetting entry credits the equity account Options outstanding and is extinguished only as options are forfeited, expired, or exercised.

In paragraph 63 of the *Exposure Draft,* the FASB defends creating the Prepaid compensation asset by stating, "employee stock options represent probable future benefits because employees have agreed to render future services to earn their options. Stock options also reduce future cash outflows otherwise necessary to compensate employees." The *Exposure Draft* engendered much controversy, most notably from preparers who were concerned by the prospect of deducting stock option-related employee compensation expense in the income statement.

With the issuance of SFAS No. 123 two years later, the FASB yielded to political pressure while acceding to its critics. SFAS No. 123 suggests that employers estimate the fair value of an employee stock option at grant date using standard option-pricing models (e.g., the Black-Scholes option valuation model). SFAS No. 123 then allows firms either to disclose *pro forma* or to recognize the effects on earnings of employee compensation expense, computed by amortizing the fair value of employee stock options over the vesting period. Virtually all firms choose the *pro forma* disclosures rather than recognizing compensation expense on the income statement. Whereas the *Exposure Draft* would have required employers to recognize and amortize Prepaid compensation and Options outstanding at grant date, SFAS No. 123 requires firms only to make a memo journal entry for the fair value of the employee stock options at grant date, and permits, but does not require, amortizing the fair value of the options over the option-vesting period.[3] Table 1 summarizes the accounting treatments under the three accounting methods.

Benchmark Valuation Equations Based on APB No. 25

This paper uses the Ohlson (1995, 1999) and Feltham and Ohlson (1999) valuation models to compare the extent to which the APB No. 25, SFAS No. 123, and *Exposure Draft* methods of accounting for employee stock options reflect the market's assessment of firm value. In the valuation equation, the dependent variable is market value of common shares outstanding, and the two independent variables are abnormal earnings and book value of common equity. Following the theoretical work of Ohlson (1999) and Feltham and Ohlson (1999), and the empirical application of Ohlson (1999) by Barth et al. (1999), we also estimate a first-order abnormal earnings forecasting equation for which the dependent variable is abnormal earnings and the independent variables are previous year's abnormal earnings and book value of common equity. Assuming that error terms of the first-order abnormal earnings forecasting equation are unpredictable with a mean of zero, Ohlson (1999, 151) shows that the coefficients of the independent variables in the forecasting equation determine the independent variables' relative importance in the valuation equation. Ohlson's (1999) result—linking the abnormal earnings equation with the valuation equation—is intuitive. It implies that abnormal earnings are more important in valuation if current abnormal earnings are more predictive of next year's abnormal earnings.

Because none of our sample firms recognize ESO expense per SFAS No. 123, reported earnings and book values of common equity reflect the APB No. 25 method of accounting

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[2] An employee's option grant vests at the date that the employee's right to receive or retain stock is no longer contingent on remaining in the employer's service.

[3] SFAS No. 123 does not allow employers to recognize the asset, Prepaid compensation. The Board agreed with respondents to the *Exposure Draft* that firms should not recognize Prepaid compensation because employees are not obligated to render the services required to earn the vesting rights associated with the options.

TABLE 1
Financial Accounting Treatments of Employee Stock Options[a]

Accounting Method	*Grant Date*	*End of Year*	*Exercise Date*
Continue APB No. 25/ SFAS No. 123 disclosure	No entry	No entry	Dr. Cash Cr. Common stock
Adoption of SFAS No. 123 compensation expense recognition	No entry	Dr. Compensation cost Cr. Paid-in capital—Employee stock options	Dr. Cash Dr. Paid-in capital—Employee stock options Cr. Common stock
Exposure Draft on stock-based compensation	Dr. Prepaid compensation Cr. Options outstanding	Dr. Compensation cost Cr. Prepaid compensation	Dr. Cash Dr. Options outstanding Cr. Common stock

[a] Assumes that the stock option is a nonqualified stock option and that the exercise price is set equal to the fair market value of the underlying stock on the grant date. For simplicity, we have omitted the tax-related journal entries.

for employee stock options. The benchmark APB No. 25 abnormal earnings and valuation equations are as follows:

$$\mathrm{AEARN}_{it} = \omega_0 + \omega_1 \mathrm{AEARN}_{i,t-1} + \omega_3 \mathrm{BVE}_{i,t-1} + \varepsilon^A_{it} \tag{1a}$$

$$\mathrm{MVE}_{it} = \alpha_0 + \alpha_1 \mathrm{AEARN}_{it} + \alpha_3 \mathrm{BVE}_{it} + \varepsilon^M_{it}. \tag{1b}$$

AEARN_t is abnormal earnings and equals $\mathrm{NI}_t - r\mathrm{BVE}_{t-1}$; NI_t equals net income before extraordinary items and discontinued operations for fiscal year t; BVE_t is the book value of common equity at the end of fiscal year t; MVE_t is the market value of common shares outstanding at the end of fiscal year t; ε^A and ε^M are error terms; and the i and t subscripts denote firms and years, respectively. Following Dechow et al. (1999) and Barth et al. (1999), we set the expected rate of return on book value of common equity, r, at 12 percent, the long-term return on equities. The error terms, ε^A and ε^M, reflect other information as well as random error. For ease of exposition, we use the same notation for coefficients and error terms across alternative pairs of abnormal earnings and valuation equations.

Although defining AEARN based on net income before extraordinary items and discontinued operations violates the clean surplus assumption in Ohlson (1995), it eliminates potentially confounding effects of large one-time items and is consistent with prior empirical research (e.g., Barth et al. 1999, 2000; Dechow et al. 1999; Hand and Landsman 2000).[4]

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[4] Ohlson (2000) points out that even bottom line net income will not satisfy the clean surplus equation under the pooling-of-interest method of accounting for business combinations and the APB No. 25 method of accounting for employee stock options. In each case, the problem arises from recording equity transactions using nonmarket prices. Future research undoubtedly will address the importance of Ohlson's (2000) observation for empirical work based on equations similar to Equation (1b).

Ohlson (1999, 160) concludes that this approach is justified in empirical work because one-time items have no forecasting ability.

As Barth et al. (1999, 208) observe, including BVE_{t-1} in the abnormal earnings equation partially relaxes the assumption that the cost of capital associated with calculating abnormal earnings is a predetermined rate, because current and lagged BVE are highly correlated. In Equation (1a), ω_1 reflects the persistence of abnormal earnings. Prior research (e.g., Barth et al. 1999, 2000; Dechow et al. 1999; Hand and Landsman 2000) leads us to expect that ω_1, the autocorrelation of abnormal earnings, is positive. Based on Ohlson (1995), we expect α_1 and α_3 to be positive.

Valuation Equations Based on SFAS No. 123

SFAS No. 123 requires crediting the equity account Paid-in capital—Employee stock options when employers debit ESO expense. We do not include Paid-in-Capital—Employee stock options in book value of common equity. Our rationale is that because the dependent variable in the valuation equation is the current market value of common shares outstanding, the related independent variable, book value of common equity, should include only those components of equity that are associated with common shares currently outstanding. Our approach follows Feltham (1995, 16), who also excludes obligations to issue equity from book value of common equity. Also note that if we include Paid-in capital—Employee stock options in book value of common equity, then book value of common equity would not drop when firms expense employee stock options. This result differs from other types of employee compensation expense, which lower both net income and book value of common equity.

Under SFAS No. 123 recognition rules, ESO expense reduces net income to defined *pro forma* net income, with an equal decrease in ending book value of common equity. Therefore, our accounting treatment satisfies the clean surplus equation because the decrease in book value of common equity equals the decrease in net income attributable to ESO expense.[5]

Abnormal earnings and valuation equations that result from applying the SFAS No. 123 approach are as follows:

$$AEARNESO_{it} = \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} + \varepsilon^A_{it} \quad (2a)$$

$$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_3 BVE\_ADJ_{it} + \varepsilon^M_{it}. \quad (2b)$$

MVE_t is defined previously; $AEARNESO_t$ is abnormal earnings and equals $NI_t - rBVE\_ADJ_{t-1} - ESOEXP_t$; $ESOEXP_t$ is ESO expense and equals reported net income less SFAS No. 123 defined *pro forma* net income;[6] BVE_ADJ_t is adjusted book value and equals BVE_t less $ESOEXP_t$ accumulated since 1995;[7] and ε^A and ε^M are error terms. Equations (2a) and (2b) restate earnings and book value of common equity to recognize ESO expense based on SFAS No. 123. If recognizing ESO expense per SFAS No. 123 better reflects investors' perceptions of the underlying economic reality, then Equation (2b) should have more explanatory power than Equation (1b). We make the same coefficient predictions for Equations (2a) and (2b) as we do for Equations (1a) and (1b), ω_1, α_1, and $\alpha_3 > 0$.

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[5] If we include Paid-in capital—Employee stock options in book value of common equity, then our accounting treatment can satisfy the clean surplus equation only by interpreting the credit to Paid-in capital—Employee stock options as an issuance of common stock.

[6] ESOEXP is the additional stock option-based compensation expense, net of a provision for income taxes.

[7] As Table 1 shows, this adjustment of BVE to obtain BVE_ADJ does not take into account that when employees exercise their options, there is a greater increase in book value of common equity under SFAS No. 123 and the *Exposure Draft* than under APB No. 25. This does not play a material role for our sample because most options have three-year or longer vesting periods, so those exercised in 1996 through 1998 come primarily from options granted prior to 1995. Options granted prior to 1995 do not cause a problem at the exercise date because accounting rules under APB No. 25 apply rather than SFAS No. 123.

To assess whether the market values ESOEXP similarly to other components of earnings, we extend Equations (2a) and (2b) by including ESOEXP as an additional independent variable:

$$AEARNESO_{it} = \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_2 ESOEXP_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} + \varepsilon^A_{it} \quad (3a)$$

$$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} + \varepsilon^M_{it}. \quad (3b)$$

If the market values ESOEXP differently from other components of income, then α_2 will differ from zero. Similarly, if current ESOEXP forecasts future abnormal earnings differently from other components of income, then ω_2 will differ from zero. Because ESOEXP is a component of AEARNESO, the total valuation coefficient on ESOEXP equals $-\alpha_1 + \alpha_2$.[8] Thus, if $-\alpha_1 + \alpha_2 = 0$, then ESOEXP is irrelevant for valuation. We test the value irrelevance condition $-\alpha_1 + \alpha_2 = 0$ against the alternative that $-\alpha_1 + \alpha_2 \neq 0$. Evidence that ESOEXP is value irrelevant would suggest that investors perceive that APB No. 25's method of accounting for ESOs captures correctly their underlying economic effects on firm value. Analogously, the total autoregressive coefficient on ESOEXP equals $-\omega_1 + \omega_2$. Thus, if $-\omega_1 + \omega_2 = 0$, then ESOEXP is irrelevant for forecasting abnormal earnings. We test the forecasting irrelevance condition $-\omega_1 + \omega_2 = 0$ against the alternative that $-\omega_1 + \omega_2 \neq 0$.

Valuation Equations Based on the *Exposure Draft*

The *Exposure Draft* on accounting for stock-based compensation would have required employers to debit an asset, Prepaid compensation, at the grant date and to credit Options outstanding. For the same reasons that we do not consider Paid-in capital—Employee stock options as part of book value of common equity in the SFAS No. 123 approach, we also do not include Options outstanding as part of book value of common equity in the *Exposure Draft* approach. Therefore, BVE_ADJ and the other variables have the same values as in the SFAS No. 123 approach, and Equations (3a) and (3b) also apply to the *Exposure Draft* approach.

However, just as Equations (3a) and (3b) isolate the ESO expense component of AEARNESO, we now can isolate the Prepaid compensation component of BVE_ADJ (ESOASSET). This is the book value of the Prepaid compensation related to unvested employee stock options outstanding at the fiscal year-end. The abnormal earnings and valuation equations resulting from the *Exposure Draft* rules are as follows:

$$\begin{aligned} AEARNESO_{it} = {} & \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_2 ESOEXP_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} \\ & + \omega_4 ESOASSET_{i,t-1} + \varepsilon^A_{it} \end{aligned} \quad (4a)$$

$$\begin{aligned} MVE_t = {} & \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} \\ & + \alpha_4 ESOASSET_{it} + \varepsilon^M_{it}. \end{aligned} \quad (4b)$$

In comparison to Equations (3a) and (3b), Equations (4a) and (4b) allow the ESO asset

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[8] Viewing the total valuation coefficient as $-\alpha_1 + \alpha_2$ follows the approach adopted by Ohlson (1999, 150) and Barth et al. (1999, 209). The same concept applies to the abnormal earnings forecasting equation (Barth et al. 1999, 208).

(ESOASSET) to have a total coefficient that differs from other components of equity book value. If the market does not value ESOASSET as an asset, then $\alpha_3 + \alpha_4 = 0$. If the market values ESOASSET similarly to other net assets, then we expect $\alpha_4 = 0$.[9] However, if the market values ESOASSET more highly than other net assets, then we expect $\alpha_4 > 0$. Such a finding would support Stewart's (1997, Chapter 6) view that employee stock options give rise to a valuable intangible asset—a form of intellectual capital. We make no predictions for ω_3 and ω_4.

Estimating ESOASSET

Because the FASB did not adopt the *Exposure Draft,* we have to estimate ESOASSET as $\text{ESOASSET}_t = \text{ESOASSET}_{t-1}$ + fair value of ESOs granted in year t − fair value of forfeitures during year t − before-tax ESO expense in year t. This updating equation for ESOASSET_t is analogous to the updating equation for net property, plant, and equipment. The fair value of employee stock options granted in year t is analogous to new purchases of property, plant, and equipment; forfeitures during the year t are analogous to sales and retirement of net property, plant, and equipment; and ESO expense is analogous to depreciation. The *Exposure Draft* expected firms to use this updating equation for ESOASSET_t.[10]

We operationalize the estimate of ESOASSET_t by Equation (5), which approximates the updating equation for ESOASSET_t. For t = 1996, 1997, and 1998:

$$\text{ESOASSET}_t = \sum_{y=1995}^{t} [(\text{ESO\_G}_y - \text{ESO\_F}_y) \times \text{FV\_ESO}_y] - [\text{ESOEXP}_y/(1-T)] \quad (5)$$

where:

ESO_G_y = number of options the employer granted during year y;
ESO_F_y = number of options employees forfeited during year y;
FV_ESO_y = weighted-average fair value per share for ESOs the employer granted during year y; and
T = assumed marginal corporate tax rate of 0.35 (all sample firms are profitable in all sample years).

In Equation (5), we measure the fair value of employee stock options granted in year *y* as ESO_G_y multiplied by FV_ESO_y and the before-tax ESO expense as $\text{ESOEXP}_y/(1 - T)$. The summation in Equation (5) begins in the year 1995 because the SFAS No. 123 ESO expense numbers include the expense of options granted from 1995 and going forward.

Data limitations require that we approximate the other term in Equation (5), the fair value of forfeitures. We would prefer to calculate the fair value of forfeitures by multiplying the number of options employees forfeited by the weighted-average of the fair value per share of forfeitures. However, we do not have the weighted-average fair value of forfeitures by year. Therefore, we approximate the fair value of forfeitures in any year y by multiplying the number of options employees forfeited during year y by the weighted-average fair value per share for employee stock options granted in year y. This approximation is exact for employee stock options that are granted and forfeited in the same year. To the extent that forfeitures stem from grants in earlier years, the ESO_F_y multiplied by FV_ESO_y subtraction in Equation (5) attributable to forfeitures is misestimated. However, the mean (median) ratio of option forfeitures to options outstanding for the firms in our sample is approximately 11 percent (7 percent). This suggests that it is unlikely that measurement error in ESOASSET relating to forfeitures will be material.[11]

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[9] This approach is in the spirit of Feltham and Ohlson (1995, Proposition 3). However, the accounting for ESOASSET described in Table 1 implies that ESOASSET should be correlated, but not perfectly correlated, with Options outstanding. Therefore, ESOASSET and Options outstanding will tend to net to zero, in which case an alternative plausible definition of value irrelevance is $\alpha_4 = 0$. Results reported later reveal that ESOASSET is value relevant for most regressions under either definition.

[10] However, the *Exposure Draft* required that the fair value of ESOs granted in year t be reduced by estimated forfeitures over the life of the grant, and that actual forfeitures during year t be reduced by estimated forfeitures during year t. Estimated forfeiture numbers are not available to us. Although estimated forfeitures over the life of the grant and the sum of estimated forfeitures in each year of the grant eventually cancel, excluding estimated forfeitures in the updating equation results in an overestimate of ESOASSET_t.

We estimate cross-sectional regressions for each abnormal earnings equation for 1997 and 1998 and for each valuation equation for 1996, 1997, and 1998, as well as pooled regressions for each equation using year fixed-effects. To minimize the effects of scale differences across sample firms (Barth and Clinch 2001), we estimate all equations after scaling by number of shares outstanding at fiscal year-end.[12] We report regression t-statistics using White- (1980) corrected standard errors and consider t-statistics with associated two-sided p-values less than 0.05 as statistically significant. We do not report constants from pooled fixed-effects regressions.

III. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

Sample firms meet the following data requirements:

1. Listed in software or related service industry on Compustat, SIC Code = 7370–7379, as of October 1999.
2. Financial statement and market value data available on Compustat for 1995, 1996, 1997, and 1998.
3. Report positive earnings in 1996, 1997, and 1998.
4. Report nonnegative owner's equity in 1995, 1996, and 1997.
5. Employee stock option data from footnote disclosures mandated under SFAS No. 123 are available from firms' 1996, 1997, and 1998 financial statements.

We adopt the first requirement, which results in a potential sample of 815 firms, because employee stock options are material motivational mechanisms in knowledge-intensive computer software firms. The second requirement, which ensures data availability, reduces the potential sample to 488 firms.

The third requirement, which further reduces the potential sample to 129 firms, constrains the sample to profitable, mature, and stable companies. Unreported diagnostic tests motivated by Hayn (1995) reveal that when we estimate valuation models separately for firms with positive and negative earnings, abnormal earnings have positive and negative coefficients, respectively. The negative coefficient for firms with negative earnings suggests that our valuation model is not specified correctly for such firms. For example, earnings for such firms may be a proxy for other information, such as information about research and development investment, especially for high-growth or start-up companies. Therefore, we restrict our tests to firms with positive earnings where our valuation model is better specified. Consequently, the generalizability of our inferences is limited because most computer software firms were unprofitable during the years sampled.

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[11] We also estimated versions of Equations (4a) and (4b) using alternative measures of ESOASSET resulting from different assumptions regarding options exercisable, exercised, and forfeited, including setting forfeitures to zero. Inferences based on these alternative versions essentially are the same as those reported below.

[12] In all regressions, we include the inverse of number of shares as a regressor and suppress the intercept, although inferences are unaltered when we include an intercept. In addition, inferences from unscaled and equity book value deflated regressions essentially are the same as those from the reported per share regressions.

The fourth requirement, which eliminates ten additional firms, constrains the sample to companies with positive beginning owner's equity to ensure that the firm's cost of capital in calculating abnormal earnings ($rBVE_{t-1}$) is positive. The fifth requirement, which eliminates an another 29 firms, ensures we can obtain ESO data to estimate the accounting variables. Following application of the fifth requirement, we eliminated five more firms with substantially larger total market capitalization relative to the remaining sample. Inferences relating to reported findings are unaffected by inclusion of these five additional firms. The final sample includes 85 software firms that meet the data requirements in 1996, 1997, and 1998, ensuring we estimate valuation and abnormal earnings equations on a common sample of firms.

Annual SFAS No. 123 ESO expenses include amortizations from options granted beginning in 1995. Paragraph 54 of SFAS No. 123 notes that most sample firms require a vesting period of three years or longer. Therefore, the full effects of applying SFAS No. 123 on *pro forma* net income will not be evident until about 1998. Thus, our ESOEXP and ESOASSET estimates are likely understated in 1996 and 1997, which could affect their regression coefficients in those years.[13]

Table 2's descriptive statistics reveal that, on average, the market value of equity far exceeds the book value of equity for our sample firms. In contrast to prior research on firms drawn from a broad cross-section of industries (Barth et al. 1999, 2000), our sample firms' abnormal earnings are positive, on average. These two findings are likely partially attributable to restricting our sample to firms with positive earnings, but they are also consistent with computer software companies' being high-growth entities. Mean (median) per-share values of ESO expense (ESOEXP) increase over 1996, 1997, and 1998 from 0.09 to 0.15 to 0.24 (0.05 to 0.11 to 0.18), respectively, which is consistent with systematic understatement of ESOEXP in the first two years following the statement adoption in 1995. On average, the ESO asset associated with amortized unvested stock options (ESOASSET) is approximately 7 percent of equity book value.

Table 3 reveals that most of the variables are correlated with each other. Notably, equity market value is highly correlated with: (1) reported equity book value and abnormal earnings, and (2) equity book value and abnormal earnings adjusted to reflect ESO expense.

IV. RESULTS

Benchmark (APB No. 25) Models

Panels A and B in Table 4 present the benchmark results from estimating abnormal earnings and valuation Equations (1a) and (1b). In Panel A's abnormal earnings forecasting Equation (1a), the coefficient on lagged abnormal earnings (ω_1) is positive and significant in 1998 and in the pooled estimation. The annual and pooled sample coefficient estimates, which range from 0.31 to 0.47, are similar to those reported in prior research for samples based on broad cross-sections of firms drawn from many industries (Dechow et al. 1999; Barth et al. 1999). Hence, profitable software firms appear to exhibit persistence in abnormal earnings similar to that for firms in other industries. The coefficient on equity book value (ω_3) is near zero, which is consistent with prior research (Barth et al. 1999), but, in contrast to prior research, it is insignificant in both years and in the pooled estimation.

Turning to the valuation Equation (1b), Panel B of Table 4 reveals that except for the abnormal earnings coefficient in 1996, the coefficients on abnormal earnings and equity

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[13] It is difficult to predict the exact effect of measurement error, absent knowledge of the correlation structure of the measurement error with the variables included in a particular regression.

TABLE 2
Descriptive Statistics for Market Value, Book Value, Abnormal Earnings, ESO Expense, and ESO Asset Estimates, for a Sample of 85 Profitable Computer Software Firms

Variable	*Year*	*n*	*Median*	*Mean*	*Std. Dev.*
MVE	Pooled	255	19.75	23.15	14.09
	1998	85	24.44	27.14	16.15
	1997	85	21.13	23.11	12.98
	1996	85	16.13	19.20	11.81
BVE	Pooled	255	4.44	5.03	3.13
	1998	85	5.33	5.82	3.26
	1997	85	4.28	5.02	3.09
	1996	85	3.70	4.26	2.86
BVE_ADJ	Pooled	255	4.22	4.74	3.08
	1998	85	4.77	5.33	3.24
	1997	85	4.00	4.75	3.06
	1996	85	3.52	4.13	2.84
AEARN	Pooled	255	0.18	0.20	0.42
	1998	85	0.21	0.22	0.47
	1997	85	0.20	0.19	0.40
	1996	85	0.16	0.20	0.40
AEARNESO	Pooled	255	0.08	0.06	0.46
	1998	85	0.01	0.00	0.52
	1997	85	0.09	0.05	0.41
	1996	85	0.08	0.11	0.43
ESOEXP	Pooled	255	0.10	0.16	0.18
	1998	85	0.18	0.24	0.23
	1997	85	0.11	0.15	0.15
	1996	85	0.05	0.09	0.11
ESOASSET	Pooled	255	0.25	0.37	0.44
	1998	85	0.26	0.49	0.54
	1997	85	0.26	0.39	0.43
	1996	85	0.13	0.24	0.28

Variable definitions (we deflate to a per-share basis by dividing by number of shares outstanding at fiscal year-end):

MVE = market value of common shares outstanding at fiscal year-end;
BVE = book value of common equity as of fiscal year-end;
BVE_ADJ = BVE minus ESOEXP accumulated since 1995;
AEARN = abnormal earnings measured as net income before extraordinary items and discontinued operations, minus 0.12 × BVE (lagged one year);
AEARNESO = abnormal earnings after ESO expense measured as net income before extraordinary items and discontinued operations, minus ESOEXP, minus 0.12 × BVE_ADJ (lagged one year);
ESOEXP = ESO expense measured as reported net income minus *pro forma* net income per SFAS No. 123 disclosure; and
ESOASSET = ESO asset associated with unvested employee stock options as of fiscal year-end. ESOASSET corresponds to the *Exposure Draft*'s asset Prepaid compensation calculated from Equation (5).

TABLE 3
Pearson Correlations among Variables: Market Value, Book Value, Abnormal Earnings, ESO Expense, and ESO Asset Estimates

Variable	*MVE*	*BVE*	*BVE_ADJ*	*AEARN*	*AEARNESO*	*ESOEXP*	*ESOASSET*
Panel A: Pooled across Years (n = 255)							
MVE	1.00						
BVE	0.49	1.00					
BVE_ADJ	0.46	0.99	1.00				
AEARN	0.42	0.14	0.15	1.00			
AEARNESO	0.27	0.09	0.13	0.94	1.00		
ESOEXP	0.32	0.14	0.04	−0.02	−0.37	1.00	
ESOASSET	0.30	0.01	−0.03	0.03	−0.12	0.41	1.00
Panel B: 1998 (n = 85)							
MVE	1.00						
BVE	0.48	1.00					
BVE_ADJ	0.45	0.99	1.00				
AEARN	0.38	0.02	0.03	1.00			
AEARNESO	0.22	0.01	0.06	0.92	1.00		
ESOEXP	0.28	0.05	−0.08	−0.07	−0.45	1.00	
ESOASSET	0.24	−0.10	−0.14	0.02	−0.14	0.38	1.00
Panel C: 1997 (n = 85)							
MVE	1.00						
BVE	0.45	1.00					
BVE_ADJ	0.43	1.00	1.00				
AEARN	0.47	0.09	0.09	1.00			
AEARNESO	0.37	0.05	0.07	0.94	1.00		
ESOEXP	0.26	0.13	0.04	0.08	−0.25	1.00	
ESOASSET	0.18	−0.04	−0.07	−0.01	−0.11	0.31	1.00
Panel D: 1996 (n = 85)							
MVE	1.00						
BVE	0.45	1.00					
BVE_ADJ	0.44	1.00	1.00				
AEARN	0.45	0.37	0.37	1.00			
AEARNESO	0.36	0.33	0.35	0.97	1.00		
ESOEXP	0.24	0.09	0.03	−0.07	−0.32	1.00	
ESOASSET	0.47	0.08	0.06	0.08	−0.03	0.41	1.00

[a] See Table 2 for definitions of variables.

book value are significantly positive in the pooled sample and across all years. The abnormal earnings coefficients, which range from 9.07 to 16.28, are somewhat higher than those reported in Barth et al. (1999) for firms in most other industries, which is somewhat unexpected given that autoregressive persistence parameters (ω_1) for our sample firms' abnormal earnings are similar to those reported in Barth et al. (1999).

TABLE 4

Regressions of Abnormal Earnings and Equity Market Value Based on APB No. 25 Accounting for Stock-Based Compensation Expense, for 85 Profitable Software Firms[a]

Panel A: Abnormal Earnings Equation

$$AEARN_{it} = \omega_0 + \omega_1 AEARN_{i,t-1} + \omega_3 BVE_{i,t-1} + \varepsilon^A_{it}$$

		CONSTANT		*AEARN*		*BVE*		*Model*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Adjusted R^2*
Pooled	170	—	—	0.39	2.66	0.02	1.69	0.203
1998	85	−0.55	−1.15	0.47	2.99	0.02	1.09	0.221
1997	85	−0.11	−0.28	0.31	1.31	0.02	1.55	0.179

Panel B: Valuation Equation

$$MVE_{it} = \alpha_0 + \alpha_1 AEARN_{it} + \varepsilon_3 BVE_{it} + \varepsilon^M_{it}$$

		CONSTANT		*AEARN*		*BVE*		*Model*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Adjusted R^2*
Pooled	255	—	—	13.12	5.25	3.58	21.57	0.780
1998	85	−21.05	−1.91	14.25	4.55	3.87	12.86	0.809
1997	85	−10.18	−1.16	16.28	5.17	3.45	14.02	0.797
1996	85	−0.83	−0.11	9.07	1.81	3.26	12.56	0.720

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[a] See Table 2 for definitions of all variables except CONSTANT.

Estimates for all equations are in per-share form with intercepts surpressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and associated t-statistics for pooled fixed-effects regressions are not reported.

The coefficients on common shareholders' equity book value range from 3.26 to 3.87. Untabulated test statistics reveal that these coefficients significantly exceed 1.00. Absent accounting conservatism, the Ohlson (1995) model predicts the equity book value coefficient to equal 1.00. Conservative accounting results in systematic understatement of equity book value and, hence, systematic overstatement of its valuation coefficient.

SFAS No. 123 Models

Panels A and B in Table 5 present the results of estimating the abnormal earnings forecasting and valuation Equations (2a) and (2b) under the SFAS No. 123 approach, which requires disclosure of ESO-related compensation expense but not an ESO-related asset. Regarding Equation (2a), Panel A reveals that the coefficient on lagged abnormal earnings adjusted for employee stock option expense (ω_1) is positive and once again significant in 1998 and in the pooled estimation. The annual and pooled sample coefficient estimates range from 0.35 to 0.46, which are similar to those reported for unadjusted abnormal earnings in Table 4. As with unadjusted equity book value in Table 4, the coefficient estimates for adjusted equity book value (ω_3) are near zero and insignificant. The notable difference between Table 4, Panel A, and Table 5, Panel A, is that the abnormal earnings forecasting model explanatory power drops by approximately 40 percent when we adjust abnormal earnings and equity book value for ESO expense. Although we cannot directly compare R^2 values because dependent variables differ across the two tables, the drop in explanatory power is nonetheless consistent with ESO expense having a different abnormal

TABLE 5
Regressions of Abnormal Earnings and Equity Market Value Based on SFAS No. 123 Accounting for Stock-Based Compensation Expense Recognition Rules, for 85 Profitable Software Firms[a]

		CONSTANT		*AEARNESO*		*BVE_ADJ*		*Model Adjusted*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	R^2
Panel A: Abnormal Earnings Equation								
$AEARNESO_{it} = \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} + \varepsilon^A_{it}$								
Pooled	170	—	—	0.41	2.88	−0.00	−0.28	0.124
1998	85	−0.67	−1.32	0.46	2.64	−0.00	−0.20	0.128
1997	85	−0.23	−0.61	0.35	1.69	−0.00	−0.12	0.108
Panel B: Valuation Equation								
$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_3 BVE\_ADJ_{it} + \varepsilon^M_{it}$								
Pooled	255	—	—	5.47	2.09	4.11	20.00	0.724
1998	85	−24.85	−1.82	4.44	1.03	4.52	12.13	0.738
1997	85	−8.57	−0.95	10.47	2.99	3.95	12.87	0.743
1996	85	−2.38	−0.31	4.05	0.99	3.63	12.10	0.692

[a] See Table 2 for definitions of all variables except CONSTANT.

Estimates for all equations are in per-share form with intercepts surpressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and asscociated t-statistics for pooled fixed-effects regressions are not reported.

earnings persistence parameter from the other earnings components. That is, adjusting equity book value and abnormal earnings for ESO expense in Panel A of Table 5, without permitting ESO expense to have a different total coefficient from other expense components of income, induces model misspecification. Alternatively, the drop in explanatory power could also be attributable to measurement error in the adjusted abnormal earnings and equity book value variables, induced by noisy ESO adjustments.

Turning to the valuation Equation (2b), Panel B of Table 5 reveals that the coefficients on adjusted abnormal earnings are positive in all estimations, but significant only in 1997 and for the pooled sample. The coefficients on adjusted equity book value are significantly positive in the pooled sample and across all years. The AEARNESO valuation coefficients and model explanatory power for Panel B are lower in Table 5 than in Table 4. Again, the drop in explanatory power, which is significant, is consistent with either investors' assessing ESO expense differently from other expenses, or with ESO adjustments inducing measurement error in accounting variables.[14] Tables 6 and 7, in which we permit ESO expense to have a separate coefficient, provide evidence on which interpretation is more descriptively valid.

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[14] We determine whether model explanatory power differs between the two non-nested valuation Equations (1b) and (2b) using the Vuong (1989) likelihood ratio test, which permits comparison of explanatory power of two alternative models without assuming under the null that either model is the correct model. The explanatory power for Equation (1b) is significantly higher than that of Equation (2b) at less than the 0.01 level in 1997, 1998, and for the pooled sample, and at the 0.05 level in 1996.

To assess incremental effects of ESO expense (ESOEXP) in the forecasting and valuation equations, Panels A and B in Table 6 present the results of estimating the abnormal earnings and valuation Equations (3a) and (3b), which permit ESOEXP to have different coefficients from adjusted abnormal earnings. Panel A indicates that ESOEXP has predictive ability for the next period AEARNESO. In particular, the total coefficient on ESOEXP ($-\omega_1 + \omega_2$) is significantly negative in 1998 and for the pooled sample, indicating that higher current ESO expenses predict lower future abnormal earnings.

Panel B of Table 6 reveals strong evidence that ESOEXP is value-relevant. The coefficients on ESOEXP range from 43.61 to 54.48 and are significant in all three years and for the pooled sample, indicating that investors value ESOEXP significantly differently from other earnings components. Moreover, the total ESOEXP coefficient ($-\alpha_1 + \alpha_2$) is significantly positive in all three years and for the pooled sample, which is consistent with investors viewing ESOEXP as an asset rather than an expense.

The abnormal earnings equation results in Panel A of Table 6 suggest that the ESOEXP valuation coefficient should be negative, not positive. The Ohlson (1995) model predicts that if higher ESOEXP leads to lower future abnormal earnings, then the market should value ESOEXP negatively. One explanation for finding that ESOEXP has a positive coefficient in the valuation equation and a negative coefficient in the abnormal earnings forecasting equation is that our models omit an intangible asset (ESOASSET) that is associated with issuance of employee stock options. Thus, we now investigate whether investors still appear to value ESOEXP positively even after we control for the ESOASSET using the *Exposure Draft* model approach.

Exposure Draft Models

In Table 7, Panels A and B present results of estimating the abnormal earnings and valuation Equations (4a) and (4b) under the *Exposure Draft* approach, which allows the total ESOEXP coefficients to differ from those on adjusted abnormal earnings and also includes ESOASSET, an estimate of the ESO asset measured as amortized fair value of unvested employee stock options. Panel A reveals that including ESOASSET does not help predict abnormal earnings and that its inclusion has little effect on the full ESOEXP coefficient in the abnormal earnings regression ($-\omega_1 + \omega_2$). As in Panel A for Table 6, ESOEXP is significantly negatively associated with next year's adjusted abnormal earnings, in 1998 and for the pooled sample.

Turning to the valuation Equation (4b), as in Panel B for Table 6, the incremental coefficient on ESOEXP is significantly positive. Relative to Table 6, the incremental ESOEXP coefficient drops by approximately one-third when we include ESOASSET. The total ESOEXP coefficient ($-\alpha_1 + \alpha_2$) is significantly positive; therefore, ESOEXP is value-relevant. Regarding ESOASSET, its incremental coefficient is significantly positive, ranging from 7.11 to 19.97, and tests confirm that the total coefficient ($\alpha_3 + \alpha_4$) is significantly positive, so ESOASSET is also value-relevant. These results are consistent with investors' not only valuing ESOASSET as a firm asset, but also with their valuing ESOASSET more highly than other net assets. This finding also supports Stewart's (1997) view that employee stock options create an intangible asset—a form of intellectual capital.

Do Investors Positively Value ESO Expense Even Though It Is Negatively Related to Next Period's Abnormal Earnings?

The evidence in Table 7 that investors positively value ESOEXP even though ESOEXP is negatively related to future abnormal earnings is contrary to theory (Ohlson 1999, 151).

TABLE 6
Regressions of Abnormal Earnings and Equity Market Value Based on SFAS No. 123 Accounting for Stock-Based Compensation Expense Recognition Rules, with Stock-Based Compensation Expenses Included as a Separate Regressor, for 85 Profitable Software Firms[a]

Panel A: Abnormal Earnings Equation

$$AEARNESO_{it} = \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_2 ESOEXP_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} + \varepsilon^A_{it}$$

		CONSTANT		AEARNESO		ESOEXP		BVE_ADJ			Forecasting Relelvance Test AEARNESO = 0
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*	*p-value*
Pooled	170	—	—	0.35	2.41	−0.61	−1.83	0.01	0.76	0.148	0.00
1998	85	−0.55	−1.18	0.41	2.36	−0.75	−1.50	0.01	0.68	0.167	0.00
1997	85	−0.21	−0.55	0.31	1.39	−0.37	−1.02	0.01	0.36	0.106	0.09

Panel B: Valuation Equation

$$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} + \varepsilon^M_{it}$$

		CONSTANT		AEARNESO		ESOEXP		BVE_ADJ			Value Relelvance Test ESOEXP − AEARNESO = 0
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*	*p-value*
Pooled	255	—	—	12.58	5.63	45.84	10.28	2.90	17.14	0.823	0.00
1998	85	−25.40	−2.44	13.50	4.64	43.61	7.14	3.12	10.72	0.852	0.00
1997	85	−15.45	−1.74	14.45	4.51	45.34	5.48	2.91	10.05	0.823	0.00
1996	85	−5.71	−0.81	10.26	2.24	54.48	4.48	2.60	10.02	0.767	0.00

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[a] See Table 2 for definitions of all variables except CONSTANT.

Estimates for all equations are in per-share form with intercepts surpressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and associated t-statistics for pooled fixed-effects regressions are not reported.

TABLE 7

Regressions of Abnormal Earnings and Equity Market Value Based on *Exposure Draft* Accounting for Stock-Based Compenation, with Stock-Based Compensation Expense and Asset Included as Separate Regressors, for 85 Profitable Software Firms[a]

													Forecasting Relevance Tests	
		CONSTANT		*AEARNESO*		*ESOEXP*		*BVE_ADJ*		*ESOASSET*		*Model Adjusted*	*ESOEXP − AEARNESO = 0*	*BVE_ADJ + ESOASSET = 0*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	R^2	*p-value*	*p-value*
Panel A: Abnormal Earnings Equation														
				$AEARNESO_{it} = \omega_0 + \omega_1 AEARNESO_{i,t-1} + \omega_2 ESOEXP_{i,t-1} + \omega_3 BVE\_ADJ_{i,t-1} + \omega_4 ESOASSET_{i,t-1} + \varepsilon^A_{it}$										
Pooled	170	—	—	0.35	2.39	−0.61	−1.50	0.01	0.74	−0.00	−0.02	0.143	0.00	0.94
1998	85	−0.56	−1.20	0.41	2.35	−0.77	−1.27	0.01	0.64	0.02	0.12	0.157	0.00	0.82
1997	85	−0.20	−0.53	0.31	1.36	−0.30	−0.61	0.01	0.39	−0.05	−0.34	0.096	0.19	0.79
Panel B: Valuation Equation														
				$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} + \alpha_4 ESOASSET_{it} + \varepsilon^M_{it}$										
Pooled	255	—	—	11.81	5.75	33.59	7.89	2.73	15.96	9.24	4.01	0.843	0.00	0.00
1998	85	−26.87	−2.47	12.61	4.75	34.02	5.41	3.00	10.61	7.11	2.19	0.864	0.00	0.00
1997	85	−16.77	−2.24	14.22	4.93	34.38	4.27	2.72	8.82	8.30	2.36	0.840	0.01	0.00
1996	85	−8.92	−1.78	8.56	2.06	27.87	2.82	2.27	8.12	19.97	3.73	0.821	0.05	0.00

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[a] See Table 2 for definitions of all variables except CONSTANT.

Estimates for all equations are in per-share form with intercepts surpressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and associated t-statistics for pooled fixed-effects regressions are not reported.

Why is the total ESOEXP coefficient negative in the abnormal earnings forecasting models, and yet positive in the valuation models? Either our empirical models are misspecified, or investors do not correctly assess the effect of ESO expense on profitable software firms' value. We consider each in turn.

One explanation is that our first-order autoregressive model of abnormal earnings is not descriptively valid.[15] For example, employee stock options may create value that does not manifest in earnings until two or more years in the future, but the single-period lag structure in our autoregressive abnormal earnings forecasting equation does not capture this effect. Unfortunately, we cannot improve the specification of the abnormal earnings forecasting equation by extending the lag structure beyond a single period, because our sample spans only three years.

A second explanation for the unexpectedly positive values of ESOEXP's total coefficients in the valuation equations is that the amortized ESO asset does not fully capture the effect of the intangible asset on firm value. Thus, we consider an alternative measure of ESOASSET in an attempt to improve the specification of the valuation equation.

The evidence in Panel B of Table 7 that ESOEXP's total coefficient remains positive even after we include ESOASSET in the valuation equation, suggests that our measure of the ESO intangible asset ESOASSET does not fully capture the valuation effect of the intangible asset. We consider an alternative measure, the gross (unamortized) ESO asset, ESOASSET_G, which equals ESOASSET plus before-tax accumulated amortization measured as pre-tax ESOEXP cumulated since 1995. This alternative measure represents the extreme view that employee stock options create an intangible asset that has an infinite useful life. Panel A in Table 8 presents results of estimating the valuation Equation (4b) using ESOASSET_G as the measure of the ESO asset. Because the ESO asset is unamortized, there is no ESO expense. Thus, we include AEARN instead of AEARNESO, and BVE instead of BVE_ADJ. We also include ESOEXP in the estimating equation, even though there is no ESO expense when the ESO asset is not amortized. If the unamortized ESO asset measures the intangible ESO asset with less error than does its amortized counterpart, then ESOEXP may no longer be value-relevant when we include ESOASSET_G rather than ESOASSET in the valuation equation. The Ohlson (1995) model permits us to include ESOEXP with ESOASSET_G by viewing ESOEXP as "other information."

As with ESOASSET in Panel B of Table 7, the incremental coefficient estimates on ESOASSET_G are positive and significant (although marginally so in 1997), ranging from 7.09 to 24.99, and tests confirm ESOASSET_G is value-relevant. The key distinction is that in all estimations, α_2, the coefficient on employee stock option expense (ESOEXP) is not significantly positive (in fact, it is negative in 1996). These findings are consistent with investors valuing the ESO asset as if it had an infinite life. Consistent with the intellectual capital perspective, the results suggest that the asset created at grant date has a useful life in excess of the option-vesting period.

A third explanation for finding negative values for the total ESOEXP coefficient in the abnormal earnings forecasting model, and positive coefficients in the valuation models, questions whether investors in profitable computer software companies correctly assess the effect of employee stock options on firm value. Although we favor the model-misspecification interpretation for the different ESOEXP coefficients, we are unable to rule out the market-inefficiency interpretation based on our tests.

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[15] Barth et al. (1999, 221) make a similar observation in the context of the valuation implications of accruals and cash flows.

TABLE 8

Alternative Regressions of Equity Market Value Based on *Exposure Draft* Accounting for Stock-Based Compensation, with Stock-Based Compensation Expense and Asset Included as Separate Regressors, for 85 Profitable Software Firms[a]

Panel A: Valuation Regression with an Alternative Measure of Employee Stock Option Asset that Adds Back Accumulated Amortization

$$MVE_{it} = \alpha_0 + \alpha_1 AEARN_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE_{it} + \alpha_4 ESOASSET\_G_{it} + \varepsilon_{it}^{M}$$

		CONSTANT		*AEARN*		*ESOEXP*		*BVE*		*ESOASSET_G*			*Value Relevance Test BVE + ESOASSET_G = 0*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*	*p-value*
Pooled	255	—	—	12.10	5.78	−1.49	−0.18	2.82	17.41	8.40	3.36	0.837	0.00
1998	85	−27.37	−2.46	12.76	4.52	−0.90	−0.08	2.98	10.83	7.09	2.06	0.862	0.00
1997	85	−17.06	−2.19	14.48	4.85	0.91	0.07	2.84	9.73	7.18	1.78	0.833	0.00
1996	85	−10.70	−2.15	8.33	2.17	−29.10	−1.94	2.59	10.70	24.99	3.76	0.824	0.00

Panel B: Valuation Regression that Includes a Growth Proxy

$$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} + \alpha_4 ESOASSET_{it} + \alpha_5 GROWTH_{it} + \varepsilon_{it}^{M}$$

															Value Relevance Tests	
		CONSTANT		*AEARNESO*		*ESOEXP*		*BVE_ADJ*		*ESOASSET*		*GROWTH*			*ESOEXP − AEARNESO = 0*	*BVE_ADJ + ESOASSET = 0*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*	*p-value*	*p-value*
Pooled	255	—	—	10.57	5.66	26.61	7.09	2.70	16.38	5.13	2.52	8.53	6.92	0.869	0.00	0.00
1998	85	−27.74	−2.58	12.15	5.15	27.52	5.16	2.89	10.18	4.31	1.61	8.79	3.84	0.883	0.01	0.00
1997	85	−23.94	−2.57	11.78	4.45	25.81	3.64	2.83	9.85	1.94	0.55	10.33	4.89	0.879	0.05	0.07
1996	85	−15.42	−2.70	7.38	1.96	19.07	1.69	2.31	7.96	15.10	2.72	6.26	2.86	0.839	0.23	0.00

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[a] See Table 2 for definitions of all variables except CONSTANT, GROWTH, and ESOASSET_G. ESOASSET_G = ESOASSET + pre-tax ESOEXP accumulated since 1995.

GROWTH = 1 (0) if sales growth in the most recent year is above (below) the sample median for the sample year.

Estimates for all equations are in per-share form with intercepts surpressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and associated t-statistics for pooled fixed-effects regressions are not reported.

V. ADDITIONAL ANALYSES

Growth

The findings reported in Panel B of Table 7 indicate that ESOASSET is value-relevant. If its value-relevance is attributable to capturing the growth potential in future cash flows arising from contributed intellectual capital, then controlling for an alternative measure of growth in the valuation equation should reduce the value-relevance of the ESO intangible asset (see Barth et al. 2001, 96–97). If ESOASSET retains explanatory power after controlling for the alternative growth measure, then it reflects value—possibly arising from contributed intellectual capital—beyond that reflected in the additional growth measure.

We re-estimate the valuation Equation (4b) after including an indicator variable equal to 1 (0) if sales growth in the most recent year is above (below) the median for our sample firms in that year. Panel B in Table 8 indicates that investors significantly value the growth proxy. In addition, its inclusion reduces the incremental coefficient on ESOASSET to insignificant levels in 1997 and 1998, which suggests that ESOASSET reflects future sales growth potential. However, the total coefficient on ESOASSET ($\alpha_3 + \alpha_4$), remains positive and significant except in 1997, indicating ESOASSET does reflect dimensions of value beyond sales growth, perhaps growth in expected future cash flows arising from employees' contributed intellectual capital.

Elimination of Price Endogeneity Bias

Aboody (1996) notes that because ESO values increase with prices of underlying stocks, regressing stock prices on ESO values creates an endogeneity problem. We use two different approaches to ensure that our finding that investors positively value ESOASSET is not merely an artifact of this endogeneity bias. First, we replace ESOASSET with an instrument that is uncontaminated by the firm's stock price—the number of unvested employee stock options (UNVESTED_ESO). We determine UNVESTED_ESO's value-relevance using its incremental coefficient, because the number of unvested employee stock options cannot be a component of equity book value. Second, we adopt a two-stage instrumental variable approach following Aboody et al. (2001) and Chamberlain and Hseih (1999). The two-stage procedure purges ESO expense of endogeneity bias by regressing ESO expense on various disclosed option-pricing model inputs, the vesting period, and the number of options granted, and then replacing ESO expense with the fitted value from the first-stage regression in their second-stage valuation regression. We follow this same procedure by replacing ESOEXP with its fitted (or predicted) value ESOEXP_PRD and ESOASSET with ESOASSET_PRD, which we estimate in a similar way to ESOEXP_PRD.[16] We include AEARN instead of AEARNESO and BVE instead of BVE_ADJ because we do not adjust abnormal earnings and equity book value for the predicted ESO expense (which is, of course, not a disclosed amount). Thus, we determine value-relevance for ESOEXP_PRD and ESOASSET_PRD using their incremental coefficients, α_2 and α_4.

Table 9 reports the results of estimating the valuation Equation (4b) regressions incorporating the two approaches to eliminate endogeneity bias. These results reveal that we cannot attribute the positive valuation of ESOASSET to endogeneity bias. In particular, the incremental coefficients on the number of unvested employee stock options

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[16] The first-stage regression includes an estimate of the firm's stock price volatility, expected option life, the risk-free rate, dividend yield, and the number of unvested options outstanding. Aboody et al. (2001) use the number of options granted instead of the number of unvested options outstanding. Unreported findings reveal that using the number of options granted does not affect our inferences.

TABLE 9
Alternative Regressions of Equity Market Value Based on *Exposure Draft* Accounting for Stock-Based Compensation, which Eliminate Price Endogeneity Bias, for 85 Profitable Software Firms[a]

Panel A: Valuation Regression with UNVESTED_ESO (number of unvested employee stock options outstanding) Substituted for ESOASSET

$$MVE_{it} = \alpha_0 + \alpha_1 AEARNESO_{it} + \alpha_2 ESOEXP_{it} + \alpha_3 BVE\_ADJ_{it} + \alpha_4 UNVESTED\_ESO_{it} + \varepsilon^M_{it}$$

		CONSTANT		*AEARNESO*		*ESOEXP*		*BVE_ADJ*		*UNVESTED_ESO*			*Value Relevance Test* *ESOEXP − AEARNESO = 0*
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*	*p-value*
Pooled	255	—	—	12.58	5.82	28.44	5.58	2.60	13.56	75.03	4.95	0.838	0.00

Panel B: Valuation Regression: Instrumental Variables Approach

$$MVE_{it} = \alpha_0 + \alpha_1 AEARN_{it} + \alpha_2 ESOEXP\_PRD_{it} + \alpha_3 BVE_{it} + \alpha_4 ESOASSET\_PRD_{it} + \varepsilon^M_{it}$$

		CONSTANT		*AEARN*		*ESOEXP_PRD*		*BVE*		*ESOASSET_PRD*		
Year	*n*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Coefficient*	*White t-statistic*	*Model Adjusted R^2*
Pooled	255	—	—	12.80	5.41	36.18	6.27	2.71	13.32			0.815
Pooled	255	—	—	11.05	4.64	−3.14	−0.36	2.68	14.07	20.68	5.39	0.831

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[a] See Table 2 for defintions of all variables except CONSTANT, UNVESTED_ESO, ESOEXP_PRD, and ESOASSET_PRD.

UNVESTED_ESO = number of unvested employee stock options outstanding deflated by number of shares outstanding at fiscal year-end.

ESOEXP_PRD and ESOASSET_PRD = fitted values from estimations of ESOEXP and ESOASSET using exogenous variables in a system of simultaneous equations similar to those used by Aboody et al. (2001).

The exogenous variables for determing the fitted values ESOEXP_PRD and ESOASSET_PRD (related to components of the Black-Scholes option pricing model) are as follows: firm's stock price volatility; expected life of the option; discount rate; dividend yield; UNVESTED_ESO; AEARN (defined in Table 2); BVE (defined in Table 2).

Estimates for all equations are in per share form with intercepts suppressed.

Included in the scaled models is the regressor CONSTANT (the inverse of number of shares outstanding at fiscal year-end) to estimate the intercept for the corresponding unscaled models.

CONSTANT coefficients and associated t-statistics for pooled fixed-effects regressions are not reported.

(UNVESTED_ESO) and the predicted ESO asset (ESOASSET_PRD) are significantly positive.[17] Notably, in contrast to Aboody et al. (2001) and Chamberlain and Hseih (1999), who find a negative relation between equity market value and *predicted* ESO expense, Panel B reveals that the valuation coefficient on the ESO expense variable (ESOEXP_PRD), α_2, remains significantly positive when the ESO asset variable (ESOASSET_PRD) is omitted from the estimating equation. However, when ESOASSET_PRD is included in the estimating equation, the ESOEXP_PRD coefficient (α_2) is insignificantly different from zero.

Differences between our findings and those of Aboody et al. (2001) and Chamberlain and Hseih (1999) are most likely attributable to sample differences. In particular, sample firms in the other two studies represent a broader set of industries and are not necessarily profitable. Employee stock options may very well create a valuable intangible asset for profitable firms in knowledge-intensive industries, but not for firms in other industries. For example, Keating et al. (2002) estimate valuation models for Internet firms in the period surrounding the Internet downturn in the first quarter of 2000. They find that their measure of the fair value of employee stock options granted in 1999 is significantly positively associated with equity value, consistent with our finding that the ESO asset is positively associated with share prices. However, their estimate of employee stock options granted in 1999 is neither a book value variable nor an earnings variable but rather a value-relevant variable outside the accounting system.

ESO Valuation vs. SG&A Valuation

The evidence in Panel B of Table 7 that investors value ESO expenses (ESOEXP) positively even when the valuation model includes a measure of the intangible asset (ESOASSET) raises the question of whether other compensation creates intangible value for the firm. Ideally, we would test this proposition by estimating the valuation of compensation expense. However, because we do not have access to compensation expense data, we instead use selling, general, and administrative expense (SG&A), which is a noisy proxy for compensation expense. First, it excludes salaries embedded in the cost of goods sold. Second, SG&A includes costs that are not entirely compensation, such as advertising expense and research and development expense, each of which reflects priced but unrecorded intangible assets related to innovative activities (Landsman and Shapiro 1995; Aboody and Lev 1998; Hall 1999; Hall et al. 1999). The effect of these noncompensation costs should bias toward finding a positive valuation effect for SG&A.

To test the proposition that investors value SG&A differently from other components of AEARNESO, we estimate Equation (4b) including SG&A as a separate regressor, which permits SG&A to have a pricing effect different from AEARNESO. Untabulated findings indicate that the incremental SG&A coefficient is not significantly different from zero, indicating that it is not valued differently from other expense components of income. In addition, including SG&A does not affect inferences relating to ESOEXP and ESOASSET.

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[17] Finding a positive pricing coefficient on the number of unvested options does not imply that firms can increase their stock prices simply by issuing stock options. Aboody (1996) and our own unreported analyses from a regression in which we substitute the number of vested options (VESTED_ESO) in place of ESOASSET in Equation (4b) yield a negative coefficient on VESTED_ESO. This is consistent with the value to the firm of the intangible capital provided by the vested options being smaller than their diluting effect on ownership value for present common shareholders.

Most significantly, in contrast to SG&A, ESOEXP still has a positive incremental association with equity value.[18] Thus, our results suggest that for profitable software firms, investors value ESO expense differently from other components of income, and differently from other forms of compensation as reflected in SG&A.

Alternative Definition of Computer Software Firms

A final robustness test estimates Equations (4a) and (4b) using a narrower definition of profitable computer software firms. In their paper on the value-relevance of software capitalization, Aboody and Lev (1998) include firms from SIC codes 7370 to 7372. Limiting our sample to these three codes could produce a more powerful test of whether investors value ESOASSET as an intangible asset—if intellectual capital is a larger component of these "purer" software firms—although this restriction reduces our annual sample to 66 firms. Untabulated results are similar to those reported for the full sample of 85 firms.

VI. SUMMARY AND CONCLUSIONS

This paper compares the extent to which the accounting methods specified by APB No. 25, SFAS No. 123, and the *Exposure Draft* reflect the market's assessment of the effects of employee stock options (ESOs) on firm value, using the Feltham and Ohlson (1999) valuation model for a sample of 85 profitable computer software firms. For each method we estimate abnormal earnings and valuation equations. The fundamental question we address is whether investors view profitable software firms' ESO expenses similarly to other expenses.

Findings from estimating equations under the SFAS No. 123 approach (which requires only disclosure of the ESO expense) indicate that similar to other expenses, current ESO expense forecasts lower abnormal earnings in the subsequent year. However, contrary to the Ohlson (1999) model's prediction given a negative relation between current ESO expense and future abnormal earnings, investors appear to value ESO expense as an asset. One explanation is that the SFAS No. 123 models omit an intangible asset. Findings from estimating equations under the *Exposure Draft* approach (which would have required recognition of an ESO asset as well as an ESO expense) suggests that investors perceive the ESO asset as value-relevant. In addition, our results suggest that investors value this asset more highly than they do other net assets of the firm, which is consistent with employee stock options creating a valuable intangible asset (i.e., contributed intellectual capital). Interestingly, our results suggest that investors continue to value ESO expense as an asset even when we include the amortized ESO asset in the valuation model. This result suggests that the amortized ESO asset may not capture fully the future net economic benefit of employee stock options. When we repeat this analysis using the unamortized (gross) ESO asset, the ESO asset remains value-relevant while ESO expense does not, which is consistent with the market expecting the ESO asset to have a useful life longer than the option-vesting period.

We interpret our results based on the market-efficiency assumption. However, we find a conflict between the positive manner in which investors value ESO expense vs. the negative relation between ESO expense and future abnormal earnings. This finding calls into question whether investors in profitable software companies assess the effect of employee stock options correctly. Another explanation is that our simple first-order autoregressive structure for abnormal earnings is not descriptively valid for ESO expense. We also interpret the study's results assuming that we measure the ESO accounting variables without error, even though firms understate ESO expense in our first two sample years.

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[18] We also estimated Equation (4a) including SG&A as a separate regressor. As with the valuation equation, untabulated findings indicate that SG&A has a negative total coefficient, significant in 1997 and for the pooled sample.

Taken together, we interpret our study's findings as suggesting that the *Exposure Draft* approach to accounting for stock options best captures the market's perception of the economic effect of employee stock options on profitable computer software firm value. Most notably, the results suggest that investors in profitable software companies perceive that employee stock options create an intangible asset that they value more highly than other assets of the firm. This is consistent with employee stock options playing an important role in employee attraction, motivation, and retention (Ittner et al. 2002) in knowledge-intensive industries. We leave it to future research to determine the extent to which our findings generalize to firms in other industries or to nonprofitable entities.

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Federal Tax Legislation as an Implicit Contracting Cost Benchmark:
The Definition of Excessive Executive Compensation

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ABSTRACT: We examine how tax legislation that restricted firms' deductions of CEO compensation above $1 million *reduced* the implicit contracting cost of compensation for firms that were expected to pay *below* that amount and that were not directly affected by the law change. We find that firms that expected to pay their CEOs less than $1 million actually *increased* their CEOs' cash compensation, contrary to Congress's expectations. Moreover, the magnitude of the unexpected increase in compensation is proportional to how far the CEO's expected compensation fell below Congress's new $1 million reasonable-compensation standard. Thus, our study provides evidence that some of the largest U.S. corporations responded in a manner contrary to policymakers' expectations. Our findings also support the theory of implicit contracting costs, by demonstrating that many firms reacted in an economically rational fashion when a change in the tax law decreased their implicit costs of CEO compensation.

Keywords: *business taxation; compensation; implicit contracting costs.*

Data Availability: *The data are publicly available from the sources indicated.*

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I. INTRODUCTION

In 1993, Congress passed legislation that restricted firms' ability to deduct (for tax purposes) "excessive" executive compensation, defined as nonperformance-based compensation of more than $1 million. The new tax code section, 162(m), applies to the compensation (generally salary and bonus) of the five highest paid executives in publicly traded firms, effective January 1, 1994. Prior work has indicated that many firms that paid their executives *more* than $1 million reduced or restructured the compensation of their Chief Executive Officers (CEOs) to preserve its tax deductibility (Balsam and Ryan 1996; Johnson, Nabar, and Porter 1999; Johnson, Porter, and Shackell 1999; Perry and Zenner 2001; Livingstone 2002). This reduction in executive compensation is consistent with CEOs' sharing in changes in the tax costs of their compensation. These findings are also consistent with the congressional objectives of reducing executive compensation and tying it more closely to firm performance.

In this study, we examine the effect of this legislation on firms paying their CEOs *less* than the $1 million limit—*firms not directly affected by the tax change.* We find that, contrary to congressional intent, these firms *increased* their CEOs' cash compensation in proportion to the extent to which each CEO's expected compensation, absent the tax-law change, fell short of the new $1 million congressional standard for reasonable compensation. We do not observe this effect in years before the tax-law change, and the effect persists after the law's first effective year, further supporting our argument that the results arise from the tax legislation.

Our results are consistent with the theory of implicit contracting costs. If Congress's $1 million standard influenced third parties' beliefs about how much compensation was reasonable, then the implicit contracting costs of CEO compensation for firms paying less than $1 million fell when Congress enacted a standard for reasonable compensation at a level higher than such firms otherwise would have paid. A reduction in implicit contracting cost should induce firms to pay more CEO compensation to earn greater revenue and additional profits.[1]

Our sample firms suffered no direct tax effect because each CEO's expected compensation was less than $1 million. This absence of any direct tax effect increases our ability to isolate implicit costs. In contrast, it is difficult to isolate implicit costs in firms paying more than $1 million, because they suffered increases in taxes as well as implicit costs, both of which we expect to reduce compensation.

To rigorously derive our hypothesis, we model a firm's profit maximization decision with regard both to CEO compensation and to the third parties' belief-revision process. The model demonstrates that increasing CEO pay in response to decreased marginal compensation cost increases firms' expected profits, if paying additional compensation generates greater firm revenues. However, this inference does not imply that firms suddenly raised their CEOs' compensation by substantial amounts. Rather, we expect firms to implement this change incrementally, by awarding bonuses and raises that are somewhat larger than they would be otherwise.

Our theory is applicable to all firms whose compensation practices are publicly disclosed. It suggests that public scrutiny of CEO compensation imposes costs on firms and that firms with the most widely disclosed compensation policies should be most sensitive to implicit compensation costs. Correspondingly, our empirical analyses are based on firms with compensation practices that are especially open to public scrutiny—a subset of the *Forbes* top 800 CEOs (*Forbes* 1993). This sample selection criterion increases the power of our tests at the expense of possibly limiting the generalizability of our results to the general population of firms.

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[1] This is based on the assumptions that (1) firms increase CEOs' salaries to retain them, which leads to greater revenue, or profit, than having to replace the CEO, (2) CEOs work harder to earn a greater bonus, the variable portion of their compensation, and (3) firms maximize profits when marginal cost equals marginal revenue. These assumptions are consistent with CEOs working more diligently for the firm to earn additional bonus payments. This could be reflected in, for example, acquiring additional training and exercising more care in all decisions, in addition to spending additional hours on the job—that is, greater effort on all dimensions. The assumptions are also consistent with firms maximizing their value by paying a competitive CEO salary.

Our evidence illuminates potential pitfalls arising from the indirect (as opposed to the direct) effects of tax legislation that led many firms to take actions contrary to policymakers' stated objectives. This study is also relevant to academic researchers concerned with executive compensation because it documents how taxes affect firms' compensation decisions. This study also extends prior work on firms' implicit contracting costs that has focused on the *level* of implicit contracting costs (e.g., Bowen et al. 1995; Tiras et al. 1998). Our evidence of implicit contracting cost effects associated with changes in, rather than the levels of, implicit contracting costs supports the notion that firms respond in an economically rational manner to changes in implicit contracting costs.

Section II discusses implicit contracting costs and their relation to perceived excessive CEO compensation. Section III formally develops our hypothesis, and Section IV describes the sample selection. Section V describes our empirical model, Section VI describes our results, and Section VII concludes.

II. IMPLICIT CONTRACTING COSTS AND EXECUTIVE COMPENSATION

Implicit Contracting Costs

Implicit contracting costs arise because firms' actions affect third parties—such as consumers, regulators and governmental authorities, shareholders, employees and labor unions, suppliers, and creditors—in ways not negotiated for and not captured in specific, tangible contracts. Corporate actions that have attracted implicit costs include: racial bias; sweatshop labor in foreign countries; child labor; dealings with countries such as South Africa before the end of apartheid; and participation in (or failure to participate in) international boycotts of religious groups. Third parties can, in turn, impose costs on firms through their purchasing, negotiating, regulating, lobbying, or selling decisions. For example, consumers can adjust their demands for products and services. Financial market participants, especially pension funds, universities, and charitable organizations, can base their selections of stocks and bonds on political and social criteria (and some have publicly stated that they do: University of Toronto 1978; University of Minnesota 1991; Social Investment Forum 1998), altering the demand for and the price of firms' public offerings. Employees and labor unions can impose costs by their willingness, or lack thereof, to negotiate compensation and benefits. Suppliers can increase their negotiating pressure based on their perceptions of excess firm resources as reflected in CEO compensation. Finally, regulators and governments can impose monitoring and direct (tax) costs on firms perceived as out of line, as demonstrated by the enactment of Section 162(m). In short, implicit contracting costs are noncontractual costs that third parties impose on firms by reacting adversely to firms' behaviors.

Prior accounting research has shown that firms react to the *level* of implicit costs and how they affect firm value. Bowen et al. (1995) conclude that firms select income-increasing inventory and depreciation methods as the relative importance of their implicit contracts with their customers, suppliers, employees, and short-term creditors increases. These results are consistent with third parties imposing lower implicit costs on firms with better reputations for fulfilling implicit contracts. In related research, Tiras et al. (1998) find that estimates (obtained from a social investment firm) of a firm's reputation for honoring customers, employees, community, and environmental implicit contracts are positively related to the market's valuation of both net book value and current earnings. Taken together, prior work suggests that third parties' implicit claims can reduce firms' market values, and that firms take steps to improve their reputations for honoring implicit contracts to reduce their implicit contracting costs.

The Heightened Implicit Cost of CEO Compensation Prior to Passage of Section 162(m)

In the early 1990s, politicians, labor activists, and the media frequently claimed that executive compensation was excessive. For example, Murphy (1995) describes the effect on Hyatt Regency when *Time* magazine (McCarroll 1992, 48) highlighted Hyatt shareholders' claims that the firm pays "outrageously generous" compensation and that the "company's board [should] resign" for "lack of oversight." Consistent with our prior discussion of how third parties impose implicit costs, Murphy concluded that widely held perceptions of excessive CEO compensation could adversely affect Hyatt by influencing consumers' choice of accommodations, reducing other employees willingness to accept pay concessions, and fostering labor-union activism:

> Public disclosure effectively ensures that executive contracts in publicly held corporations are not a private matter between employers and employees, but are influenced by the media, labor unions, and by political forces operating inside and outside companies. *Compensation committees...will respond to the political pressures by implicitly or explicitly imposing a ceiling on pay....* (Murphy 1995, 730) (emphasis added)

Prior to the enactment of Section 162(m) in 1993, several pension funds—including those representing state workers in California, New York, and Wisconsin—targeted companies for paying excessive compensation (Murphy 1995). In 1991, Congressman Martin Sabo introduced a pay cap bill that would limit tax-deductible executive compensation to only 25 times the pay of the lowest-paid company employee. This would have placed the deduction cap at about $200,000: "[T]hat's what the President of the United States gets. Why should some corporate executives get more?" (McCarroll 1992, 48). In early 1991, Senator Carl Levin conducted Senate hearings on excessive executive compensation, and in June of that year introduced legislation that called for detailed disclosures of executive compensation and allowed compensation proposals on corporate ballots. In October 1992, the Securities and Exchange Commission (SEC 1992) issued regulations requiring comprehensive new executive pay disclosures, as well as shareholder participation in compensation determination. Executive compensation was also an issue in the 1992 presidential election; President Clinton indicated that "the tax code would no longer subsidize excessive pay" (Rosenbaum 1993, A1).

This process culminated in Congress's passing Section 162(m). Because stock options generally qualify as performance-based and long-term plans are normally paid and taxed in the future, cash compensation (salary and bonus) is the primary focus of the $1 million limit. The definition of performance-based compensation specifically excludes salary and all such payments over $1 million are nondeductible to the firm.[2]

Thus, all firms faced more scrutiny of their CEOs' compensation because of the newly required disclosures and because of these widely publicized debates. Not all firms were similarly affected, however. When more-detailed CEO compensation data became publicly available, the level of implicit costs imposed on a firm depended on third parties' beliefs about how much CEO compensation was "too much." The distribution of such beliefs was likely quite diverse before the passage of Section 162(m). At that time, third parties had no objective, widely accepted, or commonly held benchmark.

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[2] Bonuses can qualify as performance-based if (1) compensation is contingent on attaining one or more objective, preestablished, and written performance goals, (2) the compensation committee of the board of directors (required to be composed solely of two or more outside directors) establishes objective goals, (3) the firm discloses the terms of the compensation agreement to the shareholders, who must approve it by a majority vote before payment is made, and (4) the compensation committee verifies that the goals are met. However, firms paying cash compensation less than $1 million did not need to take this action, and few did so.

To the extent that third parties adopted the new $1 million reasonable-compensation standard, its enactment reduced the implicit costs of compensation for firms paying their CEOs less than $1 million. If new perceptions of reasonableness were combinations of prior beliefs and Congress's standard, then the further a CEO's expected compensation fell below $1 million, the greater the reduction in the firm's marginal implicit cost of executive compensation when Congress enacted the $1 million standard.

Common perceptions that CEO compensation is excessive do not necessarily mean that CEOs are overpaid, or that there is a failure in the market for corporate control requiring congressional intervention. Even if firms set compensation at economically efficient levels, there are at least two reasons why third parties can believe compensation is excessive. First, information asymmetry exists between firm management and third parties. Outsiders evaluate the reasonableness of CEO compensation using only publicly available information, such as firm size, profitability, and growth, so they could reach different conclusions than managers who are privy to more complete information. Second, individuals may simply disagree about the trade-off between the social benefits of CEO pay packages that maximize firm value vs. the social costs of widening income differences among members of society.[3]

Thus, the implicit-contracting-cost hypothesis we examine does not assume that firms pay their CEOs more than economically efficient levels of compensation. Rather, our hypothesis requires only that: (1) outside parties believed that executive compensation was excessive (consistent with the previously discussed anecdotal evidence), and (2) Congress's action changed those beliefs. Our theoretical and empirical analyses address how Congress's legislation could, and did, alter such perceptions.

III. HYPOTHESIS DEVELOPMENT

Congress apparently believed (or concluded that their constituents believed) there was a failure in the market for corporate control and that some CEOs were overpaid in comparison to the economic value of their services. Our model, however, assumes that firms make rational, efficient decisions (although our predictions hold even if firms pay more or less than economically efficient amounts). Specifically, we assume that: (1) the marginal cost of CEO compensation equals CEO marginal revenue in equilibrium;[4] (2) third party beliefs that CEO compensation is excessive impose implicit costs on the firm; and (3) the passage of Section 162(m) changed those beliefs in the direction of Congress's new $1 million benchmark.

Theoretical Model

To examine the effect of implicit costs on firms' compensation costs, and the effect of the Section 162(m) limits on those implicit costs, we consider the following model of a firm's CEO compensation decision and third party belief-revision:

$$\pi_{CEO} = Rev(\$) - \$ - IC(\$ - RC) \tag{1}$$

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[3] Whether such compensation was or is "excessive" is beyond the scope of this paper. Livingstone (2001) investigates this issue.

[4] We focus on revenue because many implicit costs are reflected in revenue changes. However, our analyses are equally valid substituting income or market value for the term "revenue."

where:

π_{CEO} = the value to the firm of the CEO's actions, a function of the costs and benefits of compensation;
Rev = the CEO's actions' revenue function, increasing in compensation, $\partial Rev/\partial(\$) > 0$, at a decreasing rate, $\partial Rev^2/\partial(\$)^2 < 0$;[5]
\$ = executive compensation;
IC = the implicit cost function, increasing in the perceived unreasonableness of CEO compensation, $\partial IC/\partial(\$ - RC) > 0$, at an increasing rate, $\partial IC^2/\partial(\$ - RC)^2 > 0$; and
RC = third parties' average perception of reasonable CEO compensation for the firm.

Our model applies to firms paying CEO cash compensation less than \$1 million because firms paying more than this amount experienced not only an increase in implicit costs, but also an increase in actual tax costs. Our study focuses on changes in implicit costs, and therefore, our empirical tests focus on firms paying their CEOs less than \$1 million because they experienced no direct tax change; thus, their behaviors were affected only by changes in implicit costs.

This model assumes that firms' revenues are increasing in CEO compensation, and, because of implicit costs, that it costs the firm more than one dollar to increase the CEO's compensation by one dollar. The model assumes that implicit costs are increasing in "excessive compensation." This is represented by the implicit cost function, IC, which takes a general form, increasing in the difference between actual compensation, \$, and "reasonable" compensation, RC—what third parties find reasonable.[6] We assume RC is a function of not only objective factors, such as firm size and performance, but also of third parties' subjective political and social beliefs. The tax-law revision was unlikely to affect these objective measures, so we treat them as constant and omit them from the model.[7]

We model third parties' belief-revision in response to new information (such as Congress's setting a compensation standard) as follows:

$$RC_{after} = \alpha RC_{before} + (1 - \alpha)\text{ New Information} \tag{2}$$

where α is the weight placed on prior beliefs about reasonable compensation and New Information is, for our purposes, the \$1 million benchmark. The weight parameters allow for a very significant or a very small change in perceptions due to the new information.[8]

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[5] Consistent with standard economic models and to ensure a closed-form solution, we assume that firm revenues are increasing in compensation at a decreasing rate, and that implicit costs are increasing at increasing rates; see, for example, Varian (1984, Chapter 1).

[6] This model does not explicitly assume that implicit costs increase in the level of CEO compensation, independent of reasonable costs. However, the results are sufficiently general so that the same predictions follow if we add an additional implicit cost term to the model, capturing increasing implicit costs as a function of the level of CEO compensation with first and second derivative properties similar to the IC(.) function above.

[7] However, our empirical specifications control for objective factors identified in prior research.

[8] This model assumes that α does not vary across CEO compensation. This assumption is likely too strong because the \$1 million limit is unlikely to be a relevant benchmark for firms with very large or very small compensation. It is an empirical, not theoretical, question exactly where α will shift sufficiently to make Congress's law revision irrelevant. Because our sample consists of that subset of *Forbes* top 800 CEOs paid at or below \$1 million, this limit is likely to be an important benchmark for our sample firms. However, our empirical analyses investigate this issue further.

Proposition: The change in equilibrium CEO compensation is increasing in the distance between a CEO's prior reasonable compensation and the $1 million benchmark.

Proof We analyze Equations (1) and (2) to predict the effects on CEO compensation of changes in implicit costs arising from the tax law change. We begin by analyzing the firm's profit maximization problem with regard to CEO compensation to determine how compensation varies with changes in third party perceptions of reasonableness.

The first-order condition for maximizing the firm's return to CEO compensation is:

$$\frac{\partial\pi}{\partial\$} = \frac{\partial \mathrm{Rev}(\$)}{\partial(\$)} - 1 - \frac{\partial \mathrm{IC}(\$ - \mathrm{RC})}{\partial(\$ - \mathrm{RC})} = 0. \tag{3}$$

The profit-maximizing, equilibrium compensation that satisfies Equation (3), $\$^*$, is conditional on the level of RC, although RC is not a choice variable for the firm. We can derive the change in equilibrium compensation with the envelope theorem as a function of RC. To do this we first express $\$^*$ in Equation (3) as an explicit function of RC, $\$^*(\mathrm{RC})$:

$$\frac{\partial \mathrm{Rev}[\$^*(\mathrm{RC})]}{\partial\$} - 1 - \frac{\partial \mathrm{IC}[\$^*(\mathrm{RC}) - \mathrm{RC}]}{\partial(\$ - \mathrm{RC})} = 0 \tag{4}$$

and take its derivative with respect to RC, yielding:

$$\frac{\partial^2 \mathrm{Rev}[\$^*(\mathrm{RC})]}{\partial\$^2} \times \frac{\partial\$^*(\mathrm{RC})}{\partial \mathrm{RC}} - \frac{\partial^2 \mathrm{IC}[\$^*(\mathrm{RC}) - \mathrm{RC}]}{\partial(\$ - \mathrm{RC})^2} \times \left[\frac{\partial\$^*(\mathrm{RC})}{\partial \mathrm{RC}} - 1\right] = 0. \tag{5}$$

Rearranging terms and solving for $\partial\$^*(\mathrm{RC})/\partial \mathrm{RC}$ results in:

$$\frac{\partial\$^*(\mathrm{RC})}{\partial \mathrm{RC}} = \frac{-\left[\dfrac{\partial^2 \mathrm{IC}[\$^*(\mathrm{RC}) - \mathrm{RC}]}{\partial(\$ - \mathrm{RC})^2}\right]}{\dfrac{\partial^2 \mathrm{Rev}[\$^*(\mathrm{RC})]}{\partial\$^2} - \dfrac{\partial^2 \mathrm{IC}[\$^*(\mathrm{RC}) - \mathrm{RC}]}{\partial(\$ - \mathrm{RC})^2}} > 0. \tag{6}$$

This expression shows that equilibrium compensation is an increasing function of compensation's reasonableness, RC. The denominator of the right-hand side expression is the second derivative of the profit function and is negative if firms maximize their profits. The bracketed term in the numerator is the second derivative of the implicit cost of unreasonable compensation, and is positive by assumption. Thus, the overall expression is positive, consistent with compensation increasing as third party perceptions of reasonableness increase.[9]

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[9] This and the following results do not require firms' compensation amounts to be reasonable in equilibrium. Our theory would still apply even if firms pay more or less than a reasonable amount. The term $\$$—RC remains in the solution, so that any change in RC will have a similar effect whatever the level of $\$$. Because this is a nonlinear function, the magnitude of the effect depends on the level of the difference in $\$$ and RC at which such a change in RC takes place, but the positive sign of the effect is invariant.

To demonstrate that this change is increasing in the distance between \$1 million and a firm's prior reasonable compensation, we must link Equation (6) to the belief-revision function in Equation (2). To this end, we consider the approximate change in equilibrium compensation that would result from a change in third parties' perceptions of "reasonable" compensation:

$$\Delta\$^* \approx \frac{\partial \$^*(RC)}{\partial RC} \times \Delta RC. \tag{7}$$

If we define Distance = 1,000,000 − RC_{before}, then demonstrating that the change in compensation is increasing in Distance is consistent with showing that $\partial(\Delta RC)/\partial \text{Distance} > 0$. Stating ΔRC in terms of Equation (2) yields:

$$\begin{aligned}\Delta RC &= RC_{after} - RC_{before} = [\alpha RC_{before} + (1-\alpha)1{,}000{,}000 - RC_{before}] \\ &= (1-\alpha)(1{,}000{,}000 - RC_{before})\end{aligned} \tag{8}$$

and, thus:

$$\frac{\partial \Delta RC}{\partial \text{Distance}} = (1-\alpha) > 0. \tag{9}$$

Q.E.D.

The above analysis supports the following hypothesis, stated in the alternative:

> **H:** The further a firm's reasonable compensation falls below the \$1 million benchmark before the tax-law change, the greater the decrease in third-party-imposed implicit costs, and the greater the increase in CEO compensation.

IV. SAMPLE SELECTION

The sample consists of companies listed in the *Forbes* (1993) listing of 1992 Compensation for 800 CEOs, composed of CEOs of firms included in any *Forbes* 500 list based on their companies' assets, sales, earnings, and market value. Our theory suggests that public scrutiny of CEO compensation imposes costs on firms, and so these firms should be especially sensitive to implicit compensation costs.

Table 1 summarizes the sample selection. We eliminate: (1) firms whose CEOs are founders, because the structure of compensation contracting is different for a founder CEO (Kole 1997); (2) firms that are regulated utilities, because regulation of their industries leads to different contracting issues and lower CEO compensation (Smith and Watts 1992; Bizjak et al. 1993); (3) firms that changed CEOs in 1992, 1993, or 1994, because the change in CEO likely affects compensation contracting; (4) firms that have year-ends other than December or January, as the time available for such firms to change CEO compensation in response to the tax law change differs from other firms, because the legislation applies to years beginning on or after January 1, 1994; (5) firms that merged out of existence during the sample period or were not on Compustat; and (6) firms that are noncomparable or lack complete data for reasons such as bankruptcy, no address, restructuring, recent IPO, changed year-end, etc. (as detailed in Table 1). We eliminate 87 firms for lack of proxies, compensation, financial or other data, resulting in a final sample of 223 companies.

TABLE 1
Sample Selection for *Forbes* 800 Firms Not Predicted to be Directly Affected by Section 162(m)

Panel A: Selection of Firms

Total number of CEOs listed in *Forbes* (5/24/93, 1992 compensation)		800
Firms listed twice on *Forbes* list		(2)
		798
Less: CEO is firm founder	64	
Regulated utilities	76	
CEO change from 1992 to 1994	199	
Fiscal year-end not December or January	83	
Merged out of existence between 1/1/93 and 12/31/94	29	
Not on Compustat	9	
Other (operating in bankruptcy (4 firms), no address for firm (4 firms), multiple restructurings and ownership changes (4 firms), recent IPO or spin-off (3 firms), change year-end (3 firms), more than one CEO at a time (3 firms), government-related entities (3 firms), proxy not required or not filed (3 firms), went private (1 firm))	28	(488)
Sample before data restrictions		310
Insufficient back data available (1983–1992)	67	
Missing data items (unable to obtain proxies or other items)	20	(87)
Total		223

Panel B: Dividing Firms into Affected and Not Directly Affected by Section 162(m)

Firms paying (or expected to pay) cash compensation over $1 million in 1994 (tax firms)	168
Firms not predicted to be directly affected by Section 162(m) in 1994 (non-tax firms)	55
Total	223

We consider 55 of these companies as non-tax-affected because our subsequent analysis shows they were expected to pay less than $1 million in CEO cash compensation in 1994. We use this non-tax-affected sample to test our hypothesis because Section 162(m) should not directly affect these firms—thus, we need not control for alternative hypotheses concerning tax sharing or risk changes. Untabulated analyses reveal that for 1994, our sample's expected cash compensation ranges from about $279,000 to $977,000. This should include firms whose implicit costs are most affected by the tax-law change: those with compensation moderately less than $1 million. As explained previously, the $1 million limit is unlikely to be a relevant "reasonable compensation" benchmark for firms paying cash compensation far below or far above $1 million.

V. EMPIRICAL RESEARCH DESIGN

Expected CEO Cash Compensation Absent the 1994 Section 162(m) Tax-Law Change

The part of total compensation Congress targeted in Section 162(m) is cash compensation (salary and bonus). We begin by estimating firm-specific models of *expected CEO* *cash compensation* by regressing the cash compensation each firm actually paid its CEO on proxies for the objective determinants of CEO compensation, using data from 1983 to 1992.[10] We then use the parameter estimates from these regressions to estimate the firm's expected CEO compensation each year, 1991–1994, by multiplying the parameters of each firm's time-series compensation model by the values of that firm's determinants of compensation.[11] In years after the tax-law change, this is our estimate of what firms would have paid their CEOs had Congress not enacted the tax legislation.

Linking the Theoretical and Empirical Models

Our theoretical model predicts that the further a firm's reasonable compensation fell below $1 million, the larger the decline in its implicit compensation contracting costs upon the enactment of Section 162(m), and the greater the increase in CEO compensation. To formally link our model and its prediction to our empirical analysis, we reformulate our theoretical model of the change in reasonable compensation to reflect the specific effects of Congress's passing Section 162(m):

$$RC_{94} = \alpha \times RC_{before94} + (1 - \alpha) \times 1{,}000{,}000. \qquad (10)$$

We assume that firms paid actual compensation equal to reasonable compensation both before and after the tax-law change. (Although the anecdotal evidence discussed previously suggests that actual compensation [and its expectation] was greater than what third parties thought reasonable before enactment of Section 162(m), the Appendix shows that as long as actual compensation is proportional to reasonable compensation our analysis yields valid inferences.) We also assume that our estimate of expected compensation is a good proxy for pre-Section 162(m) reasonable compensation. Given these assumptions, we can use the following empirical regression model to evaluate the effect of the tax-law change (control variables omitted):

$$AC_t = b0 + b1 \times EC_{before94} + D_{94}b0_{94} + D_{94}b1_{94} \times EC_{before94} \qquad (11)$$

where AC is actual compensation, EC is expected compensation, and D_{94} is 1 in 1994 and 0 otherwise. The subscript, "before94," indicates that we project expected compensation based on parameters estimated with data from before 1994, multiplied by each year's variables' values. If our expectation model generates a good approximation of actual compensation, then $b0 = 0$, and $b1 = 1$, before the tax-law change. The coefficients $b0_{94}$ and $b1_{94}$ capture the differential effect on CEO compensation arising from the change in the tax law.

To see how the coefficients, $b0_{94}$ and $b1_{94}$, yield insight into the effects of the 1994 tax-law change on CEO compensation, we first substitute the right-hand side of Equation (10) for actual compensation *into a regression model for 1994 alone*:

$$\begin{aligned} AC_{94} &= c0 + c1 \times EC_{before94} \\ &\rightarrow \alpha \times EC_{before94} + (1 - \alpha) \times 1{,}000{,}000 = c0 + c1 \times EC_{before94}. \end{aligned} \qquad (12)$$

As α is a constant, this directly implies that:

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[10] We exclude 1993 from the time-series model. We consider 1993 a transition year, so projecting 1994 compensation based on data from 1993 may yield an estimate that includes the effect of the tax law change.

[11] Our expectation model yields approximately 40 percent greater adjusted R^2 than a naive model based on the prior year's cash compensation.

$$\begin{aligned} c0 &= (1 - \alpha) \times 1{,}000{,}000 \\ c1 &= \alpha. \end{aligned} \tag{13}$$

The empirical regression model in Equation (11) is stated in differences between the pre- (1991–1993) and post- (1994) tax-change years, and is estimated across several years, which better controls for measurement error. In contrast, Equation (12) is estimated cross-sectionally for 1994 only. We set the two regressions equal (AC is the left-hand side of both), and assume that $b0 = 0$ and $b1 = 1$ (i.e., estimated compensation is a good proxy for actual compensation). Solving for $b0_{94}$ and $b1_{94}$ yields:

$$\begin{aligned} b0_{94} &= c0 - b0 = (1 - \alpha) \times 1{,}000{,}000 - 0 \\ b1_{94} &= c1 - b1 = \alpha - 1. \end{aligned} \tag{14}$$

The above predicts $b1_{94}$, $\alpha - 1$, is negative if α lies between 0 and 1, as assumed in our theoretical model. Similarly, our model predicts that $b0_{94}$, $(1 - \alpha) \times 1{,}000{,}000$, is positive if α lies between 1 and 0. This is consistent with third parties placing less weight on prior reasonable compensation after the tax-law change and shifting that weight to the new $1 million benchmark.[12]

Thus, the total empirical effect of the tax-law change on CEO compensation equals the sum of the (positive) differential intercept, $(1 - \alpha) \times 1{,}000{,}000$, and the (negative) marginal effect, $(\alpha - 1) \times EC_{before94}$. Thus, the total effect of Section 162(m) increases as $EC_{before94}$ declines, per our hypothesis.

Our theory suggests an additional empirical result. If Congress's tax-law change had little or no effect on firms paying near $1 million, then the 1994 intercept, $b0_{94}$, should offset the marginal effect, $b1_{94} \times EC_{before94}$, at expected compensation of approximately $1 million (i.e., the total effect on such firms should be approximately zero): $b0_{94} = -(b1_{94} \times 1{,}000{,}000) \rightarrow b0_{94} + (b1_{94} \times 1{,}000{,}000) = 0$.[13]

Specification of Empirical Models

Following Healy et al. (1987), Clinch and Magliolo (1993), Balsam and Ryan (1996), Gaver and Gaver (1998), and Adut et al. (2002) we use a firm-specific time-series model to estimate expected compensation. Specifically, we compute the firm-specific relation between CEO cash compensation and executive-specific determinants of compensation that likely change across time for a given firm: accounting and market measures of firm performance, and the firm-specific sensitivity of CEO compensation to inflation:

$$\text{Cash Comp}_t = \sum_{j=1}^{J} \alpha_j D_{j,t} + \gamma_1 \text{Earnings}_t + \gamma_2 \Delta S/H\ \text{Wealth}_t + \gamma_3 \text{CPI}_t + \mu_t \tag{15}$$

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[12] To the extent that actual compensation exceeds reasonable compensation, the Appendix shows that the parameter estimates on expected compensation, $\hat{b1}$ and $\hat{b1}_{94}$, are likely too low. A constant-over-time downward bias in the coefficient on expected compensation does not directly affect the test of our hypothesis that the coefficient on expected compensation *changes* (i.e., falls) in 1994 and subsequent years after the tax change, relative to its level before the tax change.

[13] This computation assumes that actual compensation is well proxied by expected compensation and that expected compensation is close to reasonable compensation.

where:

Cash Comp_t = salary plus bonus the firm actually paid the CEO in year t (from proxy statements);
$D_{j,t}$ = 1 if individual j was CEO in year t and 0 otherwise (J is the number of CEOs during the sample period) (from proxy statements);
Earnings_t = accounting earnings before extraordinary items in year t (from Compustat);
$\Delta\ \text{S/H Wealth}_t$ = the change in shareholder wealth, measured as the change in the market value of the firm's common shares outstanding from year t−1 to year t, plus dividends paid (from Compustat); and
CPI_t = the consumer price index for year t (from Bureau of Labor Statistics).

We estimate Equation (15) separately for each firm, using data from 1983 to 1992. To ensure adequate power, we require at least 6 + J firm-observations where J equals the number of CEOs the firm employed during the estimation period. We allow the intercept to vary across CEOs to control for CEO-specific factors, such as age, ability, and education (Healy et al. 1987; Balsam and Ryan 1996). Consistent with prior work, the model includes earnings and the change in shareholder wealth to capture the firm-specific sensitivity of CEO cash compensation to accounting and market-oriented measures of firm performance. We include CPI to control for the firm-specific sensitivity of CEO compensation to inflation (Adut et al. 2002).

Table 2 provides information on the distribution of the firm-specific regression coefficients and adjusted R^2s. The expectation model explains more than 50 percent of the variation in cash compensation for 75 percent of our sample firms, and only two of our firms' adjusted R^2s are negative. Each year, 1991–1994, we multiply each firm's parameter estimates from Equation (15) by the realized values of the independent variables to estimate the CEO's expected cash compensation.

As in prior research, our firm-specific time-series compensation model in Equation (15) includes determinants of compensation that vary over time for a given firm. However, CEO compensation also varies across firms with factors that do not vary substantially across time for a given firm, such as CEO ownership, relation to the founding family, etc.

Thus, we incorporate the firm-specific estimates of expected cash compensation from Equation (15) in a cross-sectional model that adds controls for those other determinants of CEO compensation that are likely to vary more across firms than across time for a given firm:[14]

$$\begin{aligned}\text{Cash Comp}_{i,t} = {} & \beta_0 + \beta_1\text{Expected}_{i,t} + \beta_2\text{Growth}_{i,t} + \beta_3\text{Size}_{i,t} + \beta_4\text{Own }5\%_{i,t} \\ & + \beta_5\text{Own CEO}_{i,t} + \beta_6\text{Fndfam}_i + D_{94}[\beta_7\text{Expected}_{i,t} + \beta_8\text{Growth}_{i,t} \\ & + \beta_9\text{Size}_{i,t} + \beta_{10}\text{Own }5\%_{i,t} + \beta_{11}\text{Own CEO}_{i,t} + \beta_{12}\text{FndFam}_i] \\ & + \beta_{13}D_{94} + \varepsilon_i\end{aligned} \tag{16}$$

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[14] Including a variable in Equation (16), our second-stage model, assumes the coefficient is constant across the sample firms. Thus, the firm-specific model in Equation (15) includes those variables whose coefficients we expect to vary most across firms. Holding these coefficients constant in cross-sectional estimation would reduce the power of our tests. For example, CPI captures the firm-specific sensitivity of CEO compensation to inflation (Adut et al. 2002). Table 2 shows that the firm-specific coefficients on CPI vary widely across the sample firms, but, of course, there is no cross-sectional variation in CPI in a given year.

TABLE 2
Distribution of Coefficients and Adjusted R²s from 55 Firm-Specific Regressions of CEO Cash Compensation on CEO Indicator Variables, Firm Earnings, Firm Market Performance, and CPI Index, 1983–1992[a]

$$Cash\ Comp_t = \sum_{j=1}^{J} \alpha_j D_{j,t} + \gamma_1 Earnings_t + \gamma_2 \Delta S/H\ Wealth_t + \gamma_3 CPI_t + \mu_t$$

Variable	*Mean*	*Median*	*25th %*	*75th %*	*Min*	*Max*	*# > 0*
α_1, Intercept (for current CEO)	–87.79	–231.27	–737.48	108.27	–3433.86	11007.24	16
α_2, D1 (for 2nd CEO)	44.29	0.00	0.00	165.49	–2029.42	625.09	25
α_3, D2 (for 3rd CEO)	–2.16	0.00	0.00	0.00	–2701.60	903.82	8
γ_1, Earnings	2.05	0.99	0.05	3.22	–7.08	20.84	44
γ_2, Δ S/H Wealth	–0.00	0.03	–0.03	0.09	–2.78	0.85	30
γ_3, CPI	378.11	519.73	173.20	917.96	–7262.20	2960.83	44
Adjusted R²	0.69	0.80	0.52	0.92	–0.32	0.98	53

[a]

$Cash\ Comp_t$ = a firm's CEO's annual salary plus bonus, in thousands;
$D_{j,t}$ = 1 if individual j was CEO of the firm in year t and 0 otherwise, where J is the number of CEOs the firm employed during the period from 1983–1994;
$Earnings_t$ = accounting earnings before extraordinary items in year t, in millions;
$\Delta\ S/H\ Wealth_t$ = the change in firm market value from year t–1 to year t, plus dividends paid, in millions; and
CPI_t = the consumer price index for year t.

where Cash Comp is defined as before and:

$Expected_{i,t}$ = salary plus bonus we expect firm i to pay their CEO in year t, based on the firm-specific estimated parameters in Equation (15) and the values of firm i's variables in year t (to control for estimation errors, we winsorize the expected cash compensation so it is not lower than the previous year's salary or higher than two times the previous year's salary plus bonus);[15]
$Growth_{i,t}$ = the change in firm i's sales from year t–1 to t (from Compustat);[16]
$Size_{i,t}$ = the log of firm i's sales in year t (from Compustat);
$Own\ 5\%_i$ = the percentage of stock owned by blockholders holding at least 5 percent (from proxy statements);
$Own\ CEO_i$ = the percentage of stock owned by the CEO (from proxy statements);
$Fndfam_i$ = 1 if a firm's CEO is related to the founding family (from proxy statements); and
D_{94} = 1 in year 1994, else 0.

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[15] Winsorizing expected cash compensation affects 16 observations. Estimating the regressions without this adjustment does not change the inferences.

[16] Using market-to-book ratio as an alternative proxy for Growth does not affect our inferences.

If our model of expected compensation is appropriate, the coefficient on expected compensation, β_1, will approximately equal 1 in years before 1994. Our hypothesis predicts that the tax-law change reduced implicit contracting costs for firms paying their CEOs less than \$1 million, and that this reduction motivates firms to pay more cash compensation, the further their expected CEO compensation fell below Congress's new implicit contracting cost standard of \$1 million. Thus, we expect β_7 to be negative, indicating a significant reduction in the relation between expected compensation (absent the tax-law change) and actual cash compensation after the tax-law change.

The coefficients in Equation (16) link directly to our theory. β_7 corresponds to $b1_{94}$ from Equation (14) and β_{13} corresponds to $b0_{94}$. We predict that β_7 will be negative and that β_{13} will be positive. More specifically, we expect that Congress's action had little effect on firms paying near \$1 million, consistent with the estimated total effect on such firms, $\beta_{13} + (\beta_7 \times \$1{,}000{,}000)$, being approximately zero.

Equation (16) controls for several additional factors associated with cross-sectional differences in CEO compensation. To control for the possibility that growth affects CEO compensation, we include the annual change in sales.[17] CEO compensation increases with firm size and implicit contracting costs are likely higher for large firms that are more visible to investors, regulators, legislators, the public, and the press. Therefore, we control for firm size as proxied by the log of sales, even though our sample selection procedure reduces the variation in firm size.[18]

Core et al. (1999) and Cyert et al. (2002) conclude that large shareholders and the board of directors monitor the CEO to mitigate agency problems and to increase the efficiency of compensation contracting. To abstract from the effect of cross-sectional differences in corporate control effectiveness on CEO compensation, Equation (16) controls for: (1) the percentage of stock owned by 5 percent owners (Own 5%), (2) the percentage of stock owned by the CEO and any related parties (Own CEO), and (3) Fndfam, which indicates whether the CEO is related to the founder (if the CEO is related to the founder, this likely reflects a different contracting environment that may not be controlled by the stock ownership measure).[19] We cannot predict the direction in which these three corporate control variables are likely to affect firms' reactions to reductions in implicit compensation contracting costs.

VI. RESULTS

Table 3 presents descriptive statistics about the variables used in our primary analysis. Both cash compensation and expected compensation have values within a range we would expect for a sample of firms with cash compensation below \$1 million, but close enough to \$1 million to be affected by changes in implicit costs as a result of Section 162(m). Mean and median sales growth are positive but the measure takes on both negative and positive values. As expected, size, measured as the log of sales, shows minimal variation. Over 75 percent of the sample firms have at least one shareholder who owns more than 5 percent of the firm's stock, and on average, these 5 percent blockholders own 13 percent

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[17] We control for growth in the cross-section, and not in the time-series model, because its cross-sectional variance is approximately twice that of its time-series variance.

[18] The Pavlik et al. (1993) review of executive compensation literature from 1962 to 1993 concludes that many studies of compensation levels control for sales growth and firm size.

[19] Some founding families no longer own a large percentage of stock but continue to have substantial presence in the company. For the nine CEOs in our sample who are related to the founding family, the CEO ownership ranges from 3 percent to 72 percent.

TABLE 3

Sample Descriptive Statistics for CEO Cash Compensation, Expected Compensation, Growth, Firm Size, Stock Owned by 5 Percent Shareholders, Stocked Owned by CEO, and Founding Family[a]

Variable	*Mean*	*Median*	*25th %*	*75th %*
Cash Comp	690.37	681.35	500.00	806.15
Expected	645.28	632.45	498.68	771.22
Growth	45.41	38.22	−36.69	132.84
Size	7.26	7.30	6.56	7.91
Own 5%	0.13	0.09	0.05	0.22
Own CEO	0.05	0.00	0.00	0.02
Fndfam	0.16	0.00	0.00	0.00

[a] Number of firm-observations is 220, 55 firms for 4 years.

Cash Comp = a firm's CEO's annual salary plus bonus, in thousands;
Expected = expected CEO cash compensation based on firm-specific estimated parameters from Equation (15) and the values of a firm's variables in a given year, in thousands;
Growth = the change in a firm's sales, in millions;
Size = the log of a firm's sales;
Own 5% = the percentage of stock owned by 5 percent shareholders;
Own CEO = the percentage of stock owned by the CEO; and
Fndfam = 1 if firm's CEO is related to the founding family.

of the firm's shares. Most of these firms' CEOs own only a small fraction of the firm's shares (median is less than 0.5 percent). The mean CEO ownership of 5 percent is heavily influenced by a few CEOs who own a very large percentage of their firm—one CEO owns 72.24 percent of his firm's shares.

Table 4 presents correlations among these variables. The upper-right diagonal of Table 4 displays Spearman correlations, and the lower-left diagonal Pearson correlations. The two sets of correlations are generally similar. The high correlation between cash compensation and expected compensation ($\rho \geq 0.70$) suggests that our expectation model predicts cash compensation reasonably well. As expected, CEO cash compensation is significantly positively correlated with firm growth and size. In addition, as the CEO's ownership in the firm increases, the cash compensation decreases, consistent with stock ownership substituting for cash compensation, and also consistent with the CEO bearing a greater proportion of his/her own compensation's cost. The CEO's cash compensation is also lower if the CEO is a member of the founding family.

Table 5 displays the results of estimating Equation (16). Consistent with the descriptive evidence, $\hat{\beta}_1$, the coefficient on expected compensation, is significantly positive ($p < 0.001$).[20] The estimated coefficient $\hat{\beta}_1$ equals 0.88, which is insignificantly different from 1.0 ($p = 0.12$), consistent with our first-stage regression doing a good job estimating cash compensation in the period before the tax-law change in 1994.

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[20] The p-values are based on White's heteroskedasticity-consistent standard errors. We report two-tailed tests except in the case of a directional hypothesis, where we use one-tailed t-tests (β_7 and β_{13}).

TABLE 4
Correlations among CEO Cash Compensation, Expected Compensation, Growth, Firm Size, Stock Owned by 5 Percent Shareholders, Stocked Owned by CEO, and Founding Family (upper-right diagonal: Spearman; lower-left diagonal: Pearson)[a]

Variable	*Cash Comp*	*Expected*	*Growth*	*Size*	*Own 5%*	*Own CEO*	*Fndfam*
Cash Comp		0.77 (0.00)	0.21 (0.00)	0.18 (0.00)	0.03 (0.59)	–0.17 (0.00)	–0.37 (0.00)
Expected	0.70 (0.00)		0.12 (0.06)	0.14 (0.03)	–0.03 (0.57)	–0.09 (0.15)	–0.34 (0.00)
Growth	0.25 (0.00)	0.02 (0.69)		0.24 (0.00)	0.01 (0.80)	0.00 (0.95)	–0.01 (0.79)
Size	0.23 (0.00)	0.17 (0.01)	0.06 (0.34)		0.32 (0.00)	–0.29 (0.00)	–0.12 (0.05)
Own 5%	0.03 (0.63)	–0.04 (0.49)	–0.04 (0.49)	0.35 (0.00)		–0.27 (0.00)	0.01 (0.85)
Own CEO	–0.24 (0.00)	–0.21 (0.00)	0.00 (0.97)	–0.16 (0.01)	–0.28 (0.00)		0.57 (0.00)
Fndfam	–0.33 (0.00)	–0.33 (0.00)	–0.00 (0.95)	–0.12 (0.05)	–0.02 (0.74)	0.60 (0.00)	

[a] Number of firm-observations is 220, 55 firms for 4 years.

Cash Comp = a firm's CEO's annual salary plus bonus, in thousands;
Expected = expected CEO cash compensation based on firm-specific estimated parameters from Equation (15) and the values of a firm's variables in a given year, in thousands;
Growth = the change in a firm's sales, in millions;
Size = the log of a firm's sales;
Own 5% = the percentage of stock owned by 5 percent shareholders;
Own CEO = the percentage of stock owned by the CEO; and
Fndfam = 1 if firm's CEO is related to the founding family.

Consistent with our hypothesis, $\hat{\beta}_7$, the 1994 shift in the coefficient on expected compensation, is significantly negative ($p = 0.03$). This is consistent with our sample firms reducing the weight on pre-tax-change determinants of "reasonable" compensation (as proxied by expected compensation) after enactment of the new $1 million reasonable compensation standard in 1994. $\hat{\beta}_{13}$ is the shift in the CEO compensation function intercept in 1994—that is, the portion of the change in average CEO compensation for 1994 relative to prior years (1991–1993) that is unrelated to expected compensation or the control variables. $\hat{\beta}_{13}$ is significantly positive ($p = 0.06$).[21] This is consistent with our sample firms (that had previously paid their CEOs less than $1 million in cash compensation) placing weight on the new $1 million standard for "reasonable" compensation after the tax-law change.

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[21] If we add year dummies for 1992 and 1993 to the model, the coefficient on the 1994 dummy is 373.3 ($p = 0.05$). This specification controls for the possible effect of a time-trend in CEO compensation that may not be captured by the CPI variable.

TABLE 5
Regression of CEO's Cash Compensation on CEO's Expected Compensation and Control Variables, 1991–1994, with Interactions to Allow Different Slopes after the Tax-Law Change in 1994[a]

$$\begin{aligned} Cash\ Comp_{i,t} &= \beta_0 + \beta_1 Expected_{i,t} + \beta_2 Growth_{i,t} + \beta_3 Size_{i,t} + \beta_4 Own\ 5\%_{i,t} + \beta_5 Own\ CEO_{i,t} \\ &+ \beta_6 Fndfam_i + D_{94}\,[\beta_7 Expected_{i,t} + \beta_8 Growth_{i,t} + \beta_9 Size_{i,t} + \beta_{10} Own\ 5\%_{i,t} \\ &+ \beta_{11} Own\ CEO_{i,t} + \beta_{12} Fndfam_i] + \beta_{13} D_{94} + \varepsilon_i \end{aligned}$$

Variable	*Coefficient (White's p-values*)*
β_0 (Intercept)	–64.83 (0.59)
β_1 (Expected)	0.88 (0.00)
β_2 (Growth)	0.14 (0.31)
β_3 (Size)	21.56 (0.27)
β_4 (Own 5%)	59.16 (0.62)
β_5 (Own CEO)	–52.18 (0.59)
β_6 (Fndfam)	–27.32 (0.41)
β_7 (Expected × D_{94})	–0.29 (0.03)
β_8 (Growth × D_{94})	0.02 (0.86)
β_9 (Size × D_{94})	–9.03 (0.78)
β_{10} (Own 5% × D_{94})	11.94 (0.96)
β_{11} (Own CEO × D_{94})	–103.86 (0.48)
β_{12} (Fndfam × D_{94})	–123.01 (0.03)
β_{13} (D_{94})	351.12 (0.06)
Adjusted R^2	0.56

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[a] Number of firm-observations is 220, 55 firms for 4 years.

Cash Comp = a firm's CEO's annual salary plus bonus, in thousands;
Expected = expected CEO cash compensation based on firm-specific estimated parameters from Equation (15) and the values of a firm's variables in a given year, in thousands;
Growth = the change in a firm's sales, in millions;
Size = the log of a firm's sales;
Own 5% = the percentage of stock owned by 5 percent shareholders;
Own CEO = the percentage of stock owned by the CEO; and
Fndfam = 1 if firm's CEO is related to the founding family.

This is estimated for 1991–1994 on a sample of firms expected to pay less than $1 million to their CEO in 1994, based on firm-specific regressions reported in Table 2.

* The p-values are based on two-tailed tests, except in cases of a directional prediction, where we use a one-tailed t-test (β_7 and β_{13}).

Together these results suggest that firms that expected to pay their CEOs less than \$1 million increased their compensation after the tax-law change (even after controlling for other potentially confounding factors, such as inflation). For all sample firms, the magnitude of the positive 1994 differential intercept, $\hat{\beta}_{13}$, is greater than the magnitude of the negative marginal effect of the 1994 shift in the coefficient on expected compensation, $\hat{\beta}_7$, multiplied by expected compensation. $\hat{\beta}_{13}$ is \$351,120, $\hat{\beta}_7$ is -0.29, and our firms' 1994 expected CEO compensation ranges from approximately \$280,000 to \$980,000. Thus, we estimate that all firms' cash compensation increased unexpectedly in 1994 compared to prior years because \$351,120 − 0.29 × (expected compensation) is greater than 0 for all firms. Furthermore, the significantly negative $\hat{\beta}_7$ means that after the tax-law change in 1994, and in contrast to prior years, the further a firm's expected compensation was below \$1 million the greater the total 1994 unexpected CEO compensation.

For example, the 75th percentile of 1994 expected compensation absent the tax law change (i.e., for those firms nearest the \$1 million benchmark) is \$838,352. Thus, the estimated unexpected increase in 1994 CEO cash compensation is 13 percent ((\$351,120 − (0.29 × \$838,352))/\$838,352) of expected compensation. The 50th percentile of 1994 expected compensation is \$665,903. Thus, the unexpected increase climbs to 24 percent ((\$351,120 − (0.29 × \$665,903))/\$665,903). At the 25th percentile (i.e., those firms farthest from the \$1 million benchmark), expected 1994 cash compensation equals \$520,363, so the unexpected increase in 1994 CEO cash compensation is 38 percent ((\$351,120 − (0.29 × \$520,363))/\$520,363) of expected compensation. These examples show how the unexpected increase in 1994 cash compensation is higher for CEOs whose expected compensation absent the tax-law change fell further below the \$1 million benchmark. Conversely, the overall unexpected increase in 1994 CEO cash compensation approaches zero for firms that expected to pay their CEOs near \$1 million, consistent with our theoretical model's prediction ((\$351,120 − (0.29 × \$1,000,000))/\$1,000,000 = 6%).

The control variables are generally insignificant, except that the CEO's compensation increases less in 1994 if the CEO is related to the founding family. Finding that the control variables are generally insignificant although the adjusted R^2 is 56 percent, suggests that the first-stage regression, Equation (15), generates a reasonably accurate estimate of cash compensation.

Robustness Checks

If a firm changed its compensation structure as a result of the tax law effective in 1994, then the change should persist and not quickly reverse. Just as the hypothesized compensation effect should not occur in years before the tax change, it should continue after the tax-law change in 1994. To test this conjecture we repeat our analyses substituting 1995 data for 1994 data. (Untabulated) results reveal that similar to 1994, the shift in the coefficient on expected compensation in 1995 is negative and significant ($p = 0.001$). The 1995 shift in intercept relative to 1991–1993's is \$322,000. This shift is similar in magnitude to the \$351,000 shift in 1994, but is not significant at conventional levels ($p = 0.39$). Due to missing data, this analysis is based on 46 of the original 55 firms.

Our sample includes firms with expected compensation below Congress's \$1 million benchmark. However, 16 of the sample firms acted as if they expected to be affected by this tax-law change and they either qualified their bonus plans or indicated that they would defer CEO cash compensation if necessary to avoid the tax. We re-estimate our regression allowing separate coefficients for the 16 firms taking such actions, and the (untabulated) results on the relation between cash compensation and expected compensation remain unchanged or become more significant. These results suggest that our inferences are not an artifact of misidentifying firms affected by the tax-law change.

Re-estimating Equation (16) for 1994 alone yields similar results in that the coefficient on expected compensation is significantly less than 1 ($p = 0.005$).[22] This corresponds with our results reported in Table 5 where the 1994 expected compensation coefficient is less than in prior years (during which the coefficient on expected compensation was not significantly different from 1). This suggests that our inferences are not attributable to the pre-tax-law-change period. We also conduct several additional robustness analyses using the 1994-only model, because we have additional data for that year, which are not available for the entire 1991–1994 period. First, we investigate whether the post-tax-law-change increase in cash compensation reflects a substitution from currently nondeductible (but less politically visible) noncash compensation into more visible and currently deductible cash compensation (this effect also would be consistent with a reduction in implicit contracting costs). Specifically, we re-estimate our 1994-only model including the change in noncash compensation (stock options, restricted stock awards, and performance share awards granted in the current period). However, we find no significant relation between expected compensation and noncash compensation. Although cash compensation increases from 1992 to 1994, we find no such increase for noncash compensation. These findings are consistent with the unexpectedly high 1994 compensation resulting from increases in CEOs' cash compensation, and not with a substitution from noncash to cash compensation.

Also in our 1994-only sample, we continue to find similar inferences after including in the analysis tax-affected firms (firms expected to pay more than $1 million). However, similar results in this case are not surprising because prior work has shown that tax-affected firms, on average, reacted to the tax-law change by reducing compensation (Balsam and Ryan 1996; Johnson, Nabar and Porter 1999; Johnson, Porter, and Shackell 1999).

VII. CONCLUDING COMMENTS

We investigate the unintended consequences resulting from congressional enactment of Section 162(m), which mandated a highly publicized $1 million benchmark for how much is too much executive compensation. We argue that this benchmark effectively reduced the implicit executive compensation contracting cost for firms paying their CEOs less than this $1 million benchmark. We expect this reduction in implicit costs, in turn, to lead these firms to increase CEO compensation more than they would have absent the tax law change. Our theoretical model suggests that firms closest to the $1 million limit were least affected by the benchmark and we predict that they paid their CEOs the smallest amount of unexpected compensation. Our empirical results are consistent with this hypothesis.

Specifically, after controlling for other determinants of CEO compensation, we find that firms who expected to pay their CEOs less than $1 million had unexpected increases in their CEOs' cash compensation, contrary to Congress's expectations. Moreover, the magnitude of the unexpected increase in compensation is proportional to how far the CEO's expected compensation fell below Congress's new $1 million reasonable-compensation standard. These changes occur relative to the period before Section 162(m)'s enactment; they arise after the tax law instituted the $1 million benchmark in 1994, and these changes in CEO compensation persist into 1995, further supporting the conclusion that this effect is due to the tax-law change.

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[22] To investigate possible bias from measurement error in our "expected" estimate in this 1994-only analysis, we regress each firm's difference between actual and expected compensation, unexpected compensation, on expected compensation quintiles and the same control variables. This estimates the mean unexpected compensation for differing levels of expected compensation. Our theory predicts that greater expected compensation in 1994 (closer to $1 million) leads to lower actual compensation. Consistent with our hypothesis, the mean unexpected compensation is decreasing in expected compensation.

To maximize the power of our tests, we examine firms with compensation practices particularly open to public scrutiny, a subset of the *Forbes* top 800 CEOs. Congress's \$1 million limit may not have materially affected firms paying their CEOs very large or very small amounts of cash compensation. Thus, we also restrict our sample to firms whose implicit costs were most likely reduced by the tax-law change—those with compensation just under \$1 million. Because our sample firms have greater-than-usual incentives to react to the tax-law change, the magnitudes of our estimated coefficients likely represent an upper-bound on the effect of this reduction in implicit executive contracting costs for firms paying less than the \$1 million benchmark, and may not generalize to the overall population of firms.

The evidence we present contributes to our understanding of how the legislative environment can change implicit contracting costs, and also supports the theory that firms react rationally to changes in the implicit costs indirect stakeholders impose on them. Moreover, our study provides evidence that specific tax and compensation legislation had unintended consequences that were the very opposite of Congress's stated intentions.

APPENDIX

Effect of Proportional Measurement Error in Compensation

It is likely that "reasonable" and actual compensation (and its expectation) vary proportionally. This follows if the publicly observable objective determinants of "reasonable" compensation, such as firm size, profitability, market returns, and market share also influence actual compensation levels. Much prior research finds this to be the case—the analyses in such work are, in the main, based on such publicly available factors (Pavlik et al. 1993). Thus, our assumption that actual and "reasonable" compensation are proportional is plausible.

Although it is impossible to estimate the amount by which actual compensation exceeds third parties' perceptions of "reasonable" compensation, any such inflation factor does not affect either the direction or the estimated significance of relations estimated with OLS. Let A be an inflation factor such that $x^* = Ax$ where x is the true variable and x^* is the observed, inflated value. In an OLS regression the estimated slope parameter, b^*, from a regression with inflated x^* is related to the slope coefficient from a regression of true x's, b, as follows: $b^*x^* = bx \Rightarrow b^*Ax = bx \Rightarrow b^* = b/A$. This demonstrates the inverse bias in the estimated coefficients. Thus, while our coefficient estimates on expected compensation are probably low (as the inflation factor is likely greater than 1, given perceptions that compensation was, on average, excessive) this measurement problem does not bias our results in favor of our hypothesis.

Moreover, this inflation factor does not affect the statistical significance of our coefficients. The t-statistic for the estimated coefficient of b is $b/[SE/\sqrt{X'X}]$, where SE is the estimated standard error of the regression and X is the vector of independent variables. The t-statistic for b^* is $b^*/[SE/\sqrt{X^{*\prime}X^*}]$. Substituting in b/A for b^* and Ax for x^* yields:

$$\frac{\left[\frac{b}{A}\right]}{\left[\frac{SE}{\sqrt{(AX)'(AX)}}\right]} = \frac{\left[\frac{b}{A}\right]}{\left[\frac{SE}{A\sqrt{X'X}}\right]} = \frac{b}{\left[\frac{SE}{\sqrt{X'X}}\right]}.$$

Thus, the t-statistic estimated with b^* is unbiased.

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BOOK REVIEWS

Jane L. Reimers, Editor

Editor's Note: The two books reviewed in this issue are, at first glance, from two very different domains. After reading these reviews, I hope you see a relationship. The first book review deals with a very timely issue in accounting—earnings management. Thanks to Don Pagach for this excellent review. The second book review relates to individual decision making and should interest behavioral researchers in accounting. Thanks to Steve Butler for this outstanding review.

MULFORD, CHARLES W., and EUGENE E. COMINSKY, *The Financial Numbers Game: Detecting Creative Accounting Practices* (New York, NY: John Wiley & Sons, Inc., 2002, 395 pp.)

In their new book, *The Financial Numbers Game: Detecting Creative Accounting Practices*, Charles Mulford and Eugene Cominsky have provided interested readers a great reference tool for examining the issues surrounding earnings management. The authors examine the issue by defining earnings management, exploring the incentives for earnings management and then presenting illustrations and examples of how businesses manage earning. Within each chapter the authors examine the flexibility of GAAP and how some companies use this flexibility to their own ends.

While the entire book is very enjoyable, there are two features that the authors provided that I found to be very helpful. First, each chapter has a comprehensive glossary that will provide useful background for a reader who has no accounting experience. The second useful feature is the checklists provided in the later chapters to help readers apply the lessons of the chapter and detect instances of earnings management. The checklists provide a way to take the issues discussed in the chapter and apply them to a specific company.

The authors start by describing the issues surrounding earnings management in two introductory chapters. The first chapter introduces the idea of earnings management and describes why firms have an interest and an incentive to manage earnings. The second chapter provides a thorough examination of accounting policy choices and how these choices can be used or misused. In each case, the authors present numerous examples to reinforce the topics.

In the third chapter, the authors expand significantly on the introduction and examine the conditions and incentives for earnings management in greater depth by focusing on examples of abusive earnings management. SEC Enforcement Releases are used to provide illustrative cases of abusive earnings management and to help distinguish between aggressive and abusive earnings management. The authors also start a discussion of the notion of *pro forma* earnings in Chapter 3 that continues throughout the book.

The fourth chapter examines the SEC's response to earnings management that began with Chairman Levitt's 1998 speech at the NYU Center for Law and Business. The chapter starts with an examination of Chairman Levitt's plan to increase public confidence in financial reporting and moves on to analyze the SEC's response through issuance of Staff Accounting Bulletins and Enforcement Actions. The chapter continues with an interesting look at securities laws and how the SEC enforces the laws along with a multitude of examples of enforcement application. The chapter ends with a look at the penalties and consequences associated with violating SEC rules.

The fifth chapter provides the results of the authors' survey of financial professionals on the issue of earnings management. The first segment of the survey asked professionals if certain financial techniques are consistent or inconsistent with GAAP. The second portion asked professionals for their opinions about whether some commonly held beliefs about earnings management were consistent with their real-world experiences. The third section asked for the professionals to comment about earnings management they had observed personally, how it could have been detected, and what effect it has on investors.

The first five chapters would be an excellent addition to any accounting course. The authors explain earnings management and the complexities of the issue in an uncomplicated manner and use examples to bring home important points.

In Chapters 6 through 8, the authors provide an in-depth analysis of three common earnings management techniques: premature or fictitious revenue recognition, aggressive capitalization and extended amortization policies, and the misstatement of assets and liabilities. Each chapter begins with an examination of the accounting issue and provides examples of companies that have applied the accounting treatment in an aggressive manner. For example, the chapter on revenue recognition discusses the issue of identifying fictitious or premature revenue and then presents cases of different policies for recognition. The authors then go further in depth by examining the SEC's Staff Accounting Bulletins on revenue recognition and related SEC enforcement actions. The chapter ends with a discussion of how to detect premature or fictitious revenue and presentation of a checklist that readers can use to apply the concept to a specific company.

The book continues with two informative chapters examining disclosure issues associated with the income statement. The authors start with an instructive discussion of the components of the income statement and comprehensive income. Next is an interesting look at how a number of companies have creatively disclosed income statement components in order to change users' perceptions of the companies' operating strength. The tenth chapter focuses specifically on the use of *pro forma* measures of earnings and the confusion that the increasing use of these measures has introduced to the financial statements. It also illustrates how a lack of reporting standards can result in a simple issue, such as the definition of EBITDA, creating a lot of confusion. The examples in these two chapters alone will provide enough discussion points for an entire semester.

The authors finish with an examination of the cash flow statement. The initial explanation of how cash flow is reported, along with a short discussion of the indirect method of preparation, is another example of the authors' ability to explain difficult accounting subjects in a reasonable and parsimonious manner. The chapter continues with some of the accounting details that can make it difficult for novice users to understand accounting financial statements, such as how the tax benefits of stock options are treated and reported. The chapter ends with another useful checklist, in which readers can use operating cash flows to detect if their favorite firm is getting too creative with accounting practices.

The Financial Numbers Game was a very enjoyable and informative book. The authors cover all of the major issues concerning earnings management and back up each topic with a multitude of examples. The book would be appropriate for many levels of readers, from novice investors to audit committee members. This quality would make *The Financial Numbers Game* an excellent supplement to any financial or auditing course. In summary, I commend the authors for providing an excellent examination of an important current accounting issue.

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WEBER, ELKE U., JONATHAN BARON, and GRAHAM LOOMES, *Conflict and Trade-offs in Decision Making* (Cambridge, U.K.: Cambridge University Press, 2001, 347 pp.)

This book is part of a series of books on judgment and decision making. *Conflict and Tradeoffs* presents 13 chapters by various researchers on the general question: Why are some decisions easier to make than others? The answer is not as simple as the fact that some decisions have an alternative that dominates the other(s) on all relevant attributes. It is much more subtle. In experiments on choice behavior, most people appear to be decision seeking when making decisions for themselves and decision averse (i.e., prefer to have someone else makes the decision) when making decisions for others.

Trade-offs are also more difficult when the dimensions are different. The book gives a perfect example related to the appropriate penalty for causing harm. If a logging company mistakenly cuts down 100 acres of protected forest, then people find it easier to assess a penalty of an equal amount and type of timber to be given to the government than they do a larger amount of timber of a different type or even monetary damages. With some creativity in experimental design and operation, this result has direct implications for auditor and client liability to allegedly wronged stakeholders.

The chapters encompass a variety of situations in which trade-offs are necessary. As you might expect, not all are of direct interest to business or accounting researchers. Two specific examples are the chapters on impulse buying and decisions about prenatal screening. Each is interesting from a judgment and decision-making perspective, but both would be leisure reading for most of us. The chapters that would more easily translate into the realm of behavioral accounting research are ones on the emotional trade-off difficulty on decision behavior, behavioral consequences of experienced regret, and judgments of relative importance and contingent decision behavior.

Finally, there are three chapters that deal with broader issues that may translate easily into our area of academic interest. The first of these chapters, on private values and public policy, addresses the question of what monetary value should be attached to any health and safety improvement in order to weigh it appropriately against the opportunity costs of bringing it about. As the chapter documents, it is not a simple question to answer. It strikes me as similar to the question: How much are you willing to pay an auditor to reduce the probability of an audit failure and what are the opportunity costs of such a reduction? The second chapter titled, "Decisions with Multiple Stakeholders and Conflicting Objectives," describes and demonstrates a process for framing and analyzing decisions of this type. Virtually every pronouncement of accounting rule-making bodies falls into this situation. The framework and example could lead to some interesting research questions concerning the rule-making process. Finally, there is a chapter that illustrates how to use a web site to provide health care consumers with a decision aid. Managerial accountants will find this chapter especially interesting.

Conflict and Tradeoffs is not meant to be a textbook for a behavioral accounting seminar course. Its purpose is to bring together in one book a number of experts in a particular area of study. Along with its predecessors in the series, this book will attract readers from many disciplines. The audience for this book includes academics, graduate students, advanced undergraduates (probably not accounting students), and practicing professionals in an area such as decision analysis. It is an interesting book, which I am still rereading.

STEPHEN A. BUTLER
University of Oklahoma

Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended May 31, 2002. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s)*:

	Number of Manuscripts	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	86	86	28.01
31 ≤ Days ≤ 60	108	194	63.19
61 ≤ Days ≤ 90	84	278	90.55
91 ≤ Days ≤ 120	23	301	98.05
121 ≤ Days	6	6	100.00

The mean review time was 50 days; the median review time was 50 days.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal....No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow (with a slight modification) the B-format of *The Chicago Manual of Style* (14th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

FORMAT

1. All manuscripts should be typed in 12-point font on one side of 8½ × 11" good quality paper and be double spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, the author's name, title and affiliation, email address, any acknowledgments, and a footnote indicating whether the author would be willing to share the data (see last paragraph in this statement).

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years.* All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form.* See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

ABSTRACT/INTRODUCTION

An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings.

Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
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Citations: Work cited should use the "author-date system" keyed to a list of works in the reference list (see below). Authors should make an effort to include the relevant page numbers in the cited works.

1. In the text, works are cited as follows: authors' last name and date, without comma, in parentheses: for example, (Jones 1987); with two authors: (Jones and Freeman 1973); with more than two: (Jones et al. 1985); with more than one source cited together (Jones 1987; Freeman 1986); with two or more works by one author: (Jones 1985, 1987).
2. Unless confusion would result, do not use "p." or "pp." before page numbers: for example, (Jones 1987, 115).
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6. Inclusive page numbers are treated as recommended in *Chicago Manual* section 8.67.

Sample entries are as follows:

American Accounting Association, Committee on Concepts and Standards for External Financial Reports. 1977. *Statement on Accounting Theory and Theory Acceptance*. Sarasota, FL: AAA.

Demski, J. S., and D. E. M. Sappington. 1989. Hierarchical structure and responsibility accounting. *Journal of Accounting Research* 27 (Spring): 40–58.

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2. In the case of manuscripts reporting on field surveys or experiments, four copies of the instrument (questionnaire, case, interview plan or the like) should be submitted. Information that might identify the author(s) must be deleted from the instrument.
3. Four copies should be submitted, together with a check made payable to the American Accounting Association in U.S. funds for $75.00 for members or $100.00 for nonmembers of the AAA. Submission may be made electronically by attaching a Microsoft® Word file to an email sent to taruw@u.washington.edu.
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OCTOBER 2002 PLACEMENT ADS

*The deadline for information to be included in the "Placement Information" section is two months prior to the date of publication. The **Review** is published on the first day of January, April, July, and October. Placement advertising and replies should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, ATTENTION: ADVERTISING DEPT. All replies to box numbers should be on separate sheets to facilitate forwarding.*

401 CLAREMONT GRADUATE UNIVERSITY, Peter F. Drucker Graduate School of Management, invites nominations and applications for a tenure-track position in Financial Accounting. Appointment at the rank of Associate or Full Professor is likely, but Senior Assistant Professors with the requisite experience may be considered. The position includes responsibility for research and teaching at the M.B.A. and the Executive Management levels. Candidates must have a doctoral degree; a strong record of scholarship; an interest in diverse approaches, students and populations; and an ability to make their intellectual contributions accessible to experienced managers through teaching and published work. Executive-level management or consulting experience is desirable. The position will remain open until filled. Nominations and applications will be reviewed on an ongoing basis. Please direct nominations, applications, and inquiries to the Office of the Dean, Peter F. Drucker Graduate School of Management, 1021 North Dartmouth Avenue, Claremont, CA 91711. Please visit our web site at http://www.cgu.edu/fac/emp/ for more information. CGU is committed to hiring diverse, accomplished, and energetic new and established scholars.

402 BROCK UNIVERSITY invites applications for two tenure-track positions at the Assistant or Associate Professor level, starting July 1, 2003, and subject to final budgetary approval. Qualifications include a doctorate in Accounting (completed or near completion) and a commitment to classroom excellence and to quality research. Candidates in all specialty areas are encouraged to apply. Salary is commensurate with the applicants' qualifications. Brock University is located in St. Catharines, Ontario in the heart of the scenic Niagara Peninsula, approximately one hour's drive from the city of Toronto and 40 minutes from Buffalo, New York. The Department of Accounting and Finance offers a Master's and Bachelor's of Accounting and is introducing an M.B.A. in 2003. The Faculty of Business is in the process of AACSB accreditation. Send applications to Dr. Sandra Felton, Chair, Department of Accounting and Finance, Brock University, St. Catharines, Ontario L2S 3A1 CANADA; Phone: (905) 688-5550 ext. 3452; Email: sfelton@taro.bus.brocku.ca.

403 ROCHESTER INSTITUTE OF TECHNOLOGY invites applications for Assistant, Associate, or Full Professor of Accounting starting September 2003 for research and graduate and undergraduate teaching in the areas of Accounting Information Systems and one other area (preferably Auditing) to meet program needs. Candidates must hold a Ph.D. in Accounting or a related discipline from an AACSB-accredited institution. Will consider ABD with expected completion by January 2004. Associate and Full Professor positions require established record of teaching and research. Previous professional experience desirable. Send letters of interest, *curriculum vitae,* and names and addresses of three references to Professor Ashok Robin, Chair, Accounting Faculty Search Committee, Rochester Institute of Technology, College of Business, Rochester, NY 14623-5608; Email: ajrbbu@rit.edu. Review of applications begins immediately. The position will remain open until filled. RIT is an AA/EEO institution.

404 UNIVERSITY OF NOTRE DAME, Department of Accountancy, invites applications for position openings at the Assistant and Associate Professor levels. All research areas will be considered. Applicants for Assistant Professor positions should expect to complete doctoral degree requirements before the start of the contract period. All candidates should have a strong commitment to scholarly research and excellence in teaching. Salary is very competitive and research support is excellent. Please send *curriculum vitae* and an example of scholarly work to Professor Tom Schaefer, Chair, Department of Accountancy, Room 102D, Mendoza College of Business Administration, University of Notre Dame, Notre Dame, IN 46556-5646. The University of Notre Dame is an Equal Opportunity/Affirmative Action Employer.

405 Ph.D., CPA, with 15 years of teaching experience and over five years of IRS audit experience desires tenure-track or visiting position for Fall 2002. I have recent publications in *JATA*, *Advances in Taxation*, and *The Tax Advisor*, and several works in process. While my primary teaching and research interest is in Taxation, I have taught Principles, Intermediate, Advanced, Auditing, Managerial, and Cost Accounting at both the graduate and undergraduate levels. I also have extensive experience teaching nontraditional students as well as those who speak English as a second language, and seven years advising VITA program volunteers. Contact pjwestort@adelphia.net.

406 LOYOLA COLLEGE IN MARYLAND invites applications for a tenure-track position at the Assistant or Associate level in Financial Accounting, commencing in the Spring or Fall 2003. Candidates for the position must possess an appropriate earned doctorate or be near completion with a commitment to outstanding teaching, quality research, and dedicated service. The primary teaching areas are Financial and Managerial Accounting. Appropriate business and teaching experience are highly valued. Loyola College is located five miles north of Baltimore's Inner Harbor. The Sellinger School and the Department of Accounting are AACSB-accredited. Competitive salary, attractive benefits, and many technology and financial supports are available. For more information visit our web site at http://www.loyola.edu. Submit your application to Dr. Alfred Michenzi, Department of Accounting, Loyola College in Maryland, 4501 North Charles Street, Baltimore, MD 21210-2699; Email: amichenzi@loyola.edu. Loyola College is a Jesuit Catholic institution that welcomes applicants from all backgrounds who can contribute to our unique educational mission. Loyola is an Equal Opportunity Employer that seeks applications from women and members of minority groups.

407 INDIANA UNIVERSITY, Kelley School of Business Indianapolis, invites applications for a tenure-track or clinical Accounting faculty position beginning August 2003. Rank/salary will be commensurate with qualifications; a significant research record is required for senior appointment. Although the School's primary need is in Cost/Managerial Accounting, qualified applicants with teaching and research interests in other areas will also be considered. Minimum qualifications include accredited Accounting Ph.D. or equivalent. Professional experience and certification are desirable. Send application letter, *curriculum vitae*, and names of three references by mail or email to Eric N. Johnson, Accounting Search Committee Chair, Kelley School of Business Indianapolis, 801 West Michigan Street, Indianapolis, IN 46202-5151; Phone: (317) 274-5695; Email: erijohns@iupui.edu. Review of applications will begin October 1, 2002 and continue until position is filled. Preliminary interviews will be held at the 2002 American Accounting Association Annual Meeting in San Antonio. See http://kelley.iupui.edu for more information. Indiana University is an AA/EOE Employer, Educator, and Contractor, M/F/D.

408 UNIVERSITY OF COLORADO AT DENVER seeks applicants for one or more tenure-track faculty positions in Accounting. Minimum qualifications include a doctoral degree in Accounting and commitment to excellence in research and teaching. Candidates for advanced rank must demonstrate significant accomplishments in research, teaching, and service. Specializations in all areas of Accounting will be considered. Proficiency with information technology in teaching and/ or research applications is especially desirable. Review of applications begins immediately, and will continue until the positions are filled. Please send a letter of application, current *curriculum vitae,* relevant publications or working papers, and teaching credentials to Professor Bruce R. Neumann, Director of Accounting Programs, University of Colorado at Denver, Campus Box 165, PO Box 173364, Denver, CO 80217; Fax: (303) 556-5899; Email: bruce.neumann@cudenver.edu. The University of Colorado at Denver is an Equal Opportunity Employer and Educator committed to excellence through inclusiveness. The Colorado Open Records Act applies to all application materials.

409 UNIVERSITY OF NEW HAMPSHIRE, Whittemore School of Business and Economics, invites applications for two tenure-track positions in Accounting at the Assistant Professor level beginning Fall 2003. Applicants must have a doctoral degree or be near completion. Teaching interests in Financial and Managerial Accounting (undergraduate and graduate) are preferred. The successful candidate will be expected to maintain an active research and publication agenda in peer-reviewed journals. Submit a letter of interest, current *curriculum vitae,* three recommendation letters, recent teaching evaluations, and evidence of scholarly work to Professor Afshad Irani, Whittemore School of Business and Economics, Box A, McConnell Hall, University of New Hampshire, Durham, NH 03824; Telephone: (603) 862-3342. For additional information, send email to Accounting.search@unh.edu. The school is accredited by the AACSB. Application review to begin immediately and continue until the position is filled. UNH is committed to excellence through diversity among its faculty and strongly encourages women and minorities to apply.

410 MICHIGAN TECHNOLOGICAL UNIVERSITY, School of Business and Economics, invites applicants for a full-time tenure-track position in Accounting beginning Fall 2003. The School of Business and Economics is fully AACSB-accredited. A Ph.D. (or ABD near completion) in Accounting from an AACSB-accredited school is required. Applicants in all areas of Accounting are encouraged to apply. Rank and salary will be commensurate with qualifications and experience. Professional certification and experience are a plus. The school's primary focus is on teaching, but research and publication in refereed publications is necessary for tenure and promotion. Michigan Tech was founded in 1885 and is a nationally recognized technological university. The university is located on the west side of Michigan's beautiful Upper Peninsula, where publicly accessible land and outdoor recreational opportunities abound. More information may be found at: http://www.sbea.mtu.edu. Interested candidates should forward a letter of application, a *curriculum vitae,* and a list of three references to Dr. Keith Lantz, Dean, School of Business and Economics, Michigan Technological University, 1400 Townsend Drive, Houghton, Michigan, 49931.

411 UNIVERSITY OF DAYTON seeks Assistant/Associate Professor for August 2003. Appropriate scholarship and teaching record/potential are required; 3:2 load after period of reduced loads/ grants. Ph.D. in Accounting from AACSB-accredited program is required; ABD close will be considered. Collegiality essential; professional certification desirable. Priority areas are Tax, Managerial, and/or AIS; AACSB-accredited program. Send letter and *curriculum vitae* to Dr. Ron Burrows, Department of Accounting, University of Dayton, 300 College Park, Dayton, OH 45469-2242; Phone: (937) 229-2429; Fax: (937) 229-2270; Email: burrows@notes.udayton.edu. Application deadline is November 1, 2002 or until filled. Dayton area population is approximately 750,000; Cincinnati, Columbus, and Indianapolis are within 1–2 hours. The University of Dayton, a comprehensive Catholic University founded by the Society of Mary in 1850, is an Equal Opportunity/Affirmative Action Employer. Women, minorities, individuals with disabilities, and veterans are strongly encouraged to apply. The University of Dayton is firmly committed to the principle of diversity.

412 BOSTON COLLEGE, Wallace E. Carroll School of Management, invites applications for a tenure-track Accounting position for Fall 2003. The qualified candidate must demonstrate interest in and capacity to conduct high-quality scholarly research as well as a high level of teaching competence and innovation. We seek individuals with an openness to diverse intellectual outlooks and research methodologies, and an appreciation for Boston College's liberal arts tradition. We offer a collegial environment and an energetic faculty with a variety of intellectual interests and close ties to the Boston financial and academic communities. Direct inquiries to Gregory Trompeter, Chair, Department of Accounting, Carroll School of Management, Fulton Hall 520, Boston College, Chestnut Hill, MA 02467. Deadline for applications is January 20, 2003. The Carroll School of Management is an Equal Opportunity Employer and strongly encourages applications from individuals who will enhance the diversity of our faculty.

413 SYRACUSE UNIVERSITY, Department of Accounting, invites applications for faculty positions at all ranks beginning August 15, 2003. A Ph.D. is required and professional certification and teaching experience are desirable. Candidates for senior positions must have demonstrated excellence in research and teaching. Syracuse University offers an excellent environment for faculty, competitive salary, and benefits, as well as good research support and colleagues. The School of Management offers Ph.D., M.B.A., M.S. in Accounting, and B.B.A. degree programs. The School has 1,250 undergraduate, 250 master's, and 30 doctoral students enrolled and 60 full-time faculty. It is currently in the design phase of a new, state-of-the-art building. The Department of Accounting receives strong support from its alumni, including an endowment to support research and course development. Please send applications to Gerald J. Lobo, Chairman, Department of Accounting, School of Management, Syracuse University, 900 S. Crouse Avenue, Syracuse, NY 13244. Syracuse University is an EO/AA Employer. Applications by minority and female candidates are especially encouraged.

414 CONCORDIA UNIVERSITY, located in beautiful Montreal, Quebec, Canada, invites applications for tenure-track positions in Accountancy at the rank of Assistant, Associate or Full Professor, effective June 1, 2003. A doctoral degree (completed or near completion) in Accountancy is required. Salaries are competitive and commensurate with qualifications and experience. These positions are subject to budgetary approval. The Department of Accountancy is the only department in Canada accredited by the AACSB—The International Association for Management Education. The department has 22 full-time faculty members, is one of the largest in the country, and has an established record of excellence in professional Accounting education and academic research. Please send your complete application package to Dr. Dominic Peltier-Rivest, Chairman, Department of Accountancy, John Molson School of Business, Concordia University, 1455 de Maisonneuve Blvd. West, Montreal, Quebec, H3G 1M8 CANADA; Phone: (514) 848-2778; Email: drivest@jmsb.concordia.ca. Concordia has a policy of employment equity, including affirmative action for women faculty. In accordance with Canadian immigration requirements, priority will be given to Canadian citizens and permanent residents of Canada.

415 STATE UNIVERSITY OF NEW YORK AT BUFFALO expects to have three August 2003 openings for an Assistant, Associate, or Full Professor of Accounting. Primary needs are in the Financial and Managerial areas; we are actively seeking senior applicants. Must have a demonstrated capacity to undertake scholarly research publishable in leading journals and to provide quality teaching at the undergraduate, M.B.A., and/or Ph.D. level. Doctorate in hand or (at the Assistant level) near completion from a reputable research university expected. Research record is a requirement for senior positions. Salary and fringe benefits are competitive. Send *curriculum vitae,* two letters of recommendation, copy of dissertation proposal, and/or other research paper(s) to Professor Susan Hamlen, Department of Accounting & Law, Jacobs Management Center, State University of New York at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: hamlen@buffalo.edu. SUNYAB is an Affirmative Action/Equal Opportunity Employer.

416 ARIZONA STATE UNIVERSITY WEST, School of Management, is recruiting for a tenured/tenure-track Assistant/Associate Professor of Managerial Accounting to teach Managerial Accounting at the M.B.A. and undergraduate levels beginning Fall 2003. Required: Doctorate in Accountancy or related field by August 15, 2003; evidence of teaching effectiveness in Accounting and achievement in scholarship commensurate with level of appointment; evidence of teaching Managerial Accounting. Desired: consulting/industry experience; evidence of web-based or online teaching; evidence of funded research; evidence of experience in and a desire to teach a second area of Accounting. Apply by September 30, 2002; if not filled, monthly thereafter until search closed. Send two complete copies of a letter of interest, *curriculum vitae,* names of three references, and evidence of teaching and research performance to Dr. William Duncan, ACC/ISM Department Chair, ASU West School of Management, PO Box 37100, MC 2451, Phoenix, AZ 85069-7100. ASU West is an AA/EO Employer in policy and practice.

417 ARIZONA STATE UNIVERSITY WEST, School of Management, is recruiting for a tenured/tenure-track Assistant/Associate Professor of Accounting Information Systems to teach AIS at the M.B.A. and undergraduate levels beginning Fall 2003. Required: Doctorate in Accountancy or related field by August 15, 2003; evidence of teaching effectiveness in Accounting commensurate with level of appointment, achievement in scholarship commensurate with level of appointment, teaching AIS. Desired: consulting/industry experience; evidence of: web-based or online teaching, funded research, experience in and a desire to teach a second area of Accounting, experience in database applications and management. Apply by September 30, 2002; if not filled, monthly thereafter until search closed. Send two complete copies of a letter of interest, *curriculum vitae,* names of three references, and evidence of teaching and research performance to Dr. William Duncan, ACC/ISM Chair, ASUW SOM, PO Box 37100, MC 2451, Phoenix, AZ 85069-7100. ASU West is an AA/EO Employer in policy and practice.

418 WRIGHT STATE UNIVERSITY, Raj Soin College of Business, invites applications for the position of Department Chair. The position starts September 1, 2002. January 2003 or July 2003 start dates will be considered. Candidates for the Chair position must be terminally and/or academically qualified and have an established record of teaching, scholarship, and service appropriate for appointment at the rank of Professor. In addition, candidates must possess excellent leadership skills and the ability to interact effectively with stakeholders. Earned doctorate in Accounting, relevant business experience, and Professional Accounting certification are preferred. Submit a letter of application, a *curriculum vitae,* and three references with contact information to David Bukovinsky, Chair, Accountancy Chair Search Committee, Wright State University, Raj Soin College of Business, 3640 Colonel Glenn Highway, Dayton, OH 45435; Email: david.bukovinsky@wright.edu. Reviews of applications will begin immediately and continue until the position is filled. Wright State University is an EO/AA Employer.

419 MISSISSIPPI STATE UNIVERSITY, School of Accountancy, College of Business and Industry, announces an opening for a tenure- track position teaching in the School of Accountancy beginning Fall 2003. Appointment is at the Associate Professor rank teaching undergraduate and graduate courses in Managerial Accounting, Accounting Information Systems, or Auditing. The holder of this position will be expected to serve on the School's Ph.D. Committee and teach Ph.D. classes. Applicants must have doctorate degree in Accounting. Position requires excellence in teaching, based on prior experience as Assistant or Associate Professor; ability to conduct research leading to publication in top-level journals. Applicants are expected to advise and mentor students at all levels. Professional certification and professional accounting experience are desirable. Send application letter, current *curriculum vitae,* and three reference letters to Dr. Dan Hollingsworth, Director, School of Accountancy, PO Box EF, Mississippi State University, Mississippi State, MS 39762. Review will begin September 30, 2002 and continue until filled. Mississippi State University is an AA/EO Employer.

420 UNIVERSITY OF ALASKA FAIRBANKS, School of Management, invites applications for the position of Professor/Associate of Accounting. Both the School of Management and Accounting Program are accredited by AACSB. The successful candidate is expected to offer high-quality instruction in area of expertise and to make a major contribution to Department of Accounting/Information Systems, providing leadership in teaching, research, and service for the Accounting Program. Responsibilities will include playing a major role in the further development of the program and acting as a principal catalyst to strengthen relationships between the Department and professional community. Candidates must hold a Ph.D. in Accounting or related field and provide evidence of teaching, research, and communications skills; CPA, CMA, or CIA certification preferred. Salary is competitive and commensurate with qualifications. Applications will be reviewed on August 6, 2002, and position will be open until filled. Send a signed UAF application form available from http://www.uaf.edu/uafhr; cover letter; *curriculum vitae;* three letters of reference; evidence of teaching effectiveness; and examples of scholarly activity to Dean James Collins, c/o UAF Human Resources, PO Box 757860, Fairbanks, AK 99775-7860.

421 UNIVERSITY OF MIAMI, Department of Accounting, is seeking an applicant at the Assistant Professor level for a tenure-track position beginning in 2003. All areas of research and teaching interest within the Accounting field are open for consideration and our salary levels are competitive. The individual will be expected to carry a regular teaching load at both the undergraduate and graduate levels. Additionally, the individual will be expected to conduct and publish significant research in his/her area of interest—focusing particularly on important journals in the Accounting literature. Candidates should have a Ph.D. or equivalent. Interested candidates should contact Dr. Oscar Holzmann, Chair of the Faculty Recruiting Committee, either by mail or email, at Dr. Oscar Holzmann, Department of Accounting, 317 Jenkins Building, University of Miami, Coral Gables, FL 33146; Email: holzmann@miami.edu. The University of Miami is an Equal Opportunity Employer.

422 CALIFORNIA STATE UNIVERSITY, LONG BEACH invites applicants for tenure-track Accounting faculty position at the Assistant Professor or Associate Professor level for Fall 2003. Applicants must hold a Ph.D., or D.B.A. degree with a major in Accounting and have demonstrated potential for excellence in teaching, research, curriculum development, and service contributions. Professional certification is desirable. Compensation will be competitive and be commensurate with the candidate's qualifications and experience. Applicant screening will begin November 15, 2002 and will continue until the position is filled. Please send a letter of application and *curriculum vitae* to Steven A. Fisher, Chair, Department of Accountancy, California State University, Long Beach, 1250 Bellflower Boulevard, Long Beach, CA 90840-8504. CSULB is an urban university in the California State University System. It is located three miles from the ocean in the city of Long Beach. CSULB is an Affirmative Action/Equal Opportunity Employer and has a strong commitment to the principle of diversity.

423 KANSAS STATE UNIVERSITY, Department of Accounting, seeks to fill the position of Department Head beginning Fall 2003. The Department Head is responsible for managing the AACSB-accredited undergraduate and Master's of Accountancy programs and for maintaining relationships with on- and off-campus constituencies. Applicants should have a doctoral degree in Accounting, a record of excellence in teaching and research consistent with a tenured appointment at the rank of Associate or Full Professor, strong interpersonal skills, and a commitment to improving Accounting education; experience with AACSB accreditation preferred. The Accounting Department is nationally recognized for innovation in Accounting education. Screening will begin August 1, 2002 and continue until the position is filled. Send *curriculum vitae,* list of references, and other evidence of qualifications to Dr. Stacy Kovar, Department of Accounting, Calvin 109, Kansas State University, Manhattan, KS 66506; Email: skovar@ksu.edu. For more information, please visit http://www.cba.ksu.edu/cba/employ/. Kansas State University is an Equal Opportunity Employer and seeks diversity.

424 POSITION DESIRED: Ph.D. candidate with expected graduation date in May 2003, CPA, is seeking a tenure-track or visiting position starting Fall 2003. I have taught Managerial Accounting, however, I am interested in Auditing as well as General Accounting Education. Most of my professional career experience was in the Auditing and Managerial areas. You may contact me at pdp1@cobilan.msstate.edu for additional information.

425 NAVAL POSTGRADUATE SCHOOL, Graduate School of Business and Public Policy, seeks applicants for tenure-track or visiting faculty positions. Candidates should possess a Ph.D. (or be near completion), have excellent teaching ability, and a commitment to scholarly research. Tenure-track candidates at the Associate or Full Professor rank should have an established research record. In addition to teaching M.B.A. core courses in Financial Accounting and Managerial Accounting, individuals with Finance backgrounds are needed to teach the M.B.A. core course in Finance. Other courses include Advanced Financial Reporting, Advanced Finance, Auditing, Management Control, and Strategic Cost Management. The Graduate School of Business and Public Policy offers an M.B.A., as well as other AACSB- and NASPAA-accredited Master's degree programs in management. Students include U.S. and international military officers, and employees of the federal government. Send *curriculum vitae* and names of three references to Professor Douglas A. Brook, Dean, Graduate School of Business and Public Policy, Naval Postgraduate School, 555 Dyer Road, Monterey, CA 93943. The Naval Postgraduate School is an Equal Opportunity Employer.

426 WESTERN KENTUCKY UNIVERSITY, Gordon Ford College of Business (AACSB International-accredited), is seeking applicants for a tenure-track position in Accounting beginning in August 2003. A primary specialization in Taxation is strongly preferred, with secondary specialization open. Qualifications include Ph.D. or D.B.A. in Accounting, but ABDs near completion with demonstrated potential for quality teaching and research may also be considered. Preference will be given to candidates who possess professional certification and have a sound record of teaching and research, with publications in refereed journals. Applicants should possess an international perspective. Applicants should submit a letter of application, *curriculum vitae*, transcripts, evidence of research, and three letters of reference to Chair, Accounting Search Committee, Department of Accounting, Gordon Ford College of Business, Grise Hall 501, 1 Big Red Way, Bowling Green, KY 42101. A review of applicants will begin November 1, 2002 and the position will remain open until filled. More information about the University can be obtained by contacting (270) 745-3895; Fax (270) 745-5953; or by visiting our web site at: http://www.wku.edu/gfcb.

427 McMASTER UNIVERSITY, Michael G. DeGroote School of Business (a "World's Top 100" Business School, *Financial Times* 2002 rankings), is seeking tenure-stream faculty in Accounting at the Assistant Professor level. We are looking to fill one position in July 1, 2003 and possibly one more in July 1, 2004. Applicants should have primary teaching and research interests in Financial/Managerial Accounting or Assurance/Information Systems. Preferred candidates will possess a completed doctorate or be close to completion; exhibit research excellence (or potential) and display teaching excellence (or effectiveness). Please send a *curriculum vitae* official transcripts, three letters of reference, teaching evaluations (if any), and a working paper to Professor S. M. Khalid Nainar, Chair, Accounting and Financial Management Services Area, Michael G. DeGroote School of Business, McMaster University, 1280 Main Street West, Hamilton, Ontario, L8S 4M4, CANADA. Competitive salary and benefits as well as an attractive teaching load are offered. For further information, please email nainar@mcmaster.ca or visit http://www.degroote.mcmaster.ca.

428 BOISE STATE UNIVERSITY, Department of Accountancy, invites applications for a tenure-track position at the Assistant rank starting Fall 2003, subject to budgetary approval. All research areas will be considered except for Tax. A Ph.D. in Accounting is required (ABDs will be considered). Professional experience and certification are desired. Expectations include a commitment to high-quality teaching, scholarship leading to refereed publication, and university and community service. The College and Department are AACSB-accredited, offering both undergraduate and master's degrees. Salary is competitive in the western region. Candidates should send a letter of interest, resume, and three letters of reference to Dr. Paul Bahnson, Department of Accountancy, Boise State University, 1910 University Drive, Boise, ID 83725-1610. The closing date for applications is December 1, 2002. Boise State University is an EEO/AA Employer.

429 UNIVERSITY OF ALASKA ANCHORAGE invites applicants for a tenure-track position in Accounting beginning Fall 2003. Candidates must be qualified to teach Audit, Systems, Financial, or Taxation at the undergraduate and M.B.A. levels. Minimum qualifications include an accredited Ph.D. or D.B.A. in Accounting or a J.D. or L.L.B. with an Accounting degree for a Tax position. The UAA College of Business and Public Policy is accredited by the AACSB and occupies relatively new and technologically advanced facility. The College has several research and economic development centers with a focus on international business, small business, regional economic policy and development, and Russian business education. Applicants should submit a letter of interest in the position and in relocating to Alaska (please note PCN: 301337), a University of Alaska Applicant Form, a *curriculum vitae,* transcript copies, and three references to University of Alaska Anchorage HRS, Administration 245, 3211 Providence Drive, Anchorage, AK 99508. The complete vacancy announcement and application procedures are available at http://www.finsys.uaa.alaska.edu/uaahrs. Inquiries: Professor Patrick Fort, Phone: (907) 786-4138; Email: cpfort@uaa.alaska.edu. For more information, visit http://www. cbpp.uaa.alaska.edu.

430 UNIVERSITY OF OREGON, Department of Accounting, invites applications for one or more tenure-track positions in Accounting, effective September 2003. Teaching skills in all areas of Accounting will be considered. Requirements for Assistant rank include completed or nearly completed Ph.D. Requirements for more senior ranks include demonstrated teaching competence and publication record in *TAR, JAR, JAE,* and/or *CAR.* Salary is negotiable. Application review begins October 1, 2002 for Professor and Associate Professor, and January 31, 2003 for Assistant Professor. Application review will continue until position(s) filled. To apply, send letter of interest, *curriculum vitae,* selected copies of research papers, statement of teaching interests, evidence of teaching performance, and names of three references to Professor Dale Morse, Lundquist College of Business, University of Oregon, Eugene, OR 97403-1208; Fax: (541) 346-3341. The University of Oregon is an AA/EO/ADA Institution committed to cultural diversity.

431 MONTANA STATE UNIVERSITY, College of Business, seeks applicants for an unusual and exciting administrative opportunity, the newly created position of Associate Dean for Academic Affairs. This individual will be the founding director of a Center for Excellence in Undergraduate Business Education and will lead an extremely well qualified and highly motivated faculty in a unique student-focused academic program. Salary is competitive and depends on experience. Screening begins October 15, 2002 and will continue until the position is filled. For additional information and application directions, see http://www.montana.edu/level2/jobs.html, or contact Dr. Harry Benham, Search Committee Chair, College of Business, PO Box 173040, Montana State University, Bozeman, MT 59717-3040; Phone: (406) 994-6196; Email: hbenham@montana.edu. ADA/EO/AA/Veteran's Preference.

432 SINGAPORE MANAGEMENT UNIVERSITY (SMU) invites applications for positions available in the School of Accountancy within the Standing Faculty ranks of Assistant Professor, Associate Professor, or Professor. Faculty interested in visiting appointments are encouraged to send their *curriculum vitae*. Preferred commencement is July 2003, but negotiable. Preference will be given to candidates who can teach Financial Accounting, Management Accounting, Accounting Information Systems, Tax, and Auditing. All streams of Accounting research may be accommodated. CPA is preferred. SMU offers internationally competitive remuneration, research support, reduced teaching loads for research active faculty, funding to attend international conferences, and relocation assistance and benefits to help international faculty take up their appointments in Singapore. Information about SMU and application procedures are outlined at http://www.smu.edu.sg. *Curriculum vitae* can be forwarded to accountingcv@smu.edu.sg or sent to Office of Faculty Administration, Oei Tiong Ham Building, 469 Bukit Timah Road, Singapore 259756.

433 SAN JOSE STATE UNIVERSITY invites applications for Fall 2003 for a tenure-track Accounting position as Associate or Assistant Professor, or for a temporary appointment as Visiting Professor as Lecturer with an emphasis in Financial, Managerial Accounting, and Taxation. The candidate must have demonstrated versatility in subject matter competencies and pedagogy. The teaching assignment is at both graduate and undergraduate levels including traditional and accelerated formats. The positions are subject to final approval by the University. The minimum credential requirement is Ph.D. for tenure-track appointments, and an M.B.A. for temporary appointments. Strong preference will be given to candidates who hold a Ph.D. from an AACSB-accredited university. The application deadline is December 31, 2002 or until position is filled. Send application letter, resume, and three letters of reference to Dr. Joseph Mori, College of Business, San Jose State University, One Washington Square, BT850, San Jose, CA 95192-0066; Phone: (408) 924-3460; Email: mori_j@cob.sjsu.edu. San Jose State University is an Equal Opportunity/Affirmative Action Employer.

434 TEXAS CHRISTIAN UNIVERSITY, Department of Accounting, invites applications for Chair of the Department of Accounting to begin in August 2003. We seek candidates with demonstrated excellence in research and teaching, and a cumulative record that would warrant a faculty appointment at the Associate Professor or, preferably, Full Professor rank. Administrative experience is desirable but not required. The Department Chair will provide academic leadership, engage in outreach activities in the DFW area, and have administrative responsibility for the department. TCU offers an excellent environment for faculty—competitive salary and benefits, good research support including full access to the Wharton Research Data Service, and attractive teaching loads. Applications should be addressed to Steve C. Lim, Recruiting Chair, TCU Box 298530, M. J. Neeley School of Business, Texas Christian University, Fort Worth, TX 76129. Professor Lim may contacted by phone: (817) 257-7536, or by email: s.lim@tcu.edu. Fort Worth is an attractive and affordable place to live in the heart of the vibrant DFW Metroplex. Please visit http://www.tcu.edu for further information on TCU and http://www.neeley.tcu.edu for information on the Neeley School. TCU is an EEO/AA Employer. Applications by women and minority candidates are especially encouraged.

435 TEXAS CHRISTIAN UNIVERSITY, Department of Accounting, invites applications for a tenure-track faculty position as an Assistant Professor of Accounting to begin in August 2003. All ranks and all areas will be considered. A Ph.D. is required and professional certification and teaching experience are a plus. Candidates with demonstrated excellence in research and teaching are preferred. TCU offers an excellent environment for new faculty—competitive salary and benefits, good research support and colleagues, and attractive teaching loads. For example, TCU has access to Compustat and CRSP through Wharton Research Data Services, First Call I/B/E/S data, and other global financial transactions through SDC Platinum from Thompson Financial. Applications should be addressed to Steve C. Lim, Recruiting Chair, TCU Box 298530, M. J. Neeley School of Business, Texas Christian University, Fort Worth, TX 76129. Professor Lim may contacted by phone: (817) 257-7536 or by email: s.lim@tcu.edu. Fort Worth is an attractive and affordable place in which to live in the heart of the vibrant DFW Metroplex. Please visit http://www.tcu.edu for further information on TCU and http://www.neeley.tcu.edu for information on the Neeley School. TCU is an EEO/AA Employer. Applications by women and minority candidates are especially encouraged.

436 DREXEL UNIVERSITY, LeBow College of Business, Department of Accounting and Taxation, invites applications for the position of Department Head. Qualified candidates should possess a record of scholarship commensurate with appointment as a tenured Professor of Accounting. Candidates should have strong leadership, interpersonal, and administrative skills, and a demonstrated record of excellent teaching and course development. The Department of Accounting and Taxation has 13 full-time faculty members, including two chaired professorships, and is in the process of filling a third chaired professorship. Located in Philadelphia's University City area, Drexel's LeBow College of Business is AACSB-accredited at the graduate and undergraduate levels and offers doctorate, M.B.A., Executive M.B.A., M.S. in Accounting, M.S. in Taxation, and undergraduate degrees. The LeBow College focuses on research and the Department of Accounting and Taxation has a highly productive faculty. Send application materials to Michael Gombola, Department of Accounting and Taxation, LeBow College of Business, Drexel University, 3141 Chestnut Street, Philadelphia, PA 19104. Drexel University is an Affirmative Action, Equal Opportunity Employer.

437 DREXEL UNIVERSITY, LeBow College of Business, Department of Accounting and Taxation, invites applications for the KPMG Professorship in Accounting. Qualified candidates should be recognized scholars and be prepared to lead the research efforts of the Department of Accounting and Taxation. Candidates should have strong interest in supervising doctoral students and teaching at the doctoral level. The chairholder will be expected to foster relationships between the Department and leaders in the Accounting Profession. Located in Philadelphia's University City area, Drexel's LeBow College of Business is AACSB-accredited at the graduate and undergraduate levels and offers doctorate, M.B.A., Executive M.B.A., and undergraduate degrees. The LeBow College focuses on research and the Department of Accounting and Taxation has a highly productive faculty. Salary, benefits, and support are competitive. Send application materials to Michael Gombola, Department of Accounting and Taxation, LeBow College of Business, Drexel University, 3141 Chestnut Street, Philadelphia, PA 19104. Drexel University is an Affirmative Action, Equal Opportunity Employer.

438 DREXEL UNIVERSITY, LeBow College of Business, Department of Accounting and Taxation, has an opening for a tenure-track Assistant Professor or Associate Professor position beginning in September 2003. Qualified candidates should have earned their Ph.D. and have an interest in innovative research. Teaching interests should include Managerial Accounting and Financial Accounting. Located in Philadelphia's University City area, Drexel's LeBow College of Business is AACSB-accredited at the graduate and undergraduate levels and offers doctorate, M.B.A., Executive M.B.A., and undergraduate degrees. The LeBow College focuses on research and the Department of Accounting and Taxation has a highly productive faculty. Salary, benefits, and support are competitive. Send application materials to Kevin Brown, Recruiting Chair, Department of Accounting and Taxation, LeBow College of Business, Drexel University, 3141 Chestnut Street, Philadelphia, PA 19104. Drexel University is an Affirmative Action, Equal Opportunity Employer.

439 UNIVERSITY OF GEORGIA, J.M. Tull School of Accounting, is seeking applications for an anticipated tenure-track position in Accounting at the Assistant Professor level with teaching responsibilities in Taxation or Auditing/Systems at both the graduate and undergraduate levels. The anticipated date of appointment is Fall 2003. Candidates for this position should have either a Ph.D. in Accounting or be well along in the dissertation stage. Applicants must have a commitment to excellence in teaching and scholarship. Salary and benefits are competitive. Applications received before December 31, 2002 are assured of consideration. Please submit application materials (current *curriculum vitae* and dissertation proposal or example of scholarly work) to Professor Ben Ayers, Chair, Faculty Recruiting Committee, J.M. Tull School of Accounting, Terry College of Business, 255 Brooks Hall, University of Georgia, Athens, GA 30602. The University of Georgia is an Equal Opportunity/Affirmative Action Institution and strongly encourages applications from women and minorities.

440 THE UNIVERSITY OF TEXAS AT EL PASO invites applications for the position of Assistant /Associate Professor of Accounting with specialization in Accounting Information Systems. The College of Business and Administration and the Accounting Department are AACSB-accredited. The successful applicant will teach graduate and undergraduate Accounting and CIS courses, perform research, and perform academic and professional service within the context of the University's U.S.-Mexico border mission. Applicants for Associate Professor must have a record of accomplishment in these areas. Accounting certification (e.g., CPA, CMA) or demonstrated intent to achieve certification is highly desirable. Applications will be processed as received. Position will remain open until filled. Send letter of application, *curriculum vitae*, and names of five references to Dr. Gary J. Mann, Chair, Department of Accounting, UTEP, 500 West University Ave., El Paso, TX 79968. For more information, visit http://www.utep.edu. This position is contingent on availability of funding. The University of Texas at El Paso is an Equal Opportunity/ Affirmative Action Employer. The University does not discriminate on the basis of race, color, national origin, sex, religion, age, disability, or sexual orientation in employment or the provision of services.

441 UNIVERSITY OF REDLANDS, School of Business, is searching for an applicant to fill a full-time faculty position in Accounting for the 2003–2004 academic year. This position will be either tenure-track or clinical appointment. Tenure-track position requires an earned doctorate in business discipline or closely related field. Clinical position requires an M.B.A. or other business-related Master's degree. Applicants should have a record of achievement in their functional area and vibrant interest in cross-disciplinary undergraduate and graduate teaching; also the desire to work with other faculty and with practitioners to create and implement innovative curricula and learning approaches. Salary is competitive. Applicants should send a cover letter, resume/*curriculum vitae*, copies of transcripts (unofficial copies will suffice), and the names and contact information for five references to Interdisciplinary Faculty Screening Committee, c/o Liz Woods, School of Business, University of Redlands, 1200 East Colton Avenue, Redlands, CA 92373-0999. Review of applications will begin on November 1, 2002. Position will be open until filled. EOE.

442 UNIVERSITY OF MASSACHUSETTS BOSTON invites applications for tenure-track positions at the Assistant level beginning Fall 2003. Special consideration will be given to candidates with a primary interest in Financial and/or Tax. A Ph.D. in Accounting is required, but ABDs nearing completion will be considered. Send *curriculum vitae* to Arindam Bandopadhyaya, Chair, Department of Accounting and Finance, College of Management, University of Massachusetts Boston, 100 Morrissey Boulevard, Boston, MA 02125-3393. UMass. Boston is an Equal Opportunity/ Affirmative Action Employer. The positions are subject to budgetary approval.

443 UNIVERSITY OF TENNESSEE, KNOXVILLE is accepting applications for one full-time, tenure-track position (Assistant or Associate Professor) to begin August 1, 2003. A second position might be available if funding permits. Applicants should have a primary interest in Financial Accounting, Auditing, or Information Systems with an interest in Corporate Governance preferred. A candidate for Assistant Professor should hold a doctoral degree in Accounting or expect to complete all degree requirements by Fall 2003. The candidate should demonstrate excellent teaching and research potential. A candidate for Associate Professor should hold a doctoral degree in Accounting and should demonstrate excellent achievement in teaching and research. Applications from minorities and women are strongly encouraged. Review of applications will continue until position is filled. Send applications to Dr. Keith G. Stanga, Head, Department of Accounting and Business Law, CBA, University of Tennessee, Knoxville, Tennessee 37996-0560. UT Knoxville is an EEO/AA/Title VI/Title IX/Section 504/ADA/ADEA Institution in the provision of its education and employment programs and services.

444 ANGELO STATE UNIVERSITY invites applications for a tenure-track position at the Assistant or Associate Professor level beginning in the Fall of 2003. Salary is competitive and summer school or a research grant is usually available at one-sixth of the nine-month salary. Doctorate in Accounting is preferred. Applicants with doctorates in non-Accounting fields must have a minimum of 18 hours of graduate work in Accounting. The teaching load will be 12 hours in the fall and 9 hours in the spring, and will include a graduate Financial Theory course, a graduate course in Financial Statement Analysis, and Principles of Accounting. Applications should consist of a (1) a cover letter, (2) a current resume, (3) photocopies of undergraduate and graduate transcripts, (4) three letters of reference, and (5) the ASU Application for Faculty Employment, which is available at http://www.angelo.edu/forms/pdf/appfacapptweb.pdf. Applications will be considered until the position is filled. Contact Dr. Norman Sunderman, Head, Department of Accounting, Economics, and Finance, Angelo State University, San Angelo, TX. Angelo State University is an Equal Opportunity/Affirmative Action Employer.

445 WASHINGTON UNIVERSITY, Olin School of Business, is seeking faculty in Accounting at all levels starting Fall 2003. The qualifications include a demonstrated interest in and capacity to do research publishable in leading Accounting journals and a high level of teaching competence in graduate and undergraduate classes. Candidates should hold a doctorate degree or be very near completion. Salary and faculty support are highly competitive. Each application should include a *curriculum vitae* and a copy of the dissertation proposal (or the completed dissertation); more established scholars should include several samples of their scholarship. Send all materials to Dean Stuart I. Greenbaum, John M. Olin School of Business, Campus Box 1133, Washington University, 1 Brookings Drive, St. Louis, MO 63130-4899. Washington University is an Equal Opportunity/Affirmative Action Employer and welcomes applications from women and minorities.

446 UNIVERSITY OF PENNSYLVANIA invites applications from qualified persons for positions available July 2003 in Financial Accounting at all professional ranks. A Ph.D. is required. Demonstrated ability or potential for effective teaching and quality research are important considerations. Interested persons should send their resume including educational background, professional experience, publications, personal data, and, if applicable, a dissertation proposal and abstract to Professor Stanley Baiman, Accounting Department, University of Pennsylvania, Suite 2400, Steinberg-Dietrich Hall, Philadelphia, PA 19104-6365. The deadline for applications is January 15, 2003. University of Pennsylvania is an Equal Opportunity/Affirmative Action Employer.

447 THE UNIVERSITY OF MEMPHIS, School of Accountancy, invites nominations and applications for the Arthur Andersen Chair of Excellence in Accountancy. In addition to an earned doctorate with an emphasis in Accounting, candidates should have outstanding qualifications in teaching, research, and service. This position is supported by endowed funds. Screening of candidates will begin September 30, 2002, and continue until the position is filled. Please contact Dr. Ken Lambert, Director, School of Accountancy, The University of Memphis, Memphis, TN 38152; Phone: (901) 678-4569; Email: klambert@memphis.edu. The University of Memphis, a Tennessee Board of Regents Institution, is an Equal Opportunity/Affirmative Action Employer. All qualified applicants are urged to apply for this position. Appointment will be based on qualifications as they relate to the position requirements without regard to race, color, national origin, religion, sex, age, disability, or veteran status.

448 THE UNIVERSITY OF MEMPHIS, School of Accountancy, invites nominations and applications for the position of Assistant/Associate Professor of Accountancy starting Fall 2003. Qualifications include an earned doctorate in Accounting, and a commitment to excellence in teaching, research, and service. We are particularly interested in candidates who have teaching and research interests in Managerial Accounting. Screening of applicants will begin September 30, 2002 and continue until the position is filled. Please contact Dr. Ken Lambert, Director, School of Accountancy, The University of Memphis, Memphis, TN 38152. Phone: (901) 678-4569; Email: klambert@memphis.edu. The University of Memphis, a Tennessee Board of Regents Institution, is an Equal Opportunity/Affirmative Action Employer. All qualified applicants are urged to apply for this position. Appointment will be based on qualifications as they relate to the position requirements without regard to race, color, national origin, religion, sex, age, disability, or veteran status.

449 BARUCH COLLEGE OF CUNY, Zicklin School, Stan Ross Department of Accountancy, invites applications for tenure-track positions at the rank of Assistant or Associate Professor. A doctoral degree in Accounting is required for appointment. Candidates who expect to complete their Ph.D. by September 1, 2003 are encouraged to apply (but any appointments revert to instructorships if the Ph.D. is not completed by that date). Salary and rank are dependent on qualifications. Applicants for Associate Professorships must demonstrate teaching effectiveness and a desire to work with doctoral students. Please send resumes by February 14, 2003 to Professor Bharat Sarath, Department of Accountancy, Baruch College, 1 Bernard Baruch Way, Box B12-225, New York, NY 10010. Baruch College of CUNY is an AA/EO/IRCA/ADA Employer.

450 QUINNIPIAC UNIVERSITY invites applications for a full-time, tenure-track position. While the primary teaching responsibilities will be in Auditing, secondary teaching responsibilities may be in graduate AIS. The candidate should demonstrate high potential for quality teaching and strong interest in maintaining a research agenda commensurate with AACSB standards. Rank is commensurate with qualifications. Candidates must hold a Ph.D. in Accounting, but will consider ABD. Send letters of interest, *curriculum vitae,* and names and addresses of three (3) references to Professor Julia K. Brazelton, Chair, Department of Accounting, School of Business, Quinnipiac University, 275 Mt. Carmel Ave., Hamden, CT 06518; Email: julia.brazelton@quinnipiac.edu. Preliminary interviews will be held at the AAA Annual Meeting in San Antonio. Quinnipiac University has a strong commitment to the principles and practices of diversity throughout the university community. Women, members of minority groups, and individuals with disabilities are encouraged to consider this opportunity and to apply.

451 MASSACHUSETTS INSTITUTE OF TECHNOLOGY, Sloan School of Management's Accounting Group, seeks new faculty at all levels (Assistant, Associate, and Full Professor) for the academic year beginning July 1, 2003, or as soon as possible thereafter. Applicants will be evaluated on their potential for and/or record of excellence in research and teaching. For Assistant Professor positions we give priority to applicants who submit completed research. Information about the Alfred P. Sloan School of Management can be found at: http://mitsloan.mit.edu/. Applicants should submit a brief cover letter, up-to-date *curriculum vitae,* three letters of reference, and evidence of research such as a publication, working paper, or dissertation proposal to Professor S. P. Kothari, Chair, Accounting Search Committee, MIT Sloan School of Management, 50 Memorial Dr., E52-325B, Cambridge, MA 02142-1347. The deadline for applications is January 15, 2003. However, applications will be evaluated as soon as they are complete, if submitted at an earlier date. MIT is an Equal Opportunity Employer committed to building a culturally diverse and pluralistic intellectual community, and strongly encourages applications from women, minorities, and other historically underrepresented groups.

452 UNIVERSITY OF CALIFORNIA, DAVIS, Graduate School of Management, invites applications for up to two tenure-tenure track Professors at any rank in Accounting. Applications should demonstrate excellence or potential excellence in research and teaching Financial or Managerial Accounting. Candidates should have a Ph.D. degree in Accounting or related field. Your letter of application should describe your research and teaching interest, and include a resume, evidence of research (papers, dissertation) and teaching, and the names and addresses of three references. Send to Associate Dean, Paul A. Griffin, University of California, Davis, Graduate School of Management, One Shields Avenue, Davis, CA 95616; Email: FacultyRecruit@gsm.ucdavis.edu. These positions have no closing date and will remain open until filled. However, the Committee expects that an initial consideration of candidates' applications will be completed no later than October 31, 2002. The University of California is an Affirmative Action/Equal Opportunity Employer.

453 DARTMOUTH COLLEGE, Tuck School of Business, is conducting a faculty search in Financial Accounting. We are open to all levels of appointment, with a preference for those who have at least three years experience since completing their Ph.D. We are interested in candidates who will produce research of excellent quality that will have high impact on the field. The candidate should be able to teach Tuck's "core" Financial Accounting course as well as offer second-year electives. We look for excellence in both research and teaching and provide ample financial and logistical support in a highly collegial culture. Tuck provides faculty with state-of-the-art computer technology and programming support. Library and secretarial support is generous, and funding is provided for research assistance and travel. Send resumes to Professor Richard Sansing, Tuck School of Business at Dartmouth, 100 Tuck Hall, Hanover, NH 03755. Dartmouth College is an Equal Opportunity, Affirmative Action Employer.

454 VIRGINIA TECH, Pamplin College of Business, Department of Accounting and Information Systems, invites applications for an Assistant Professor, tenure-track position starting August 10, 2003. Applicant's primary research and teaching interests should be in Cost/Managerial Accounting. Successful candidate will instruct undergraduate and graduate students in Cost/Managerial Accounting. Qualifications: Doctoral requirements completed and demonstrable prospects for conducting scholarly research and excellence in teaching. Applications accepted until October 15, 2002, or until position is filled. Send applications to Wayne Leininger, Department of Accounting and Information Systems, 3007 Pamplin (0101), Virginia Tech, Blacksburg, VA 24061. Virginia Tech has a strong commitment to the principle of diversity and, in that spirit, seeks a broad spectrum of candidates including women, minorities, and people with disabilities. Individuals with disabilities desiring accommodations in the application process should notify Wayne Leininger, (540) 231-6591 or TTY (540) 231-9460, by the application deadline.

455 AMERICAN UNIVERSITY, Kogod School of Business, invites applications for a tenure-track appointment in Accounting at the Assistant Professor level beginning Fall 2003, subject to final budgetary approval. Applicants should have a Ph.D. in Accounting or anticipate completion by August 31, 2003. Teaching and research interests in Financial Accounting are preferred, as is an established record of teaching and research. Experience in mentoring women and minority graduate students is desired. Responsibilities include undergraduate and master's level teaching, pursuit of scholarly research goals, participation in department, school, and university activities, and student advising. Salary and benefits are competitive. Send applications (including *curriculum vitae,* three letters of recommendation, teaching evaluations, and evidence of scholarly research activity) to Dr. Ajay Adhikari, Kogod School of Business, American University, 4400 Massachusetts Ave. NW, Washington, D.C. 20016-8044; Telephone: (202) 885-1993; Email: aadhika@american.edu. American University, an Equal Opportunity, Affirmative Action Employer, strongly encourages applications from women and minorities.

456 CLARION UNIVERSITY, College of Business Administration, Department of Accountancy, invites applications for one or more tenure-track positions beginning August 2003. A doctorate in Accounting is required, but consideration may be given to advanced ABD candidates. Primary teaching responsibilities will be in Managerial/Cost, Systems, Advanced and/or International Accounting. Research and publication, completion of a successful interview, and excellent teaching and service are required. Clarion University is fully accredited by AACSB International. Review of applications will begin October 15, 2002. Send *curriculum vitae* to Dr. Thomas W. Oliver, Chair, Department of Accountancy, College of Business Administration, Clarion University, Clarion, PA 16214; Email: toliver@clarion.edu. Clarion University is a diverse academic community and encourages people of color, women, veterans, and persons with disabilities to apply.

457 SIEMENS ENERGY & AUTOMATION, INC. invites applications for the position of Senior Accountant. Requirements include B.A./B.S. or equivalent in Accounting; CPA or equivalent; four years of experience in job offered or related occupation performing duties outlined below, including two years of audit experience. Position's responsibilities are completion of reports in SAP financial reporting systems; DBS General Ledger; Mexican fiscal law; audit of Mexico operations (fluency in Spanish necessary); completion of all closing/accounting processes, including consolidations/eliminations and federal tax provision; two years' developing and writing technical accounting policies; conducting investigations as assigned; performing audit operations in U.S. and Mexico; support of investigations, formulating conclusions, providing recommendations. Must have proof of legal authority to work in U.S. Salary is $55K per year. Send resume and minimum requirements to Siemens Energy & Automation, Inc., Attention: Carol Hepler, 3333 Old Milton Parkway, Alpharetta, GA 30005.

458 FAIRFIELD UNIVERSITY, Dolan School of Business, invites applications for a tenure-track position as Assistant Professor of Accounting/Taxation beginning Fall 2003. The successful candidate must be a CPA and have a Ph.D./DBA in Accounting, or a J.D./LL.M. with a specialization in Taxation. Candidates must have a strong commitment to teaching and research. Candidates in the final stage of their graduate studies will also be considered. Salary is competitive. Fairfield University, a Jesuit University located about an hour from New York City, offers graduate and undergraduate programs in Arts and Sciences, Business, Education and Allied Professions, Engineering, and Nursing. The Dolan School of Business is accredited by AACSB International. Send a letter of application, *curriculum vitae,* transcripts, and information for three references to Dr. Roselie McDevitt, Chair, Accounting Department, Dolan School of Business, Fairfield University, Fairfield, CT 06824; Phone: (203) 254-4000, extension 2829; Email: rmcdevitt@mail.fairfield.edu. Fairfield University is an Affirmative Action/Equal Opportunity Employer. Women and minorities are encouraged to apply.

459 THE CLOROX COMPANY, Oakland, CA, invites applications for the position of Assistant Manager, Logistics. Responsibilities include the following: (1) Establish budget and monitor performance against transportation and warehousing budgets for all U.S. Logistics for employer; (2) Serve as key interface between Logistics Operations and domestic business units it supports; (3) Ensure business initiatives have been factored into Logistics budget and that business units are current on operations issues; (4) Manage four professional staff; (5) Recruit, train, and develop two analysts and two senior analysts; (5) Act as advisor and financial consultant to senior managers of Logistics and Vice President of Logistics; (6) Manage sharing and transferring of Logistics' best practices to Latin American division. Will use Hyperion Essbase, Mainframe Focus query language, and freight payment systems. Requirements for position include a Bachelor's degree in Economics, Finance, or related field; two years of experience in job offered; two years of experience in related position within Financial Management. Send resumes to Sonrisa Choy, The Clorox Company, 1221 Broadway, Oakland, CA 94612-1888.

460 UNIVERSITY OF ILLINOIS, Department of Accountancy, is recruiting faculty for full-time appointments at all ranks. Candidates at advanced ranks must possess a doctoral degree, and candidates for Assistant Professor positions must possess or be near completion of a doctoral degree. Applicants for senior positions should have a strong research record and the ability to participate in doctoral education and curriculum development at the undergraduate and graduate levels. In addition, nontenure-track Lecturer and Visiting positions may become available. Applicants should submit a resume that lists research publications and courses taught. Compensation is competitive and will be commensurate with the appointment level and applicant's record. To ensure full consideration, applications should be received by November 1, 2002. Applications received thereafter will receive full consideration if there are positions still available. Proposed starting date is August 21, 2003. Correspondence and resumes should be sent to Professor Mark Peecher, Recruiting Chair, Department of Accountancy, 343B Wohlers Hall, College of Commerce and Business Administration, University of Illinois, 1206 S. Sixth Street, Champaign, IL 61820. The University of Illinois is an Affirmative Action, Equal Opportunity Employer.

461 UNIVERSITY OF CALGARY, Haskayne School of Business, is seeking qualified candidates with a Ph.D., or nearly completed doctorate, to fill an Assistant level tenure-track position in the Accounting area. The successful candidate will be expected to take an active role in research, as well as teach at both the undergraduate and graduate levels. Preference will be given to candidates in Financial Accounting and Auditing. Anticipated starting date is July 1, 2003. Please submit a *curriculum vitae* and three letters of reference by December 15, 2002 to Duncan Green, Chair, Accounting Selection Committee, Haskayne School of Business, University of Calgary, SH 322, 2500 University Drive NW, Calgary, Alberta, CANADA; Email: duncan.green@haskayne.ucalgary.ca.

462 STANFORD UNIVERSITY, Graduate School of Business, invites applications for faculty positions in Accounting effective September 2003. We seek tenure-level and nontenured candidates and invite applications from candidates at all applicable stages of their careers who are interested in Financial Accounting, Managerial Accounting, Tax, and other areas that will add to our research and teaching portfolio. Candidates must have a Ph.D. and a high level of accomplishment or promise in both scholarly research and graduate-level (M.B.A. and Ph.D.) teaching. Applications should include *curriculum vitae* and copies of research publications. Applicants are strongly encouraged to submit their applications electronically by visiting the web site: http://www.gsb.stanford.edu/recruiting/ and uploading their *curriculum vitae*, research papers and publications, and teaching evaluations (if applicable) on that site. Alternatively, applicants may email electronic materials to Faculty_Recruiter@GSB.Stanford.Edu or postal mail a diskette, CD, or zip disk to Faculty Recruiting Manager, Box A, Graduate School of Business, Stanford University, Stanford, CA 94305-5015. The application deadline for receipt of materials is January 15, 2003. Stanford University is an Equal Opportunity, Affirmative Action Employer.